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Studies X

**Edition Open Access
2015**

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Giuseppe Castagnetti and Jürgen Renn (eds.)

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Max Planck Research Library for the History and Development of Knowledge
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Communicated by:
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Image Processing:
Digitization group of the Max Planck Institute for the History of Science

Cover image: XXX. Photo by XXX.

ISBN XXXXX

First published 2015 by Edition Open Access,
Max Planck Institute for the History of Science
<http://www.edition-open-access.de>

Printed in Germany by epubli, Oranienstraße 183, 10999 Berlin
<http://www.epubli.de>

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The Deutsche Nationalbibliothek lists this publication in the Deutsche Nationalbibliografie;
detailed bibliographic data are available in the Internet at <http://dnb.d-nb.de>.

Chapter 5

Establishing the Quantum in Göttingen: David Hilbert, Peter Debye and Max Born in Context (1900–1926)

Arne Schirrmacher

5.1 Introduction

Göttingen is without doubt one of the central places where quantum physics developed. Here quantum mechanics was established for the first time in the collaboration of Max Born with Werner Heisenberg and Pascual Jordan, two of his former students who had grown up to become real partners in science. Collecting the specific contributions to quantum theory that came from Göttingen in the two decades before the quantum mechanical revolution, however, does not combine easily into a satisfactory narrative.

Relevant publications actually emerged as isolated and mostly unrecognized works. The first were by Walter Ritz on his combination principle and by Max Abraham on black-body radiation (Ritz 1903; 1908; Abraham 1904).¹ Paul Ehrenfest, too, was quite interested in the quantum hypothesis early on, for example, he was instrumental in recognizing the inevitable discontinuity it entailed, and he also spent much time in Göttingen. However, he was not able to gain a position or influence here (M. J. Klein 1970). More prominent work had been done by Walter Nernst and Johannes Stark. Nernst developed his heat theorem while already preparing to leave for Berlin. And Stark made his discovery only after he left Göttingen, having taken with him the general idea for the Stark effect from Woldemar Voigt (Stark 1987 [1945], 22).

Born turned to Thomson's atom for his habilitation lecture and attended Albert Einstein's Salzburg talk in 1909, thus developing an interest in atomic physics and the quantum question. He contributed to the emergent research field through work with Rudolf Ladenburg on black-body radiation, with Theodore von Kármán in their papers on specific heat (at the same time as Peter Debye), and with Richard Courant relating quantum theory to the law of Eötvös, as well as in a number of single-authored papers he wrote from 1911 on. Both Born and Debye focused during the war on the question of what results would come from Bohr's atom if taken seriously. Born, together with Alfred Landé and Erwin Madelung at a Berlin outpost of Göttingen physicists on war duty, used it for the constitution of matter, Debye and Paul Scherrer applied it to the diffraction of X-rays. Debye also brought in the quantum for the Zeeman effect (in parallel with Arnold Sommerfeld). At this time the quantum, as the key to the atomic structure of matter, became the Göttingen credo. After the war—when the failures of the Bohr atom became apparent—it was the quantum that opened up the fruitful road to matrix mechanics paved by Born and James Franck.

¹ As Abraham did his doctorate with Planck and was his assistant from winter term 1897/98 to winter term 1900/01, he hence witnessed Planck's work culminating in his law, while Planck in turn recognized his work, cf. (Planck 1906, 68).

In this way, a brief sketch of the contributions to quantum theory from Göttingen can be given; however, it hardly indicates a conclusive conceptual development, nor does it refer to the conditions and driving forces behind it. One person who will be considered central in this study, the mathematician David Hilbert, might not appear at all, as his publications on quantum or atomic problems are none and citations of his influence few. Moreover, such a perspective on the Göttingen story of quantum theory throws into question what the result of a local perspective should be. It might just confirm a view that the only sensible way to approach the historical development of quantum theory is a conceptual one that focuses on idealized discussions in journals, instead of real-life discourses, possibly enriched by some celebrated conferences and meetings. Everything else appears to spring from the ingenuity of the individual researchers. But as the only context in this approach is purely cognitive, probably consisting of personal ensembles of knowledge and world views, such a study would lose sight of the inevitably wider context of the creation of knowledge referring to particular local circumstances.

5.1.1 Local Contexts: Resources and Research Politics

To avoid this outcome, I suggest exploiting a number of indicators for scientific change in order to reconstruct the local situation in which research fields are created and altered. Understanding scientific activity as investments of resources, while de-emphasizing “pure genius” and the inherent logical structure of scientific theory, a local story of the establishment of quantum theory in Göttingen becomes feasible. I will call this scientific entrepreneurship *research politics*. It comprises decisions to direct or redirect resources in a way that favors or encumbers research in a certain field, irrespective of whether these resources are financial, personal, directed toward public perception, or even relevant for exploitable technology. In this sense, seeking for research politics is at the same time a kind of an economic or resource-oriented history of science, for it asks what resources were invested to what anticipated end. These expectations and hopes, however, often do not correspond with the successes the investments yield, a fact hard to handle from a viewpoint focused on a conceptual development. In short, I will argue for the thesis that a sound historical account of the development of quantum physics cannot properly be given without dwelling on these kinds of research politics.² In consequence this study will depart to some extent from other treatments of this period, both in perspective and sources.³

Why rather provincial Göttingen became a place for eager advocates of quantum physics at all, and how it happened that a specific constellation was created that enabled quantum mechanics to be established here, is far from obvious. To answer this question I go back to the local context that is out of the center of a strictly conceptual description of the historical development of quantum physics. I consider the present resources, i.e.

²The use of the term *research politics*, instead of *policy*, reflects the contextual point of view taken in this study in order to elucidate a development that does not exhibit a linear story. While any policy is related to a plan, a philosophy, to principles, guidelines etc, *politics* emphasizes the probably less coherent actual doings, the concrete order of steps taken, the process of trial and error etc., hence a development that may in cases lead to rather unplanned results.

³Part two on “The Emergence of the Quantum Discontinuity, 1905–1912” by (Kuhn 1978a) is a particularly important starting point for this study. Although the focus is on concept development and propagation, aspects like the quantitative analysis of publications suggest developing further indicators of interest, and investing time and funds, which can be localized. Cf. for the period under investigation also (Hermann 1971; Hund 1967; 1987; Mehra and Rechenberg 1982a).

personnel, experimental facilities, but also the accumulated knowledge—the cognitive resources—which might have made Göttingen a favorable place for quantum theory. In addition I look for an organizer or a pressure group that was driven by more or less clear objectives. This serves to determine what factors may have turned Göttingen into a particularly favorable place with regard to the emerging competing centers of quantum and atomic physics. Furthermore, disciplinary and institutional constraints that may have influenced the development (or that had to be overcome) are analyzed as well as the question to what extent a particular science policy by the state encouraged the reorientation.

A closer look will be taken at how the individual researchers reacted to the news about quanta and how they adjusted their interests, both in research and teaching, towards quantum problems, and how they accepted or rejected state support and decisions, in particular with regard to filling positions. The process between the reception of the theory and participation in work on quantum problems, while fulfilling academic duties to reorganizing the curriculum, appears most important. Three key figures of the Göttingen development will be discussed in some detail: Hilbert, Debye and Born. They will be contrasted with the older principal Göttingen physicists Voigt and Eduard Riecke, whom they complemented and then replaced, as well as with other colleagues.

The foremost aim of this paper is to describe the various fields of research politics that can be inferred from the Göttingen story of quantum physics. It will then, however, be a second step to associate this approach with a certain conceptual point of view, which brings to the fore cognitive preconditions that underlie decisions on research politics. In our case different ways of organizing knowledge can be seen as the root of different levels of confidence in atomic reality and reductive thinking, and hence had implications for the emergence of quantum physics. Here, the organization of knowledge refers to the ways of arranging phenomena and their description according to categories that depend on a broader scope of scientific, personal, and cultural determinants. They sometimes amount to an elaborate vision.⁴

5.1.2 Setting the Stage: Voigt and Hilbert

The separation of physical and mathematical research programs is part of the evolution of sciences, and in particular of the disciplinary differentiation often seen as an indicator of scientific maturity. For these two branches of knowledge, however, a complete separation does not appear feasible. Only the defining research questions can be disentangled as, roughly speaking, mathematicians seek general sets of logical schemes, while physicists are looking for the one and only completely satisfactory theory that applies to nature. If Göttingen stands for an exceptional collaboration between mathematics and physics *after* their disciplinary emancipation, this must be regarded more as an effect of higher order than as a retrograde step.

⁴A further thesis to be argued for elsewhere is hence that Hilbert, on the basis of reductive thinking (axiomatics, atomism), formed the vision of an *Einheitswissenschaft* (unified science)—a conceptual scheme valid for all science—that played a crucial role in Göttingen becoming a center for quantum physics. Cp. (Corry 2004; McGuinness 1987).

The disciplinary situation of physics in Göttingen, as described in the seminal study by Jungnickel and McCormmach, which is taken here as a starting point, was rather special even back in 1883, since

[...] physics was counted as mathematics [...] and the physicists Weber, Riecke, and earlier Listing, belonged to the mathematical, not the physical, class of the Göttingen Royal Society of Sciences. The practical advantage was that at Göttingen Voigt could find over 130 well-prepared, hard-working students working in mathematics.⁵

The mathematical physics institute was intended for “advanced physics through exact measurements in the laboratory” (Jungnickel and McCormmach 1986, 113). The work was divided between the two full professors:

Riecke would retain control of the beginners’ exercises and the examinations for students of medicine, pharmacy, and agriculture, but he would divide with Voigt the advanced exercises and the examinations for secondary teachers and doctoral candidates. (Jungnickel and McCormmach 1986, 115)

Voigt arranged laboratory practice courses for the more mathematically trained students and paid a mathematics student to assist him (exhibiting a kind of reversal of roles between mathematics and physics that we will encounter later in various ways). He used his personally owned instruments for his research and there was no doubt that his “optical work belonged to an active research area in Germany [...]” (Jungnickel and McCormmach 1986, 116).

Voigt, educated in Königsberg, had finished his thesis under Franz Neumann in 1874 and succeeded him as an associate professor in the following year. He eventually was granted a full professorship in Göttingen in 1883 where he remained all his life.⁶ For Voigt precise measurements were the primary task in his mathematical physics institute and theory was a good grounding for this task. The method employed was “to draw mathematical consequences from a few general principles based on experience [...] rather than to draw them from special pictures or mechanisms” (Jungnickel and McCormmach 1986, 116). “By ignoring special mechanisms, Voigt impressed upon theoretical physics a characteristic direction” (Jungnickel and McCormmach 1986, 124). This was apparent, for example, in his textbooks of the years before 1900, but it was not in terms of research facilities and buildings. A new institute had been promised upon his appointment in 1883, yet the new buildings were not inaugurated before 1906. Voigt was fairly well equipped for his main research field of optics and crystal physics, for his interest in the Zeeman effect “he had begged and borrowed spectroscopic apparatus and a powerful electromagnet [...] These were small gifts compared with what industrialists gave Göttingen’s applied physics institutes [...]” (Jungnickel and McCormmach 1986, 269).

It was the rapidly increasing number of students, in particular, that made Voigt worry about necessary equipment, and this situation eventually rendered him more or less unable to do further research. In a ceremonial address to the university about “physics research and

⁵(Jungnickel and McCormmach 1986, 115), paraphrasing a letter of H. A. Schwarz to Voigt, 7 January 1883.

⁶For literature on Voigt cf. (C. Runge 1920; Försterling 1951; Goldberg 1970b; Jungnickel and McCormmach 1986; Heilbron 1994; Wolff 1996).

teaching in Germany during the last hundred years” in 1912, Voigt did not hesitate to allude to the situation at his institute:

Of course, the red [brick] walls of the new institute tell nothing of the long tenacious fights that preceded its erection, nothing of the budget calamity that reigns behind them, nothing of the strange means which alone are ensuring that the operation of the institute is maintained in an appropriate way, nothing of the overcrowding which, true to our predictions, occurred few years after the institute’s inauguration.⁷

The “strange” means mentioned here—viz. ways of operation and unconventional funding—point, in fact, to Voigt’s personal subsidies for equipment and personnel, which, according to his assistant at that time, sometimes equaled the regular budget. Too late, he realized that this way of improving working conditions was wrong (Försterling 1951, 221). The point he made repeatedly, that even a provincial institute like Göttingen should have the right to be well-equipped at least in one field (like spectroscopy in his case), never did receive a warm welcome from the state.

As for the topic of the establishment of quantum physics, it might be instructive to recall Voigt’s research efforts on the Zeeman effect, an important proto-quantum problem⁸ that contributed greatly at various stages in the development of atomic and quantum physical concepts. In an address delivered on the occasion of the inauguration of the new buildings, which took place in 1906, Voigt told his audience, which included the 1902 Nobel laureate Zeeman,

As I have dealt with the theory of these phenomena for a long time, I have also aimed to provide a home for its experimental investigations in our institute. For it is true that this physics laboratory of a provincial university naturally cannot be adequately equipped for all research fields; however, it may claim to own first-rate instruments for those problems that are worked on creatively at this place.⁹

Having made clear his dissatisfaction with the budget of his institute that was of a “depressing austerity” [bedrückende Dürftigkeit]—bankruptcy was prevented only through private sacrifices. He even addressed the representatives of the Prussian Ministry of Culture, directly stating that

One cannot but hope, that when only the royal state government supports us with a belated grant of a moderate fraction of the curtailed 50 percent, we will

⁷“Freilich erzählen die roten Mauern des neuen Institutes nichts von den langen zähen Kämpfen, die ihrer Aufrichtung vorangegangen sind, nichts von der Etatmisere, die hinter ihnen herrscht, nichts von den seltsamen Mitteln, durch die allein der Betrieb des Institutes in angemessener Weise aufrecht erhalten wird, nichts von der Überfüllung, die, unserer Voraussage entsprechend, in wenigen Jahren nach der Einweihung des Institutes eingetreten ist.” (Voigt 1912, 16).

⁸The term “proto-quantum” problem was introduced by Jürgen Renn, cp. (Renn 2000).

⁹“Wie ich mich lange Zeit um die Theorie dieser Erscheinungen bemüht habe, so habe ich auch danach gestrebt, ihrer experimentellen Bearbeitung in unserem Institut eine Heimstätte zu bereiten. Denn wenn zwar dies physikalische Laboratorium einer Provinzialuniversität naturgemäß nicht für alle Forschungsgebiete vollwertig ausgerüstet sein kann, so darf es doch wohl beanspruchen, für diejenigen Probleme, in denen am Ort schöpferisch gearbeitet ist, erstklassige Hilfsmittel zu besitzen” (Voigt 1906, 41).

someday get a truly productive institution for the study of the Zeeman effect and its related phenomena.¹⁰

Voigt's book of 1908 on magnetic and electrical optics, though motivated by Zeeman's and Lorentz's work, was later praised for its mathematical elegance and great orderliness rather than any experimental or theoretical advances (Goldberg 1970b). And Voigt himself regarded his *opus magnum* on crystal physics of 1910 as a document of estrangement from current interest (Jungnickel and McCormmach 1986, 273).

Being a good friend of both Zeeman and Lorentz, with whom he exchanged a host of letters, was one side of Voigt's scientific life,¹¹ the largely unsuccessful fights for resources another. By 1911 things had not changed significantly as he writes to Lorentz:

Well, in my inner self I always feel it oppressive how little I am able to accomplish, and when I look back tired in the evening after the day and realize with what trifling work it was spent—discussions with students on elementary theoretical and technical questions—then my job appears menial to me. Recently I wrote to Planck: while he with others—with you in particular—dealt with the pure ether of fundamental questions, I am a mole burrowing in the soil for minor specialties.¹²

Voigt's colleague Eduard Riecke came to Göttingen on a scholarship in 1870 and obtained his doctorate under Weber in the following year. He became Weber's assistant and received the *venia legendi* for both physics and mathematics. In 1873 he became professor and he finally succeeded his teacher as a full professor in 1881.¹³ Riecke and Voigt did not separate their research fields and equipment, but demonstrated great harmony in sharing the institute and its facilities. It was, however, also a marriage of purpose in part, as their interests and attitudes differed in many respects. Riecke, for example, in distinction to Voigt, made great use of hypothetical models and worked on cathode rays to show their independence from the cathode material in order to confirm the existence of the electron. As one may summarize his work with Geissler tubes, on atmospheric electricity, on the behavior of crystals and on the electrical conduction of metals from 1880 to 1915 under the main objective of proving an atomic structure of electricity, the identification of his work as dealing with proto-quantum problems still does not appear straightforward.

Riecke is hence probably less central than Voigt, at least his pronounced statements and activities are few. Concerning his views on the future of the quantum ideas in particular, there

¹⁰“Es steht zu hoffen, daß, wenn uns nur die Königliche Staatsregierung durch die Nachbewilligung mäßiger Bruchteile der gestrichenen 50 Prozent unterstützt, wir in einiger Zeit in den Besitz einer wirklich leistungsfähigen Einrichtung zum Studium des Zeemaneffektes und seiner Begleiterscheinungen gelangen werden” (Voigt 1906).

¹¹Voigt's correspondence with Lorentz and Zeeman of many years is a source rarely consulted; cf. e.g. (Wolff 1996).

¹²“Ich fühle ja im Inneren immerzu drückend, wie wenig ich zu leisten vermag, und wenn ich am Abend müde nach dem Tag zurücksehe und mir klar mache, mit wie untergeordneter Arbeit er verbracht ist—Verhandlungen mit den Schülern über elementare theoretische und technische Fragen—so kommt mir meine Tätigkeit subaltern vor. Ich schrieb noch kürzlich an Planck: während er mit Anderen—mit Ihnen insbesondere—sich im reinen Aether der allgemeinsten Fragen bewegte, wühlte ich als Maulwurf in der Erde nach kleinen Spezialitäten.” Voigt to Lorentz, 18 August 1911. Lorentz Correspondence IV (AHQP).

¹³For literature on Riecke cf. (Voigt 1915b; Sommerfeld 1916b; Goldberg 1970a; Jungnickel and McCormmach 1986).

is one remark of significance to be found in a letter to Stark. Riecke wrote in October 1911—as we will see, briefly after quantum physics entered research and teaching in Göttingen—that

[...] I do not consider the relativity principle and quantum theory as definite forms in which we can put our physical knowledge, but physics will be quite a step further when everything has been depleted that can be learned from these principles or concepts.¹⁴

In Voigt's eyes, Riecke stood for a modest and conservative research program of precise measurements similar to his own, “[...] where he performs experiments, it is usually never a question of pioneering into undeveloped fields, but of doing measurements under the guidance of theory.”¹⁵

Apparently, Riecke made peace with quantum theory only in the last month before his death, as seen in a brief review of Bohr's theory he wrote, which appeared posthumously and was immediately used to reinterpret his attitude. It was Debye who turned Riecke's strong reservations into an ostensible motivation to deal with the quantum in the first place, when he wrote in an introduction to his late colleague's final publication that he had “followed with rising interest and youthful freshness the new and far-reaching achievements physical research had gained by the extension of the sphere of influence of Planck's quantum of action, literally until his very last days.”¹⁶ The Göttingen mathematicians enjoyed a much higher visibility and reputation than their physics colleagues Riecke and Voigt. Felix Klein and David Hilbert were the strongest players and both had build up strategic relations to neighboring fields. Klein was an eager advocate of linking mathematics to technical applications, while Hilbert focused on theoretical physics, which he wanted to make more rigorous with the help of mathematics. Hilbert is often considered the preeminent mathematician of the twentieth century, who founded influential and fruitful research programs that continue to enthral mathematicians up to the present day. Like Voigt, his senior by twelve years, the only two sites of activity in Hilbert's life were Königsberg and Göttingen. In Königsberg

¹⁴ “[...] relativitätsprinzip und quantentheorie halte ich für keine definitiven formen, in die wir unsere physikalischen kenntnisse fassen werden, aber die physik wird sicher um einen guten schritt weiter sein, wenn sie alles erschöpft hat, was aus diesen principien oder vorstellungen zu lernen ist. Ueber das relativitätsprinzip hatten wir am anfang des vorigen semesters grosse aber resultatlose debatten in der physikalischen und mathematischen gesellschaft. Die mathematiker sind von der eleganz der rechnungsregeln hypnotisiert, die physiker kritisch.” Riecke to Stark, October 13, 1911, cited in (Tobies 1994, 348).

¹⁵ “Und wo er experimentiert, handelt es sich der Regel nach nicht um das pioniermäßige Vordringen in ein noch unerschlossenes Gebiet, sondern nach Messungen nach Anleitung der Theorie” (Voigt 1915b, 7).

¹⁶ This paper appeared with an introduction by Debye: “Mit stets steigendem Interesse und jugendlicher Frische verfolgte Riecke buchstäblich bis zu seinen letzten Tagen die neuen weittragenden Errungenschaften, welche der physikalischen Forschung durch die Erweiterung der Einflusssphäre des Planckschen Wirkungsquantums beschieden waren. Das viele Unklare und Unbefriedigende, das noch unüberwunden sich in diesem Gebiete einem restlosen Verstehen entgegenstellt, war ihm dachaus nicht ein Grund zur überkritischen Ablehnung. Vielmehr schöpfte er gerade daraus einen wesentlichen Teil der Freude, welche ihm seine Beschäftigung mit dem Atominneren eingebracht hat.

Restlos überzeugt von der großen Wichtigkeit und Tragweite der neuen Ideen, war es ihm unmöglich als müßiger Zuschauer abseits am Wege zu stehen. Er empfand das Bedürfnis zunächst in möglichst weiten Kreisen das Interesse für die reizvollen neuen Probleme, welche hier zum Greifen nahe liegen, zu beleben und zu wecken. In dieser Absicht und in diesem Sinne verfaßte er in der allerletzten Zeit vor der Krankheit, die mit dem Tode endigen sollte, die folgende Notiz, welche in knapper Form das Wesentliche der neueren Ansichten über das Entstehen und die Gesetzmäßigkeiten der Spektren darstellt.” (Riecke 1915, 222).

he had to wait six years as a *Privatdozent* before becoming an associate professor, finally receiving a full professorship in 1893. “In 1895, 33 years old, I was called to Göttingen by Felix Klein,” he once wrote, “essentially, I think, due to my work on the theory of invariants; the central problems of it I had tackled and solved from a novel point of view.”¹⁷ Hilbert gained great prominence with his axiomatization of Euclidean geometry in 1899, and even more in 1900, when he gave a speech in Paris listing problems that set an agenda for generations of mathematicians. Making his point pronouncedly against Emil Dubois-Reymond, he declared that there is no *ignorabimus*, at least not in mathematics. While Hilbert’s vigorous scientific leadership is undisputed, at the same time he is often seen as a rather quixotic and even childish genius.¹⁸ With his spectacular successes at the turn of the century Hilbert monopolized Göttingen’s reputation for mathematical progress. Klein stepped aside, concentrating on his role as a science organizer.¹⁹

As it may not be clear at this point why Hilbert should be of importance for a study on the early history of quantum theory in Göttingen, it should be obvious that incorporating the mathematical dimension of this development is necessary. Many later quantum physicists started as mathematicians, and leading mathematical figures like Hilbert and Henri Poincaré included physics in their greater mathematical visions—the axiomatization of physics was simply Hilbert’s “sixth problem.”

For a first comparison of Voigt and Hilbert two points shall suffice. Their different administrative engagement in the university might strike one’s eye first, as Voigt repeatedly held official posts like that of the rector of the university or dean of the faculty, while Hilbert was strikingly abstinent in this respect. When it came to important decisions, however, this was not the case. In his autobiographical sketch he wrote:

In all respects of organization Klein undisputedly had the leadership; I never cared about administrative things. But when it came to essential decisions, in particular on appointments, on the creation of new positions and the same, I always took an active interest.²⁰

Secondly, there were the frequent offers of appointments to Hilbert that gave him weight in negotiations with the ministry about improvements to his conditions in Göttingen, critical weight Voigt badly needed.²¹

Hilbert’s relationships with the other Göttingen professors was not always smooth, and even towards Klein Hilbert himself did not gloss over the “differences in temperament.”²² In a dispute about the new professor Friedrich Schilling in 1900, Hilbert fought against Klein

¹⁷Document 17.

¹⁸For a biography of Hilbert cf. (Reid 1970). Note, however, that this (as acknowledged in the foreword of the second edition) is a “romantic” book that lives from a certain “mathematical innocence” at the time of writing. Nonetheless, it has benefited much from the assistance of Richard Courant and Paul Ewald and is a quite reliable source of information about Hilbert’s life, despite the romanticization of his personality. Unfortunately, no references are given. Some attempts to shed light on Hilbert’s personality by dismissing the traditional and unverified pictures of a naive eccentric or ivory tower dreamer can be found in (Rowe 1992).

¹⁹Rowe’s distinction between Hilbert as the pure *Fachmathematiker* (a mathematician only concerned with his field) and Klein as the *Wissenschaftspolitiker* (science politician) in (Rowe 1989, 201f), will be scrutinized in the following.

²⁰Document 17.

²¹Hilbert lists calls from Berlin (multiple), Leipzig, Heidelberg, and Bern, cf. document 17.

²²Document 17.

and Voigt. While the former “gave up his hangover idea [Kateridee] in time on his own and—as must be acknowledged—unconditionally and for all times” the latter “who with pleasure and on all occasions inveighs against Klein behind his back but fails completely when it is necessary to speak up,” remained opposed, to Hilbert’s annoyance.²³ Without question, the often invoked picture of a Göttingen community of mathematicians and physicists of mutual interests and forces is a untenable hypothesis.

As a consequence of this particular assemblage of resources, interests, personalities and their coalitions in Göttingen, which is already apparent from the few examples mentioned, one must expect that the history of Göttingen’s ascent to a center of quantum physics during the first quarter of the twentieth century will turn out to be the result of a rather complex interaction of forces that steered the whole course in one direction, not fully foreseen by the different players, who pushed in their various directions.

In order to exhibit this parallelogram of forces, which at the same time relates to a kind of subtle economy of resources, I will proceed in four steps. First, I analyze the opportunities, retirements and new positions offered for a new alignment of research fields and personnel in the years before the Great War. Then I try to identify a certain “vision” that was central in this reorganization of Göttingen physics and ascribe it to Hilbert. Next, I reconstruct the role of Debye, who made a first research and teaching effort in order to establish quantum theory in Göttingen even back during the war. And finally, I consider the various groups and collaborators around Max Born in Berlin, Frankfurt and Göttingen, whose activities paved the way to matrix mechanics, and try to explain the extent to which this was the product of a long-term program - both experimental and theoretical - that drew on various resources including institutional ones, which converged in an extraordinary way at Göttingen.

²³“Die Affaire Schilling ist zu aller Zufriedenheit erledigt. Schilling bleibt—aber als Extraordinarius.

Für mich hat die Sache das Gute gehabt, dass ich die Gewissheit erhalten habe, dass ich mich in solchen Fragen auf die Fakultät absolut verlassen kann; sie hätte wie ein Mann auf meiner Seite gestanden—trotz Voigt, der ja stets mit Behagen und bei jeder Gelegenheit hinter Klein’s Rücken auf diesen raisonniert, aber, wenn es gilt ihm entgegenzutreten, völlig versagt. Voigt habe ich meine Meinung gesagt. Klein gab selbständig rechtzeitig und—wie anerkannt werden muss—bedingungslos und für alle Zeit seine Kateridee auf; es ist ihm, glaube ich, überhaupt unangenehm, dass er sie je gehabt hat.” Hilbert to Sommerfeld, 27 September 1900, DM, Archive HS 1977–28/A, 141.

5.2 Difficulties and Chances within a Generational Change of Physics Professors

Scientific change is most often also a change of scientists, but does the inverse also make sense? Perhaps it may when the changes are made intentionally. Thus the first field of research politics—which does not necessarily add up to a stringent policy—considered here concerns the discussions and even fights about candidates and successors for the faculty at Göttingen. In the following I begin with the description of the changes in the personnel of the chairs of physics and neighboring fields. These are included to illustrate the rather exceptional changes in some fields like physics. The analysis of continuities and discontinuities is then employed to identify driving forces behind the observed changes.

5.2.1 The Dynamics of the Main Mathematics and Physics Positions, 1900–1926

German ministerial and university bureaucracy typically have kept detailed records about personnel matters, as the formal processes for filling professorial positions were complex.²⁴ Therefore there is ample material to reconstruct the development of the staff in the physical and mathematical sciences, at least according to the “official” reading. I will proceed in describing the institutional and personnel dynamics in this way, starting with mathematics and then turning to theoretical, experimental and applied physics as well as related fields, in order to finally arrive at a pattern of structural change. This will in particular exhibit some crucial instances at which local conditions and specific actors were able to exert influence.

Mathematics

From 1904 on the mathematics section of the philosophical faculty was represented by four full professorships, relegating Berlin to second place in mathematics (Lorey 1916; Rowe 1992; 1985, 436). How much Göttingen dominated the field of mathematics is apparent, for example, in the request by Ferdinand Frobenius to the Ministry of Culture to fund a fourth chair in Berlin and to appoint Hilbert in 1914. Frobenius wrote that while Hilbert had declined the offer in 1902 “in the mean time, however, after Berlin has become a little Göttingen itself, he might have changed his mind.”²⁵ Hilbert remained in Gauss’s chair until 1930, when Hermann Weyl became his successor, not really a close disciple but near to his understanding of mathematics.

The next important mathematics position in Göttingen was that of Felix Klein, who had to retire early in 1913 for health reasons. Constantin Carathéodory was his direct successor, then Erwin Hecke took over the position in 1917, followed by Richard Courant in 1920. Weyl and Brouwer were also present on all the three lists of candidates prepared by the committees for the ministry. Gustav Herglotz was also suggested in 1919, but eventually none of the three actually came. After Weyl took half a year to consider the offer and finally decided against it, a further proposal became necessary. This allowed Courant to return to

²⁴I have used, in particular, the following archival sources: Geheimes Staatsarchiv Preußischer Kulturbesitz Berlin, GStA PK Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV–XXVIII (correspondence with the ministry of culture); Universitätsarchiv Göttingen, Kur. 4 I 105 (formerly XVI.V.B.7), Vol. I–II (correspondence with the university curator); Kur. 4 Vc and Kur. P.A. (staff files); Phil. Fak. II Ph. 36 a-f (faculty issues); Niedersächsische Staats- und Universitätsbibliothek, Special Collections, Papers of David Hilbert, Felix Klein, Woldemar Voigt, Karl Schwarzschild.

²⁵Frobenius to Ministry 22 June 1914, reprinted in (Biermann 1988, 325): “Damals [1902] hat er den Ruf abgelehnt; inzwischen hat er sich, nachdem Berlin ein kleines Göttingen geworden ist, vielleicht eines besseren besonnen.”

Göttingen. Interestingly, almost all of the proposed candidates were students of Hilbert (only Carathéodory did his doctorate with Minkowski), and none of them of Klein.

In 1902 Hilbert had succeeded in trading a call to the prestigious Berlin university for a new full professorship for Hermann Minkowski. After his sudden death, Minkowski was succeeded by Edmund Landau in 1909. Though Hilbert was eager to find a position for Zermelo, Landau was his choice, as he was probably Klein's.²⁶ And the fourth position was the one created for Carl Runge in the new field of applied mathematics, this time at the instigation of Klein in 1904, who was at excellent terms with Friedrich Althoff, the most influential planner of faculty hiring at Prussian universities. Runge also played a certain role for theoretical physics, as he was interested in spectra.²⁷

In 1925 Gustav Herglotz, although a representative of pure mathematics, became Runge's successor in applied mathematics, augmented by astronomy.

²⁶The faculty named Hurwitz, Blumenthal and Landau in no particular order, cf. (Rowe 1992, 564 f.); it has been suggested that Hilbert supported Landau, cf. (Peckhaus 1990, 121). In an undated letter to Klein in 1909, Hilbert neutrally writes that Landau accepted the call and immediately afterwards reports that he was successful in increasing Zermelo's remuneration considerably. He adds that Landau will lecture mainly on prime numbers, "Crooked lines and surfaces do not suit him at the moment" (Frei 1985, 138). In a letter to Sommerfeld, however, Hilbert stated that Landau was his choice: "Fakultät und Ministerium legten die Wahl ganz in meine Hände; ich hoffe, dass sie gut ausgefallen ist." Hilbert to Sommerfeld, 10 April 1909, (Sommerfeld 2000, 356–358, on 357).

²⁷Cp. the article (C. Runge 1907) which was presented in the Göttingen Mathematische Gesellschaft on 7 May 1907.

| Position | Candidates | Notes |
|---------------------------------------|---|---|
| new position 1902 | Minkowski , Königsberg | Hilbert's condition to stay |
| Minkowski 1909 | Hurwitz, Zurich Blumenthal, Aachen Landau , Berlin | all placed on equal level, all of Jewish decent |
| Schwarzschild 1909 | 1. Hartmann , Potsdam 2. Cohn, Königsberg 3. Kapteyn, Groningen | |
| Klein 1913 | 1. Carathéodory , Breslau 2. Weyl, Göttingen and Brouwer, Amsterdam | |
| no free position 1914 (Voigt 1915) | Debye | plan: early replacement for Voigt, not Riecke |
| Riecke 1914 | 1. W. Wien, Würzburg 2. Paschen, Tübingen 3. "first" Stark, Aachen, "second" Zeeman—Amsterdam | Simon, v. Sehlhorst, G. Frölich, G. E. Müller, Lorenz, Mossbach, Pohlenz, I. Hartmann against placing foreigner Zeeman |
| – new list 1915 | 1. E. Meyer, Tübingen 2. Pohl , Berlin, P. P. Koch, Munich [Franck]* [Schweidler, Geiger, Grüneisen]** | |
| Wallach 1915 | 1. Willstätter, KWI "unavailable" (declined 23 June) 2. Thiele, Strasbourg (28 June) 3. Harries, Kiel (8 July); Auwers, Greifswald; Knorr, Jena | |
| – new list | 1. Windaus , Innsbruck 2. Wieland, Munich 3. Diels, Berlin 4. Braun, Breslau ... | |
| Caratheodry 1917 | 1. Hecke , Basel 2. Brouwer, Amsterdam and Weyl- Zurich 3. Blaschke, Königsberg | |

| Position | Candidates | Notes |
|----------------------------|--|---|
| Hecke 1919 | 1. Brouwer, Amsterdam 2. Herglotz, Leipzig 3. Weyl, Zurich | statement of Hilbert |
| – added 1920 | 4. Courant , Münster | |
| Simon 1919 | 1. M. Wien, Jena 2. Gaede, Freiburg | the prestigious chair for technology created by Klein now given to a local <i>Privatdozent</i> |
| – new list | 1. Prof. Dr.-Ing. Rüdberg, Oberingenieur at Siemens-Schuckert and PD at TH Berlin 2. Barkhausen, Dresden 3. Möller PD Hamburg or Reich , PD Göttingen | |
| Debye 1920 (Voigt 1915) | 1. Sommerfeld, Munich (2. Born, Frankfurt 3. Mie, Halle) | officially Riecke's old position for experimental physics, Born argued that Voigt's position had yet to be filled |
| – modified list | 1. Born , Frankfurt 2. Madelung, Kiel 3. Lenz, München + Franck , Berlin | |
| Hartman 1921/1927 | Kienle , Göttingen Wirtz, Kiel Graff, Hamburg | |
| Runge 1925 | Herglotz , Leipzig | |

Table 5.1: Candidates for mathematics and physics professorships 1900–1926.

*Mentioned in interview Debye 1962, p. II/5 (AHQP).

**Invited for talks in winter term 1915 of candidates for the position, details see note 153 below.

Physics

In contrast to the mathematicians, the years from 1914 to 1921 saw much greater change in the main physics positions. The changes resulted in rather complicated patterns of replacements and successions. This dynamics in personal resources reflects difficulties in acquiring proper successors and, probably, also differences about prospective research programs as well as opportunities for exerting influence to set a new course.

The two leading physicists in Göttingen, Woldemar Voigt and Eduard Riecke, set great store on smooth cooperation, as neither of them restricted himself to the experimental or theoretical domain alone, attending to both lecturing duties and research projects. Yet formally, Riecke had the experimental chair and Voigt the theoretical one. Both had become ordinary professors of physics in Göttingen around 1880. Riecke was mostly interested in the corpuscular structure of electricity, Geissler tubes and the theory of conductivity, while Voigt, who was probably the most productive theoretical physicist in the 1880s, worked on the properties of crystals and their interaction with light (Jungnickel and McCormmach 1986, 112–124).

Riecke and Voigt had intended to retire at the same time in 1915 to make room for first-class replacements. Peter Debye, however, was called to a position that had not existed before, but was created for him in 1914. The idea was that he might take Voigt's chair the following year, but due to the war Voigt had to teach up to his death in 1919, as had Riecke, who died in 1915. The circumstances of Debye's appointment will be detailed in section (5.4) against the larger backdrop of the general developments in Göttingen in both mathematics and physics.

For Riecke's succession four physicists were nominated, all of whom coincidentally were associated with research on quantum problems: Wilhelm Wien in first place, then Friedrich Paschen, and Johannes Stark and Pieter Zeeman equally on the third rank. However, none of these men came. Wien was looking ahead to move on to Röntgen's prestigious chair in Munich, and Paschen negotiated too long, such that the ministry finally rescinded the offer, while Stark was not welcome to one wing of the faculty as Zeeman was not to the other.²⁸

The fact that Voigt's chair did not become available as planned due to the war, and the insight that Debye's experimentalist capabilities qualified him beyond theory, made the move to make him Riecke's successor instead appear attractive. This, in turn, meant that another subordinate position opened up for teaching students from other fields like medicine and chemistry. For this position younger candidates were nominated like E. O. Meyer, in first place, and Robert Pohl and Peter Paul Koch in second. After considering an even larger circle, James Franck, Egon Schweidler, Hans Geiger and Eduard Grüneisen, Pohl was finally chosen in 1916; however, he did not take his position until after the war and was soon after promoted to a full professorship in 1920.

In the same year Max Born was called (with Erwin Madelung and Wilhelm Lenz as alternatives) to succeed Debye, who was lured away by the rivaling Zurich ETH and the better living conditions Switzerland offered directly after World War I. Initially, the Göttingen faculty had suggested, besides Born, the more senior physicists Sommerfeld and Mie, but after the first choice Sommerfeld declined, the list was rewritten. Born succeeded in getting James Franck called for an additional full professorship, arguing that Voigt's posi-

²⁸See next section below with more details.

tion had not been filled after his death (although it had been traded for creating new and enhancing other positions).²⁹

Additional physics positions were Emil Wiechert's in geophysics and Karl Schwarzschild's in astronomy. Wiechert came to Göttingen in 1897 and soon became extraordinary professor of geophysics, though his more important work was in electron theory and he was known to move in the direction of pure physics. He was promoted to a full professor in 1904, at a time when he was offered the Munich chair of theoretical physics. With his promotion in Göttingen, he held the chair of geophysics until 1928. Schwarzschild, who came as director of the Göttingen observatory in 1901 and soon became full professor, contributed to questions of general physics, too, but unlike Wiechert, he was lost to the Potsdam observatory early in 1909. He was succeeded by Johannes Hartmann, yet his activities flagged through the war and he eventually left his position in 1921. A successor, Hans Kienle, was not appointed until 1927.

Applied Sciences

While specialized fields like geophysics and astronomy were pursued by their Göttingen proponents Wiechert and Schwarzschild with fundamental physics in mind, further positions in applied fields had been created on the initiative of Felix Klein, in particular. And while the position for technical physics, comprising fields like agricultural machines and aeronautics, was Ludwig Prandtl's from 1904 for over 40 years, the one for applied electricity was held until 1919 by Hermann Simon. Particular problems arose in finding an appropriate replacement for Simon. After the failure of the proposal to hire Max Wien and Wolfgang Gaede, a second list was submitted with Reinhold Rüdtenberg, a Siemens engineer, on the top position, then Heinrich Barkhausen and finally Hans Georg Möller, who was preferred even over Max Reich, the last choice. It was Reich, however, Simon's former assistant, a Göttingen *Privatdozent*, and only of second-rank quality, who would become representative of the field of advanced technical applications once so promoted by Klein in the early Weimar period.

Chemistry

Besides applications in technology, chemistry was the other big field that would develop ever closer relations to physics and its new theories on atoms and quanta. Chemistry in Göttingen in this period was just about to become a club of Nobel laureates. Physical chemistry was introduced by Nernst, winner of 1920, who, however, left for Berlin in 1904. He was succeeded first by Friedrich Dolezalek, and then in 1907 by Richard Zsigmondy, who received the Nobel prize in 1925. After futile efforts to get Clemens Winkler, William Ramsey (NP 1904) and T. W. Richards (NP 1914) for a chair of inorganic chemistry, Gustav Tamman was hired in 1903, leaving this field to Zsigmondy and doing physical chemistry instead. Nearly as difficult as the Simon succession but with a better outcome was the case for Otto Wallach's organic chemistry chair. In 1915 the Nobel winner of 1910 was replaced by the later laureate of 1928, Adolf Windaus.

²⁹Slightly differing accounts can be found in (Hund 1987; Jungnickel and McCormach 1986; Dahms 2002; Greenspan 2005).

Scientific Philosophy

The parallel developments of philosophy positions related to mathematics and the sciences show similarities to the problems in field of physics and also in terms of involvement by mathematicians. When the Prussian Ministry suggested to the Göttingen philosophical faculty a third professorship for Edmund Husserl in 1900, this met with stiff opposition from the philosophers—who were largely in favor of experimental psychology and philology. They were ignored one year later, however, when Husserl, a philosopher trained in mathematics and interested in foundational issues, became extraordinary professor.³⁰ He entered into a fruitful exchange with Hilbert in the *Mathematische Gesellschaft*. When the third *Ordinariat* was suggested again in 1908, Natorp, Maier, and Cassirer were put on the list of the faculty. Hilbert, however, demanded the promotion of Husserl to this position, thus causing persistent trouble in the faculty. Maier was called only in 1911 and left for Berlin in 1918, which again caused major problems between the “historical-hermeneutical” and the “natural-scientific” wings of the faculty: Husserl accepted an offer from Freiburg for a full professorship in 1916. Hilbert tried hard to save this position for Leonard Nelson, also a mathematically trained philosopher. Finally, Georg Misch was called to Husserl’s position in 1918 and promoted to Maier’s in 1919, making room to confer an extraordinary professorship on Nelson at the same time. The list of natural scientists named Schlick second. Courant even suggested that Weyl succeed Maier,³¹ as Hilbert had nominated the vitalist philosopher Hans Driesch.³² As it was the case for the physics chairs, the part of the philosophy faculty that dealt with science and mathematics also underwent considerable change. This fact suggests that the mathematicians and physicists in the philosophical faculty tried to establish a kind of philosophy to their taste.

And a Host of Privatdozenten

This picture of the human resources of Göttingen physics and neighboring fields would not be even roughly appropriate without mentioning the relevant staff at Göttingen below the professorial rank. Typically, the the aspirants for future professorships play a decisive role for the scientific potential of a research site. In Göttingen the great number of *Privatdozenten* turned out to be a special characteristic. As David Rowe has put it, the richness of Göttingen in qualified scientific candidates was striking:

Most impressive of all, between 1890 and 1914 Göttingen had no fewer than eighteen *Privatdozenten* whose names read like a “Who’s Who in German Science” during the Weimar era [...]. This is in contrast with the situation in Berlin, where between 1897 and 1901 there were no *Privatdozenten* whatsoever. (Rowe 1989, 202)

The same remained true for later years as well. The picture that emerges for the years of nascent interest in quantum theory up to the establishment of quantum mechanics is given in table (5.2). In this table mathematical subfields are not distinguished; with respect to physics,

³⁰Cf. also for the following: (Peckhaus 1990, 208ff).

³¹Courant to Hilbert, July 1918, as cited in (Reid 1970, 90).

³²Cf. the answer Becker to Hilbert, 10 October 1918, Hilbert Papers, folder 15A, Bl. 1/1–1/2.: “Ihre Wünsche gehen aber darin zu weit, wenn Sie meinen, mehr oder weniger sämtliche Lehrstühle der Philosophie in Göttingen mit naturwissenschaftlich orientierten Philosophen besetzen zu können.”

the differentiation is according to the three divisions of the physics department, which were directed by Max Born, James Franck and Robert Pohl in the 1920s. A special quality of Göttingen in the field of mathematical and physical sciences was hence that besides the strong group of professors *in office*, there was an equally large and strong group of professors *in spe*. They had the right (and duty) to teach and would do so mainly in the particular fields of their research interests. Apparently, self-recruitment was also widespread at Göttingen, as sooner or later scholars like Theodore Caratheodory, Hermann Weyl, Erich Hecke and Richard Courant from the mathematicians and the physicists Max Born, Max Reich and Werner Heisenberg (however, only after 1945 and for very special reasons) would return to the university as full professors where they had earned the qualification for this position.³³ Some of the *Privatdozenten* were also able to become special extraordinary or rather *außerplanmäßige* professors, though only outside the ordinary positions and without the pay and privileges of a civil servant, when they did not find such a post elsewhere and the Göttingen colleagues succeeded in finding them funding through special arrangements.

³³Note that the German system is a strictly non-tenure track, so that a scholar who did his habilitation at one university first needs to get a position at another in order to return to a professorship.

| Mathematics | Mathematical/ theoretical physics | Applied math. technical physics | Experimental physics | Physical chemistry |
|---|---|---|---|--|
| Carathéodory 04–08 Koebe 07–10 Bernstein 07–11 Toeplitz 07–13 H. Müller 08–10 Weyl 10–13 Schimmack 11–12 (didactics) | Abraham 1900–09 Ritz 09 Born 09–14 Haar 10–11 (Hellinger 07–09 nicht PD) | Herglotz 04–07 v. Kármán 10–13 | Stark 1900–06 Krüger 06–09 Bestelmeyer 06–17 Gerdien 07–30 (at Siemens since 1908) Rümelin 11–14 | Bose 01–06 Coehn 1899–1919, <u>21–28</u> scientific philosophy Nelson 09–19, <u>21–27</u> |
| Hecke 12–15 Courant 12–19 Behrens 13–17 | Madelung 12–19 P. Hertz 12–21, <i>21–33</i> | Reich 12–20 v. Sanden 11–18 | Rausch v. Traubenberg 12–21, <i>21–22</i> L. Geiger 12–19 Gerlach 17–18 | |
| Bernays 19–22, <i>22–33</i> Noether 19–22, <i>22–33</i> Neder 20–22 Behmann 21–25 Siegel 21–22 Ostrowski <i>23–27</i> | H. Kneser <i>22–25</i> | König 20–22 Betz 22–26, <i>26–?</i> Nádai 23–26, <i>26–29</i> | Scherrer 19–20 Busch 20–21 Grotrian 21–24 Gudden 21–24, <i>24–26</i> Oldenberg 22–26, <i>26–33</i> | Lipps 22–28, <i>28–36</i> |
| | Heisenberg 24–26 | Schuler 24–28, <i>28–41</i> Walther 24–28 | Goetz 23–29, <i>29–39</i> | |

Table 5.2: *Privatdozenten* in Göttingen 1905–1925.

Years in italics indicate “außerplanmäßige (nichtbeamtete außerordentliche)” professorships, i.e. special non-permanent lecturing positions without many professorial privileges; underlined years indicate permanent associate professorships. Data from *Catalogus professorum göttingensium 1734–1962*, W. Ebel, ed., Göttingen 1962, and *Amtliches Verzeichnis des Personals und der Studierenden der Kgl. Georg-Augusts-Universität zu Göttingen*, Göttingen 1905–1917.

In this way a number of scholars became professors in the crisis years after the Great War and the Revolution. Paul Bernays, who wrote a second habilitation thesis on axiomatics in 1922 (after he had already done so in Zurich with a work on function theory) became extraordinary professor of mathematics without tenure in the same year, as did Emmy Noether, who, as a woman, had to fight for many years to be granted the habilitation at all. The physicist Paul Hertz acquired the title of professor a year before; however, his field had shifted into a more philosophical direction, while Leonard Nelson, a philosopher who did his habilitation with David Hilbert back in 1909, but never found a position in philosophy until he was given an extraordinary professorship in the mathematics section of the philosophical faculty in 1919, which became tenured in 1921.³⁴

5.2.2 From Structures to Actors

The overview of positions and staff of physics, mathematics and related fields in Göttingen has provided a particular rich ensemble of scientists, quite a number of whom were able, and sometimes also eager, to relate to various fields of research outside the formal description of their appointments. Moreover, the fields of experimental and theoretical physics, which was often characterized as pure physics or physics proper, and was later famously represented by the triple professors Born, Franck and Pohl, was, in fact, broadly framed by a host of specialized and applied fields as well as related mathematical and philosophical projects. I will hence proceed by identifying a striking pattern or structure in the dynamics of this academic field, which then allows breaks, discontinuities and transformations to be identified, which shall be analyzed more thoroughly from a perspective of relevant actors.

If one summarizes the information collected on the dynamics of research positions in physics and neighboring fields at Göttingen between the turn of the century, and at the time when quantum mechanics was established in 1925 and 1926, in a pictorial way, one can easily observe how new lines emerge while other are discontinued. This indicates dates when decisions for changes in the focus of research fields occurred. On the one hand, one observes a fairly constant background configuration of positions in mathematics, chemistry, and some physics like geophysics and technical physics. On the other hand, great problems in personnel politics can be read from the graphical representation in the core physics positions: except for Wiechert's geophysics and Prandtl's technical physics, no other physics position preserved a continuity in the research field covered under the developments from 1914 to 1921. The broken lines in this picture, roughly speaking, represent the possibilities to establish new research programs; they may be considered in this sense as necessary, but clearly not as sufficient conditions for a shift of focus in the research interests of the physics faculty.

³⁴See (Ebel 1962) and (Tollmien 1991).

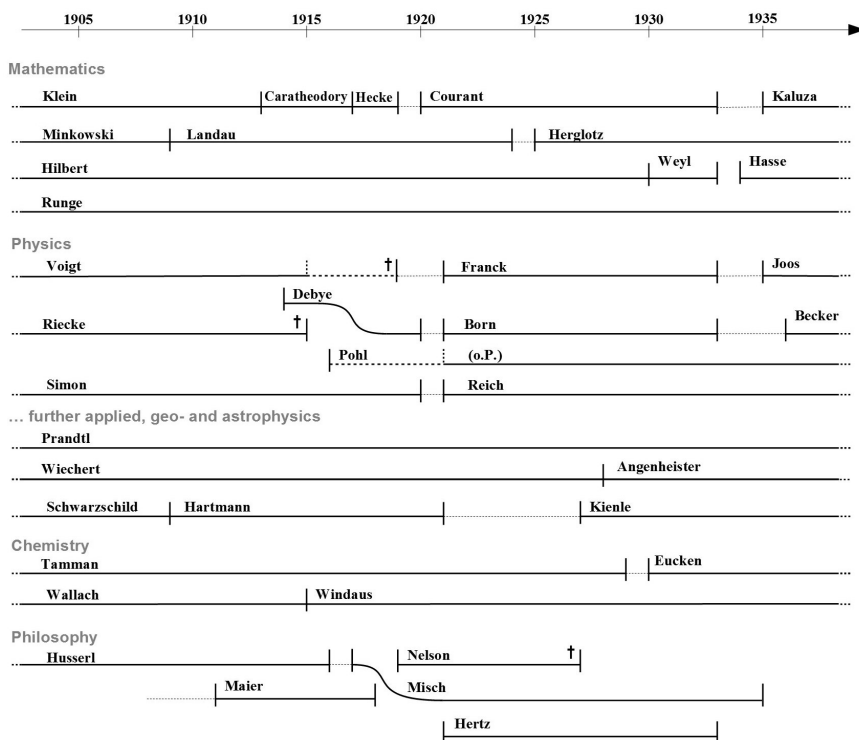


Figure 5.1: Dynamics of the main mathematics and physics positions as well as from related fields in chemistry and philosophy 1905–1935 (broken lines indicate extensions beyond regular duty or precursory appointments to ordinary positions; †deceased).

The Fate of Eduard Riecke's Chair in Experimental Physics

To give evidence for such a shift I now turn to some more details on a particularly difficult position. After Debye's call to a newly established position in fall 1914, which I will discuss below in greater detail, the succession of Riecke, the holder of what was understood to be the experimental physics chair, was the first instance for turning towards physicists who stood for quantum physical research. In December 1914 the faculty presented to the ministry a list of candidates that favored Wilhelm Wien. Wien, who had received the Nobel prize in 1911 for his work on heat radiation, a field in which he had been engaged since 1893, tried to relate X-ray wavelengths to Planck's quantum in 1907. Besides Lorentz, he was the only other weighty supporter of Planck's theory by 1910 and had been feeding the discussion on quantum physics ever since (Kuhn 1978a, 202ff). Publishing on the laws of black-body radiation on almost a yearly basis from 1893 to 1901 and from 1907 to 1915, Wien clearly exhibited experimental as well as theoretical expertise in quantum matters. For certain, his publication record also covered other branches of physics—among them sea waves, canal rays, and X-rays, yet by no means can his work be identified as a continuation of Riecke's

lines of research. In particular, their views departed most drastically on the future of the quantum.

When Riecke finally applied for retirement due to health reasons in fall 1914, he did not fight for a specific successor nor for a specific field, but merely required that a “fresh person in the middle of his development” should take over his position. He was more concerned about how the general mathematical level of the students in the experimental lectures could be improved in the future, suggesting “that nobody in particular should attend the lectures on experimental physics who is not accustomed to the elements of differential and integral calculus.”³⁵

It appears that Riecke’s chair was never officially offered to Wien.³⁶ As Hilbert pointed out to the ministry early in 1915, in his view Wien was the only one still missing in the new Göttingen physics team, at the same time, he did not believe that his appointment would be probable during the war.³⁷ To the ministry it may have seemed futile to negotiate with Wien, who was Röntgen’s presumptive successor. One year later Wien mentioned that he actually might have considered such an offer independently of his chances in Munich. But he concedes: “I cannot say, however, whether negotiations with me would have succeeded, since the conditions at the Göttingen institute are in fact not very favorable. The premises of experimental physics are relatively small since the theoretical physics has occupied as much.”³⁸

As early as one year in advance, the simultaneous retirement of Riecke and Voigt had been agreed upon, and since that time the first priority had been to find a single new director of outstanding quality.³⁹ When it seemed clear that Wien was out of reach, Paschen became the alternative.

In particular, Paschen met the requirement of being able to combine experiment and theory in a way Voigt and Riecke had performed collectively, which for them went back to Franz (and Carl) Neumann’s understanding of theoretical physics (Jungnickel and McCormmach 1986). A new kind of collaboration that differed considerably from the Neumann model had been established between Paschen and Sommerfeld in late 1915.⁴⁰ This was, however, only after the offer of the Riecke chair and long negotiations had finally failed.

Paschen’s research in his early career at Hanover stood in competition with the Berlin group, which was working on black-body radiation, viz. Rubens, Lummer, Pringsheim, and

³⁵Riecke to Ministry, 4 October 1914, requests that a “frische, noch mitten in der entwicklung stehende kraft an meine stelle tritt,” complains about “sehr ungleiche vorbereitung der zuhörer;” “Ich bin überzeugt, dass die schwierigkeit nur gehoben werden kann, wenn der physikalische unterricht an der universität auf einen höheren standpunkt gehoben werden kann, so dass er mehr als bisher eine einleitung in die mathematischen theorien der physik bildet;” “[...] insbesondere sollte keiner die vorlesung über experimentalphysik besuchen, der nicht mit den elementen der differential- u. integralrechnung vertraut ist.” GStA PK Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 108–109v.

³⁶Ministry to Göttingen Curator, 2 July 1915, writes that appointing Wien was “hopeless.” GStA PK Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 181.

³⁷Document 10.

³⁸Document 14.

³⁹Curator to Minister, 18 April 1914, GStA PK Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 69–70.

⁴⁰Cf. Paschen to Sommerfeld, 14 November 190, DM Archive HS 1977–28/ A, 253, who reflects upon the relationship between the theoretician and the experimentalist, questioning whether any pure experimentalist exists (“reinen Beobachtkünstler”), since even experimental physicists can do experiments in a theoretical manner (“theoretisch experimentieren”).

in particular Wien. As Paschen derived the same law as the latter and communicated it to him before its publication, it might well have been called the Paschen-Wien law.⁴¹

Paschen was in several respects a Berlin-independent physicist dealing with quanta and spectra. As Paul Forman has pointed out: “In striking and curious contrast with the Berlin experimentalists, who were literally enraged at him, throughout his work on the black-body radiation problem Paschen the pure experimentalist showed himself to be more than ready to enlist experiment in the service of theory” (Forman 1970b, 346). In addition, he collaborated with Runge on spectroscopy. After his appointment in Tübingen and a phase of work on radioactivity, X-rays, canal rays, and the mechanism of light emission, in 1908 he returned again to spectral series. Motivated by Göttingen student Walther Ritz, who spent winter 1907/08 in Tübingen, he looked for and found what later became known as the “Paschen series.” The Paschen-Back effect of 1912, again, was based on Ritz conceptions, and “was immediately seized upon as one of the potentially most revealing clues to atomic structure and the mechanism of emission of spectral lines” (Forman 1970b, 346). In 1914 Paschen began work on “Bohr’s helium lines,” an occupation that was to distract him from hurrying to the colors. It had become of utmost importance to him when Sommerfeld was inquiring about data for his theory, thus starting a fruitful collaboration in November 1915.⁴²

Paschen had considered the Göttingen offer seriously and it had turned out in the negotiations that securing working conditions in spectroscopy was a central point. But like Wien, he saw resources inequitably distributed between the experimental and theoretical institutes. For example, the new diffraction grating bought with support of the *Göttinger Vereinigung* promoting applied physics and mathematics in 1911 came to Voigt’s facilities, and was presented to the public in a paper on which he collaborated with a mathematician (Voigt and Hansen 1912). Under the condition of a new allocation of rooms and that “instruments for spectroscopy” would be transferred to him, he signed an agreement with the Prussian ministry to accept the Göttingen offer. Using a four-day respite, he negotiated with the Württemberg ministry after his conditions in Göttingen were met and then turned again to Berlin to ask for post-war budget guarantees. As a result, according to Elster, the official in charge, Paschen “couldn’t make up his mind” and the ministry asked for a new list of candidates, making Hilbert and Debye hurry to Berlin.⁴³ Having overplayed his hand, Paschen stayed in Tübingen with a lower salary than Göttingen had offered.⁴⁴ As a consequence, one may argue on the basis of the greatly increased correspondence with Arnold Sommerfeld on atomic spectra and models, Paschen intensified his collaboration with Munich physics instead (Sommerfeld 2000, 469ff.).

On their first list the members of the Göttingen commission had indicated that concerning Wien and Paschen they would “value the abilities of the above characterized men

⁴¹Cf. (Kangro 1969), compare also Heinrich Kayser’s critical view on Wien’s work in his *Erinnerungen*, (Kayser 1996, 136): “Through ‘Wien’s radiation law’ he became a great man and he was supported by the fact that about at the same time Paschen took great pains over deducing the same law experimentally.” [“Durch das ‘Wiensche Strahlungsgesetz’ wurde er ein großer Mann und wurde dadurch gestützt, dass etwa gleichzeitig Paschen dasselbe Gesetz mit unendlicher Mühe durch experimentelle Arbeit abgeleitet hatte.”]

⁴²(Forman 1970b, 346), and see correspondence Paschen to/from Sommerfeld, (Sommerfeld 2000; Paschen 1916).

⁴³Agreement, Paschen with Elster, 19 June 1915. Paschen to Elster and Debye to Elster, 23 June 1915. Paschen to Elster, 27 June 1915. Elster to Curator and Elster to Voigt 2 July 1915. Telegram Hilbert and Debye to Elster, 6 July 1915. GSPtKB Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 307–338.

⁴⁴Göttingen offered 8400 M basic salary, Tübingen 6500M. Agreement Elster with Paschen, 19 June 1915, GStA PK Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 307–309; (Forman 1970b).

so highly, [...] that we would rather restrict ourselves to mentioning their names.” In the third position they then added Stark “first” and Zeeman “second.” They explained, “We can, however, not suppress certain doubts in other respects [than their scientific abilities] against them.”⁴⁵

Immediately after Paschen’s withdrawal, Stark claimed that it was now his turn. His many letters to the ministry, in which he alternated between pointing out his ability to foster “German physics in its current crisis,” the humid weather in Aachen that was harmful to his wife’s health, and the allegation by Göttingen staff that he was an anti-Semite, admitting, “of course I fight against one-sided philo-Semitic attempts in academic circles.”⁴⁶

After things remained unresolved under these circumstances, Wien finally stepped in by writing to Naumann at the Ministry in December 1915. He tried to take the initiative in order to solve the problem of the full professorship in experimental physics in Göttingen, as he saw it, “in the general interest of our science.” This position, he explained, could become very important for the “future development of the German physics” or it could recede into “utter insignificance.” He argued that

[I]f no really productive person [*Kopf*] comes, all the younger workers in particular, who gather there like at hardly any other university, would not be properly guided and especially the many inspirations that come there like nowhere else from the mathematical and theoretical-physical side would remain unused. Naturally the lacking response will also be absent that should come from the side of experimental physics and should act on theoretical physics and mathematics, and which is particularly desirable for Göttingen. For the mathematical developments there currently predominate the experimental ones to such an extent, that they became, as one might say, almost autocratic against their own will.⁴⁷

This unequivocal assessment points to two distinctive characteristics of the development of physics in Göttingen in the 1910s. First, the guiding role of mathematics for the development of physics is noted as a source of inspiration for theory. Here Wien’s term “autocratic” might be read as the turning to criteria of inner consistency and notions of simplicity that motivate expectations for physical relationships. Second, it draws attention to the fact that not only unfamiliar mathematical reasoning and content, but also a special group of “younger workers,” e.g. mathematicians, infiltrated physics in Göttingen. Thus Wien’s comments both realize the extraordinary energy of young talent and foreshadow, however deprecatingly, theories like the later Göttingen matrix mechanics. Further, in his letter, we find Wien drawing a most alarming scenario:

In my opinion the appointment of a young physics mediocrity [...] would be the worst. Then physics in Göttingen would be paralyzed for more than a generation. [...] It is my conviction that this would occur if one of those gentlemen

⁴⁵Dekan to Minister, 24 December 1914, GStA PK Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 124–126v. “Wir schätzen die Fähigkeiten der beiden Vorstehenden charakterisierten Männer so hoch und würden die Berufung eines jeden von ihnen mir so lebhafter Genugung begrüßen, dass wir uns am liebsten auf die Nennung nur ihrer Namen beschränken” 124a)v) concerning Starck and Zeeman “gegen die wir aber gewisse Bedenken in anderer Hinsicht [als Forschungsleistung] nicht unterdrücken können.”

⁴⁶Stark to Ministry, 18 June and 1 July 1915, GStA PK Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 326, 330.

⁴⁷Document 11.

particularly recommended by some parties, viz. Franck, Pohl, Edgar Meyer, were to be appointed at Göttingen.

Considering Stark to be the only scientifically sound candidate, he writes,

[...] I can only regret that the appointment of Stark, whom I consider by far the most appropriate candidate despite his personal shortcomings, seems impossible for personal reasons. But if the appointment of Stark is truly ruled out, it will still be better to take a physicist who is not quite so young, but can offer positions to younger researchers, than to call one who is completely inexperienced in organizational matters and who eventually will leave the imprint of mediocrity on the Göttingen institute for four decades.⁴⁸

It was to come differently, and some of the personifications of mediocrity would eventually become Nobel winners like Stark. The Göttingen faculty was finally supported in its decision against Stark by the ministry after it found out that even strong supporters of Stark would not like to have him in their own laboratory.⁴⁹

When the ministry communicated that it considered the negotiations with Paschen to have failed, at the same time it explicitly asked for opinions about two other candidates not mentioned by the Göttingen faculty. This is an interesting move that demonstrates how differently candidates might have been chosen. In proposing Wolfgang Gaede from Freiburg, the ministry shows that it was by no means clear by 1915 that quantum physicists were the superior choice. Proposing the Berlin *Privatdozent* Robert Pohl, who had explicitly been excluded by Wien, it indicates that the close interaction of theory and experiment was not necessarily seen as worth preserving.⁵⁰

Entering a Quantum Condition

On the new list of July 1915, Robert Pohl was rated second, Edgar Meyer first and Peter Paul Koch third. When the dean of the Göttingen faculty reported on the selection procedure and the candidates' talks, which could only be arranged with difficulty, all three physicists were characterized as being interested and competent in the field of quantum or atomic physics. As the candidates should represent "pure physics in the sense of W. Weber and E. Riecke" and show off to advantage "highly topical fields of molecular physics and radioactivity, especially the ones so successfully pursued by the latter," Meyer qualified through his work on "radioactive oscillations," which allowed the number of atoms per mole unit to be determined, and he won the lead through his work on the photoelectric effect, which allowed him to "penetrate even more deeply into atomistic phenomena."⁵¹ Pohl,

⁴⁸Document 11.

⁴⁹Cf. Rubens' attitude in Document 13.

⁵⁰Elster to Curator, 2 July 1915, GStA PK Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 181.

⁵¹Dean to ministry, 18 December 1915, GStA PK Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 348–354: The list consisted of 1. E. Meyer—Tübingen, 2. Pohl, P. P. Koch—Munich, while Simon was not included. Instead, applied and technical physics was preferred (p. 350). "reine Physik im Sinne der Tradition von W. Weber und E. Riecke,—spezieller noch die von dem letzteren so erfolgreich gefertigten und hoch aktuellen Gebiete der Molekularphysik und Radioaktivität—zur Geltung zu bringen." With respect to Meyer: "Nachweis der [...] radioaktiven 'Schwankungen' und der darauf gegründeten Zählung der Atome im Grammatom", "experimentell Erbrachte Entscheidung zwischen zwei Ansichten über die Natur der γ -Strahlen," "Noch tiefer in die atomistischen Einzelercheinungen einzudringen gelang ihm dann im Gebiete der lichtelektrischen Effekte."

for his part, was said to have a “gift for precision physics,” was seen to have opened with his work on the selective photo effect “a new avenue to the solution of questions about the structure of atoms that are currently in the center of interest.”⁵² Even Koch, who according to the judgment of his teacher Sommerfeld was neither theoretical nor mathematical physicist, “but only a brilliant physical technician,”⁵³ was considered due to his work on specific heats and the Zeeman effect, although the Göttingen candidate Simon (like Max Wien and Jonathan Zenneck) was disqualified for being a “technical physicist” instead of a “pure” one; as to Gaede, only doubts of his appropriateness for the position were mentioned.⁵⁴

At this point one can safely state that a shift in focus towards quantum physics took place, and with it a reorganization process of the Göttingen physics staff. The fact that—as a consequence of all the redefinition and trading of positions—Riecke’s chair of experimental physics became Born’s theoretical one, while Voigt’s theoretical chair became Franck’s position in experimental physics, appears to be more than a curiosity. Rather, it hints at evidence of strong dynamics on the level of staff, or personnel resources, which suggests a closer relationship with the disciplinary constellation and with the various visions and plans that key players may have tried to realize.

As the Göttingen faculty was not interested in Stark, he tried in vain to succeed to Riecke’s position after 1914, causing a “bitter dispute” that lasted until 1917 (Hermann 1970).⁵⁵ It was finally mainly up to Hilbert and Debye to fill the vacancy, who considered Pohl and Franck and decided for Pohl, because he promised to be a better lecturer.⁵⁶ Although things are less uniform for the “younger staff” that was considered for the associate professorship in 1915/16, a clear guideline for the arguments can still be made out. Edgar Meyer, Robert Pohl, and Peter Paul Koch cannot count as quantum or atomic physicists to the same degree as Debye or Born. The reasons that were given for their proposal, however, seem suggest the opposite.

One can leave aside the question of the extent to which the above characterizations included wishful thinking, and to what degree these candidates could be called quantum or atomic physicists, the central point is how the Göttingen faculty argued. Interestingly, a closer look at the lists of candidates for the main physics chairs, especially at times of reorganization in 1914 and 1920/21, shows that researchers in the field of quantum physics,

⁵²“Begabung für Präzisionsphysik,” Beugung von Röntgenstrahlen, selektiver Photoeffekt “der einen neuen Weg eröffnen dürfte zur Lösung der gegenwärtig im Mittelpunkt des Interesses stehenden Fragen nach der Struktur der Atome,” guter Vortrag.

⁵³Sommerfeld to Wien, 1 June 1916, DM Wien Papers, box 010. “Auch Wagner ist theoretischer Physiker, nur nicht die Spur mathematischer Physiker. Koch ist wohl beides nicht, sondern nur glänzender physikalischer Techniker.”

⁵⁴On Koch: “Präzisionsphysik im Sinne Röntgens,” spezif. Wärme, Zeeman-Effekt;—Zweifel an: Füchtbauer—Leipzig, Gaede—Freiburg, Ladenburg—Breslau, Franck—Berlin.

⁵⁵For Voigt’s views on Stark, see his review of Stark’s book on “Elektrische Spektralanalyse chemischer Atome,” Leipzig 1914, (Voigt 1915c). Göttinger gelehrter Anzeiger 1915: 7/8, 500–504. “Das Einzige, was bleibt, und was für Leser, die Starks eigenartige wissenschaftliche Persönlichkeit aus seinen Originalabhandlungen nicht zuvor kennen gelernt haben, ist der Einblick in dessen eigne Vorstellungswelt. Wie bei anderen experimentell ungewöhnlich begabten Forschern ist in Stark das Bedürfnis nach lebendiger innerer Anschauung der von ihm behandelten physikalischen Vorgänge äußerst ausgeprägt, ein Bedürfnis, das sich zur Not auch mit einem Modell befriedigt, das nach manchen Seiten hin physikalisch unmöglich erscheint, wenn es nur nach der einen ihm momentan wichtigsten Seite hin eine Deutung des Vorgangs anbietet. [...] Stark ein Experimentator erstern Ranges [...]” p. 502.

⁵⁶Debye interview 1962 p. II/5 (AHQP). It appears that Hilbert had convinced Debye of Stark’s inappropriateness. “[...] Hilbert insisted that that as a *völkisch* nationalist and outspoken anti-Semite, Stark was simply unacceptable for Göttingen, and after he apparently persuaded Peter Debye of this, no one was prepared to argue otherwise” (Rowe 1992, 502).

particularly on the first list, and of atomic physics and also on, particularly on the second, were not only first, but often the only choice. No case was found where advocates of the quantum had to compete with scientists from other research fields; classical fields like those Gaede stood for, and which had been still represented by Riecke and Voigt, had ceased to play a role.

The by this time long-standing wish to replace Riecke by Wilhelm Wien can be seen at least as a sign for a turn towards accepting quantum physics, for Wien's name and work was inseparably linked with the quantum. As all candidates of the list from December 1914 were important figures in the development that finally led to quantum mechanics—Wien, as already mentioned, Paschen as his competitor and the experimental alter ego of Sommerfeld and his theory, Stark and Zeeman as those who raised central problems of the quantum description of atoms—, it is fair to assume that candidates in this field were chosen intentionally. And Debye was clearly a representative of quantum physics, whose appointment was meant to strengthen this particular field in Göttingen. He engaged in quantum research both theoretically and experimentally. When he left and Born and Franck took over his duties in 1921, it had to be seen as a continuation of a research program rather than the onset of a new one.

By 1915 work on quantum physics and interest in the structure of the atom had become practically indispensable for candidates of the Göttingen physics faculty. The shift of interest away from Voigt's and Riecke's research programs, and in particular from their pessimism about quantum theory, was already complete at this point. Neither Riecke's wish for a "fresh person" nor Voigt's request that the physics institute under Debye should live up to the standards set by Franz Neumann in the middle of the 19th century⁵⁷ give any indication that they had any influence on this shift of focus. Who else could have? The "personal" full professor Wiechert may have to some extent, the applied physicists Simon and Prandtl definitively were not the only ones left. Hence, we cannot but consider the mathematicians.

⁵⁷Voigt to Naumann, 20 May 1914, GStA PK Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 71–73. "[...] der Universität ein theoretisch-physikalisches Institut zu schaffen, das im Sinne des Ideales meines Lehrers Franz Neumann die breiteste Berührung zwischen Theorie und Beobachtung vermittelt."

5.3 “Hilbert und die Physik”—Vision and Resources

Klein’s agenda for an applied mathematics materialized at the beginning of the century in the *Göttinger Vereinigung zur Förderung der angewandten Physik und Mathematik*, a group of professors and industrialists that tried to promote applied sciences, and which raised considerable financial means for this aim.⁵⁸ This gave Klein a major influence on the appointments and institutional resources of Runge, Simon, Prandtl, and to a lesser extent also Wiechert. Except for the last of these men, none of the scientists mentioned is of greater relevance for our question. Klein himself, the influential science organizer and adviser, did not entertain any interest in the quantum, and besides this he lost most of his influence after he fell ill in 1911. Therefore simple reasoning by exclusion suggests that it may have been David Hilbert who took over Klein’s role on the new stage of opening mathematics for fields like quantum physics and atomic theory. To give further indications for the validity of this thesis one can quote from Debye’s recollections, which point to Hilbert’s vigorous engagement. When it came to the choice between the two young experimental physicists Franck and Pohl, Debye remembered: “I told Hilbert ‘Well you’d better look at them.’ So I went with Hilbert to Berlin and we went to Ruben’s laboratory—didn’t say why, you see—and met both of them.”⁵⁹

Max Born’s article on “Hilbert and physics,” written to commemorate his sixtieth birthday, is perhaps the best account of Hilbert’s physics activities, as it describes and assesses his interests and contributions to physics at a time when the quantum riddle was still largely unsolved and nobody would have anticipated that eigenvalues, integral equations and infinitely dimensional spaces, later known as *Hilbert spaces*, would become the tools for its solution. Though Born did not mention that Hilbert had studied some physics at Königsberg, and even left out the sixth problem of his 1900 Paris address, he described very clearly what he had witnessed: the 1905 seminar on electron theory “that withdrew the two befriended mathematicians from their actual field”;⁶⁰ and after Minkowski’s death, the application of integral equations to the kinetic theory of gases and to elementary radiation theory. He also pointed to at least two different phases in which Hilbert had dealt with established physical theory. In the first he attempted to improve its foundations, while in the second he then considered “modern” physics like statistics and quanta. Born also mentioned, that while for the latter there was no materialization into published work, on other topics he did have great local influence on students and faculty. Part of this influence is found in Hilbert’s radiating verve: “The almost youthful fervor with which Hilbert entered the completely new fields of radiation theory and quantum hypothesis, thoroughly new to him, had something tremendously inspirational for everybody who witnessed it.”⁶¹ How did this come about?

Hilbert’s views on the development of mathematics and his axiomatic program prompted him to look at physics, most prominently demonstrated by the sixth problem of his 1900 Paris speech and his 1905 lecture course on the “logical principles of axiomatic

⁵⁸For the aims and successes of the Göttingen Association for the promotion of applied physics and mathematics see (F. Klein 1908) and (Tobies 1991).

⁵⁹Interview Debye 1962, p. II/5 (AHQP).

⁶⁰“Die Anregung zu diesem Unternehmen, das die beiden befreundeten Mathematiker von ihrem eigentlichen Arbeitsgebiet abzog und sie zum tieferen Eindringen in die Nachbarwissenschaft veranlaßte, mag damals von Minkowski ausgegangen sein [...]” (Born 1922, 88).

⁶¹“Das geradezu jugendliche Feuer, mit dem er in die für ihn ganz neuen Gebiete der Strahlungstheorie und Quantenhypothese hineinstürmte, hatte für alle, die es miterlebten, etwas ungeheuer mitreißendes” (I. Runge 1949, 149).

thinking,” in which theories of mechanics, thermodynamics, electrodynamics and even psycho-physics were used as examples.⁶² Hilbert’s collaboration with Minkowski was on the application of the axiomatic method to rich examples of theories that were essentially considered finished by their respective fields. As Leo Corry observed: “In Hilbert’s view, the definition of systems of abstract axioms and the type of axiomatic analysis described above was meant to be conducted retrospectively for ‘concrete’ *well-established and elaborated* mathematical entities” (Corry 1997, 115). Physics, as the “mother of all sciences,”⁶³ offered the best source of examples for the axiomatic method around 1905.

Hilbert’s later writings on the axiomatic method, however, show a strikingly different attitude. In his private notebooks there is repeated emphasis on understanding the axiomatic method in the correct way and in particular against the need of having a fully developed theory before axiomatics could be applied.⁶⁴ By 1917 the axiomatic method had matured into an axiomatic thinking. It is in this new sense of a “leading part in the sciences” that mathematics has its “calling.”⁶⁵ Born described Hilbert’s method as a “methodological requirement,” which guides theory construction rather than reconstruction (Born 1922, 91).

With regard to this general turn of perspective in the axiomatic method, it may suffice here to point to four basic factors that contributed to this turn. First of all, Hilbert was following Poincaré’s lead. The Frenchman had addressed the problem of discontinuity, which Lorentz had raised in 1908, in his last papers written shortly after participating in the first Solvay congress in 1911. He concluded that “[...] the physical phenomena cease to obey laws expressible by differential equations and this will be, without any doubt, the greatest revolution and also the deepest one that natural philosophy has gone through since Newton.”⁶⁶ The leading mathematicians Hilbert and Poincaré, though in competition from time to time (e.g. on geometry or integral equations), were always cordial to one another and both became proponents of quantum theory. Upon Poincaré’s death Hilbert might have felt an obligation to continue Poincaré’s ideas and probably saw an opportunity to bring Göttingen into play.

A second factor was the advent of atomic models like Thomson’s, Rutherford’s, and Bohr’s, which extensively employed new rules and hypotheses in a way that suggested improvement by axiomatic formulation. Third, quantum theory in general was implemented into physics as a kind of additional, axiomatic hypothesis. The axiomatist, therefore, clearly saw the all-important need to avoid contradictions, which could falsify the entire framework. And finally, even general relativity and Mie’s related theory gave rise to an attempt

⁶²This part of Hilbert’s work has been widely discussed in the literature. A comprehensive recent account was given in (Corry 1997). Here the 1905 lectures are discussed in detail.

⁶³Hilbert to Sommerfeld, 29 July 1906, DM, Archive HS 1977–28/A, 141. “Hilbert freut sich über den Ruf von Arnold Sommerfeld nach München; falls Wiechert den Ruf angenommen hätte, sei AS als Wiecherts Nachfolger in Göttingen in Frage gekommen; in München sei AS ganz bei der Physik, der ‘Mutter aller Wissenschaften,’ in deren Schoß er sich glücklich fühlen werde.”

⁶⁴In Hilbert’s undated private notebooks, section “Allgemeine Mathematisch-Philosophische Bemerkungen,” Hilbert Papers, 600:3. “Ich protestire gegen den Einwand, die Physik sei noch nicht weit genug, zur Axiomatisierung. Jede Wissenschaft ist zu jeder Zeit nicht nur reif genug sondern erfordert mit Notwendigkeit die Axiomatisierung, diese im richtigen Sinne verstanden.”

⁶⁵Ibid. “In dem Zeichen der axiomatischen Methode erscheint die Mathematik berufen zu einer führenden Rolle in der Wissenschaft.”

⁶⁶“[...] les phénomènes physiques cesseraient d’obéir à des lois exprimable par des équation différentielle, et ce serait là, sans aucune doute, la plus grande révolution et la plus profonde que la philosophie naturelle ait subi depuis Newton” (Poincaré 1912, 626).

for an axiomatic treatment by Hilbert, which he communicated in papers and lectures on the “foundations of physics” (Majer and Sauer 2009).

Hilbert’s turn coincided with a turn in quantum theory, which shifted from considering the problems of black-body radiation to the application of the quantum to atomic structure.⁶⁷ As Arthur Haas’ physical model for Planck’s resonator, which employed Thomson’s atomic model, stood at the beginning of this process (besides ideas of Thomson himself and Nicholson), its discussion by Lorentz in his 1910 Göttingen lectures may be seen as a further ingredient in the Göttingen perception that the quantum was becoming relevant to atomic constitution. In this way, quantum physics already became popular in Göttingen circles even before the 1911 conferences in Karlsruhe and Brussels put the topic more visibly on the agenda. As the historical analysis of early quantum theory has used publication analysis to show “that by 1911 and 1912 the quantum had entered physics to stay and that a major reconstruction of classical theory had become inevitable,” with Hilbert’s example we have found a case that demonstrates how such a shift in focus towards quantum problems can be found and explained on a local level.⁶⁸

It is instructive to recognize that the case of the development of quantum theory differs to a great extent from that of relativity, which was largely taking place at the same time. Besides Einstein, Hilbert, too, contributed to the successful formulation of general relativity in particular in 1915 (Hilbert 1915; Sauer 1999). However, while from Hilbert’s perspective the hopes to find new mathematics lay in the field of quantum problems, the mathematics used for general relativity appeared to him as of 19th-century origin. As Debye recollected, Einstein had to learn the right mathematics in order to succeed with his theory; for mathematicians, and in particular for Hilbert, “everything was finished with Minkowski,” since the mathematics of general relativity “was something quite common [gang und gäbe] [...] for mathematicians it was nothing especially new.”⁶⁹ Accordingly, Klein had also tried to subsume general relativity in his Erlangen program (Frei 1985, with comment on p. 142). As a consequence, the development of general relativity—as important it may have been for the conceptual progress of physics—appears surprisingly neutral with respect to the question of the impact the programs of mathematicians might have had on the local development of physics in Göttingen.

5.3.1 Hilbert Investing in Physics—Personal Resources

The central question of this study concerns the effects that occur when scientific interest becomes transformed into action with respect to resource allocation and research politics. The development of Hilbert’s strong interest in recent physics and its resonance in the mathematics community will be complemented in the following with the analysis of the recruitment and use of assistants, the teaching and training of doctoral students, the discussion of physics topics that took place in the *Mathematische Gesellschaft*, with Hilbert’s lecture topics, and with the spending of additional funds, in particular the Wolfskehl prize money. All of these fields can be understood as personal resources, since they constitute assets on which a person can exert influence or from which he or she can gain knowledge. This can happen either through one’s own decisions about what to work on, by delegating or directing work into

⁶⁷For the following cf. (Kuhn 1978a, 206–232).

⁶⁸Cp. (Lorentz 1910, with annotations by Born, esp. p. 1252; Kuhn 1978a, 228).

⁶⁹Debye Interview 1962, p. II/13 (AHQP).

specific directions, by organizing forums for information exchange or even by sitting on a committee that distributes funds. In this way I will try to assess the role of mathematicians in the reorientation of physics research. I will ask more directly to what extent David Hilbert became a kind of *Ersatz* physicist, albeit only temporarily, when he was both filling a vacuum in advocating for modern physics and pursuing genuine aims of fostering modern mathematics.

The most obvious resources for Hilbert's research were his assistants, with whom he often established an intimate working relationship, mostly at his home. Some of them digested and reported progress in mathematical and physical matters to Hilbert in regular meetings, others took notes of his lectures and revised them with their master later in the day; Hilbert preferred to talk science, rather than to read it. Although the assistants followed individual lines of research, most of them were closely related to their master's interests. It is revealing to see the extent to which mathematics and physics assistants were distinguished, both financially and with respect to their importance. Table (5.3) collects the available information reconstructed from the files of the Ministry of Culture and from correspondence.⁷⁰

First of all, it turns out that Hilbert had various types of assistants. We find the unpaid *Privatassistent* like Max Born, who did not depend on financial support but benefited from personal contact with his master while discussing the lecture notes to be made available in the mathematical reading room.⁷¹ Similar was the case of Paul Ewald, who started his studies with chemistry at Cambridge but turned quickly to mathematics at Göttingen. He, however, was paid privately by Hilbert.⁷² Next there is the group of regular assistants who, after having taken their doctoral degrees, were paid moderately from funds provided by the state. This was the case for Ernst Hellinger, Alfred Haar, Richard Courant, Wilhelm Behrens and Erich Hecke. The latter may also fit into a group of young up-and-coming professional academics who were assistants in order to receive some support, while otherwise working as unpaid lecturers (*Privatdozent*). In general, one observes an interesting pattern of maturation in Hilbert's assistants over time.

⁷⁰“Allgemeine Angelegenheiten des mathematisch-physikalischen Seminars 1869–1923,” GStA PK 76, Nr. 591, pp. 176–300, in particular Hilbert to Elster, 8 March 1907 (182–183v), and Hilbert to Wende, 17 February 1920 (260–262).

⁷¹Born had regular contact with Hilbert from summer 1904 on and prepared lecture notes. Due to family relations to Minkowski he was more friend than student of both. From 1905 on he was appointed *Privatassistent*. Due to the relative wealth of his family there was no need to get a paid position, in contrast to e.g. Hellinger, who depended on support. Cf. (Born 1975).

⁷²According to Reid(1970, 108f), Ewald served as “a special *Ausarbeiter* for a large class” and received a low salary from some non-mathematical funds Hilbert had acquired.

| | Mathematics assistants | Pay/yr. | Physics assistants | Pay/yr. |
|---|---|-------------------|------------------------------------|-------------------|
| (SS 04, WS 04) ^a from SS 05 | Born Privatassistent | — | | |
| WS 05 | Hellinger | 900 ^b | | |
| SS 06, WS 06 (WS 06) | Ewald | ? | | |
| SS 07, WS 07 | Haar | 900 | | |
| SS 08, WS 08 | | | | |
| SS 09, WS 09 | Courant (1910: Dr.) | 900 | | |
| SS 10, WS 10 | | | | |
| WS 10 | Behrens | 1200 | | |
| SS 11, WS 11 | Hecke (Dr.) | 1200 | | |
| SS 12, WS 12 | Hecke (PD) | 1200 | Ewald (Dr.) | 800 ^c |
| SS 13, WS 13 | Hecke (PD) as 1st assist. (Baule) | 1200 | Landé as 2nd assist. | 800 |
| SS 14, WS 14 | Hecke (PD) as 2nd assist. | 1200 | Landé (Dr.) as 1st assist. | 1200 |
| SS 15, WS 15 | Krafft (PD) | 1200 | | |
| SS 16 (part) | | | | |
| SS 16 (part) | Bär (Dr.?) | 1200 | | |
| WS 16, SS 17 | | | | |
| WS 17 | Bernays (Dr.) | 1200 | | |
| SS 18, WS 18 | | | | |
| SS 19, ZS 19 | Bernays (PD) | 1200 | Baule (Dr.) SS, Sponer (ZS, WS) | 1200 |
| WS 19 | | | | |
| SS 20, WS 20 | Bernays (PD) | 2400 ^d | Kratzer (Dr.) | 3000 ^d |
| SS 21, WS 21 | Bernays (PD) | 10,000 | E. Hückel (Dr.) | 8400 |
| SS 22 ... 1927 after 1927 | Bernays (a. pl. Prof.) Bernays, Schmidt, Gentzen | (16,000) | Nordheim (1923: Dr.) Wigner | (16,000) |

Table 5.3: Hilbert's assistants and their pay 1904–1930, compiled from various sources.

^aPeriods of no official appointment but collaboration in annotating lectures etc. are given in brackets. SS = summer term, WS = winter term, ZS = special additional terms after World War I.

^bHilbert supplemented M 300 to the M 600 provided by the ministry for SS and WS 06.

^cAccording to (Reid 1970, 130), M. 50 per month only. No evidence was found for this fact or for the doubling of this sum also mentioned upon a visit by Hilbert to the ministry in its files.

^dSums appropriated; Hilbert requested M 3000 for Kratzer and 2–3000 for Bernays; later payments adjusted for inflation differed.

The physics assistants can also be accommodated in this scheme. They appear as a second regular type of assistants, mostly postdoctoral researchers of different backgrounds. Ewald initially came from chemistry and had first turned to mathematics (under Hilbert) and then to physics (under Sommerfeld in Munich). Landé, who started as an experimental physicist like Erich Hückel, had turned to theory.⁷³ Furthermore, there were additional students and researchers of comparable standing close to Hilbert and his work. In this group one might also put Otto Toeplitz, who had collaborated a great deal with Hellinger on a theory of infinite-dimensional matrices, a topic Hilbert had raised and which became a central resource in the formulation of matrix mechanics,⁷⁴ and Hermann Weyl, who did some mathematical work related to black-body radiation.⁷⁵

As regular paid positions existed only for the the applied branches of mathematics, represented by Runge and Prandtl from 1905 on, and for the collection of mathematical models or the mathematical reading room, strictly speaking, none of Hilbert's assistants belonged to the personnel of the university as it was listed in the university catalog. They were assistants to Hilbert only, and they had been granted by the ministry *ad personam*. Some of them, however, came from one of the institutional assistantships or received one later. For example, the position of "assistant of the collections of mathematical instruments and models," was held, among others, by Hellinger (winter term 1907 to winter term 1908), Hecke (summer and winter term 1910) and Behrens (summer and winter term 1911). Hertha Spöner, in turn, came to James Franck after having been Hilbert's physics assistant, as did Erich Hückel who then worked for Born, thus again exhibiting the close relations between physics and mathematics researchers on the assistant level.⁷⁶

The table of assistants reveals another, rather different kind of feature of Hilbert's recruiting politics. It strikes the eye that, at least before World War I, a quite homogeneous group of assistants was selected by Hilbert as regarded age, geographical and religious provenance. Max Born (born Breslau 1882, school and studies there), with Breslau-born or educated Toeplitz (1881 Breslau, school and studies there), Hellinger (1883 Striegau, school and studies at Breslau) and Courant (Lublinitz 1888, school and studies at Breslau), recalled that "it was quite natural that we four from Breslau constituted a group and that the others recognized us as a group. Particularly Hilbert did so very consciously" (Born 1975, 138).⁷⁷ This group of central European Jews, which was as the first, or at most the second, generation with access to academic careers in certain fields, especially mathematics, also included Alfred Haar (Budapest 1885) and to lesser extent Paul Peter Ewald (Berlin 1888, Jewish wife and ancestors).⁷⁸ The impact of Jewishness on career paths and membership in Göttingen groups, and in scientific fields, methods and problems—as superposed to the dis-

⁷³Hückel was later also assistant with Born and Debye.

⁷⁴Toeplitz took over a professorship in Kiel in 1913 but continued work with Hellinger, cp. (Toeplitz and Hellinger 1906; 1910; 1927).

⁷⁵Weyl, however, was strictly addressing a mathematical audience, cp. (Weyl 1912b; 1912a; 1913).

⁷⁶Cf. *Amtliches Verzeichnis des Personals und der Studierenden der Kgl. Georg-Augusts-Universität zu Göttingen*.

⁷⁷"[...] es war ganz natürlich, daß wir vier aus Breslau eine Gruppe bildeten und das die anderen uns als Gruppe anerkannten. Vor allem Hilbert tat dies ganz bewußt." Born, Courant and Hellinger were inspired by the same teacher of the Breslau König-Wilhelms-Gymnasium, cf. (Staley 1992, 29–36).

⁷⁸One could probably define a second group of north German Jewish mathematicians and physicists who were influenced and considerably supported by Hilbert, including among others Hermann Weyl, Alfred Landé and Paul Hertz. One might also wonder why Paul Ehrenfest, although present at Göttingen for a considerable time, did not find entry to this congenial group; he was called a "representative of the Göttingen school," see (Ioffe 1967, 42).

ciplinary development of the mathematical sciences—cannot be neglected. In this respect Göttingen was a particularly welcoming place, although there were limits (Rowe 2009).⁷⁹

As the result of a meeting in January 1912 in the Berlin ministry, Hilbert was now able to appoint two assistants instead of one from summer term 1912 on, one of them specifically for physics. This was a concession to keep Hilbert in Göttingen, who had received a number of offers from other universities, also from abroad. For recruitment Hilbert collaborated with Sommerfeld and a typical Göttingen-Munich-Göttingen exchange was established.⁸⁰

Ewald was Hilbert's *Privatassistent* and shared a flat with Born, von Kármán and other students of Hilbert during his first Göttingen period. He was taking notes on one of Hilbert's lectures and perfected them under his direction and, in this way, was part of the distinctive Göttingen crowd, which was particularly open for new mathematics and its neighboring fields (Kaiser 1970; Born 1975; Hilbert 1907). After completing his doctorate with Sommerfeld in Munich on "the dispersion and birefringence of electron lattices (crystals)" (Ewald 1912), he returned to Göttingen with an option for habilitation.⁸¹ When he returned to Sommerfeld a year later, he would be replaced by Alfred Landé, himself a good example for collaboration by sharing students: After having studied in Marburg and Munich for two years, he continued in Göttingen from 1910 to 1912 and began his dissertation under Sommerfeld in Munich thereafter, but went as assistant to Hilbert in spring 1913. He presented his dissertation "On the method of eigen-oscillations in quantum theory" (Landé 1914), which was published in Göttingen, to the Munich faculty in the middle of his Göttingen period in 1914.

The war made him leave his Göttingen position, however, as he wanted to do his duty as a paramedic in the Red Cross, reportedly to some annoyance on Hilbert's part.⁸² Landé's case offers a good indication of what Hilbert wanted. In an interview Landé recalled "I went to Göttingen already a convinced quantum theorist." and described his duties: "Every morning and afternoon I had to report to Hilbert on new literature in quantum mechanics [NB: quantum physics is meant], on ideas about the behavior of solid bodies at low temperature, on spectroscopy, and the like."⁸³

The fact that a mathematician spends part of his financial means on the borderland to a neighboring discipline, rather than for central mathematical questions, may not be too surprising. Hilbert's actions went much further, however. Not only did he employ researchers

⁷⁹Born was not able to get a position for Otto Stern, with whom he had collaborated closely in Frankfurt before his return to Göttingen in 1921, neither in Frankfurt nor in Göttingen.

⁸⁰Hilbert to Naumann (*Ministerialdirektor* in the Prussian Ministry of Culture), 10 March 1912, GStA PK Rep. 76, Nr. 591, Bl. 204. "Sodann haben Euer Hochwohlgeboren mir—gelegentlich meines Besuches Anfang Januar—einen zweiten Hilfsassistenten zu 800 M. für meine mathematisch-physikalischen Studien bewilligt. Ich habe dafür einen Schüler von Sommerfeld—München gewonnen, nämlich Dr. Ewald—München, [...]" Since nothing can be found about this meeting in the papers of the ministry, one can only speculate that Hilbert used, as he frequently did before, offers of positions elsewhere to improve the Göttingen situation. (At this time Carl Neumann retired from his Leipzig chair, the position was ultimately not filled, but converted into an associate professorship for Göttingen *Privatdozent* Paul Koebe; among the other professors were Otto Hölder and Gustav Herglotz. Also, Philipp Lenard wrote to Sommerfeld on 25 September 1913 of a failed attempt to win Hilbert for new second chair of mathematics in Heidelberg. DM, Archive HS 1977–28/A, 1982).

⁸¹Hilbert to Naumann (*Ministerialdirektor* of the Prussian Ministry of Culture), 23 April 1912, GStA PK Rep. 76, Nr. 591, Bl. 208. "Ich bemerke noch, dass Dr. P. Ewald ein aufgebildeter theoretischer Physiker ist, der beste Schüler von Sommerfeld—München und künftiger Habilitand, der mir in Korrespondenzen und Besprechungen—er ist bereits hier in Göttingen—schon jetzt die besten Dienste geleistet hat."

⁸²Cf. (Reid 1970, 141); the letters from Landé to Hilbert of 1915, however, suggest a less dramatic account. Hilbert Papers, folder 207.

⁸³Interview Landé, 1962, session I, pp. 7 and 5 (AHQP).

of a standing the physics institute should have been striving for, they—and what may be even more extraordinary, even he himself, as we will see below—were dealing with genuine physics problems, work that could have earned them advanced degrees elsewhere. Moreover, the physics assistant was not only equal to the even more highly qualified mathematics assistant in financial terms, he also was ranked higher. Hilbert wrote to the Ministry in spring 1913:

For the forthcoming budgetary year I have employed my past 2nd assistant Dr. Landé (theoretical physics), who has become indispensable for the preparation of my theoretical physics lectures, as 1st assistant, and thus an increase in his remuneration from 800 Marks to 1200 Marks appeared necessary to me.

The mathematical assistant had only a narrow escape from financial demotion, as Hilbert cleverly proceeded

My past 1st assistant Dr. Hecke, *Privatdozent* for mathematics, will dedicate his assistant service to me one more year and thus shall become my second assistant. Since Dr. Hecke so far received 1200 Marks and gave invaluable service, I propose to appropriate for the forthcoming budgetary year as an exception 1200 Marks instead of 800 Marks, which is what my second assistant usually gets.⁸⁴

This exceptional instance shows a clear turn of Hilbert's investments from fostering mathematics to taking over physics.

In later years, and in particular in the period of World War I, great changes occurred with respect to the availability and tasks of the assistants. Here we find a second instance of inverted investment in Hilbert's request for remuneration of Paul Bernays and Adolf Kratzer in 1920. Hilbert wrote to the ministry that

Dr. Kratzer, up to now full assistant at Sommerfeld in Munich, shall become in future my mathematical physical help, of whom I expect very much. He told me that he could not come if paid less than 3000 Marks yearly salary [...]

As he also requests the same amount for Bernays, who was already *Privatdozent*. However, he concluded: "In any case I ask to appropriate for Dr. Kratzer at least 3000 Marks, for Dr. Bernays at least 2–3000 Marks for 1920/21."⁸⁵

In consequence, by analyzing the spending on assistants an indicator has emerged that points to a period between 1912 and c. 1920, in which physics carried more value to Hilbert than mathematics.

Finally, Hilbert's role as a substitute physicist is also obvious from the production of graduates in the field of mathematical physics (or physical mathematics), as the dissertations he supervised show. As a number of scientists who played an important part in physics later

⁸⁴Document 7.

⁸⁵Hilbert to Ministerialrat Wende, 17 February 1920, GStA PK Rep. 76, Nr. 591, Bl. 260–261. "Dr. Kratzer, bisher voller Assistent bei Sommerfeld - München, soll künftig meine math-physikalische Hilfe werden, von der ich mir besonders viel verspreche. Er erklärte nicht unter 3000 M. Jahresgehalt kommen zu können [...]" – auch Bernays Bezüge auf 3000 M erhöhen – "Jedenfalls bitte ich Herrn Dr. Kratzer mindestens 3000 M. " " Bernays " 2–3000 M. pro 1920/21 bewilligen zu wollen."

on did their doctoral work on pure mathematics before 1911,⁸⁶ between 1912 and 1921 five of ten dissertations were related to physics: atomic structure,⁸⁷ dilute gases,⁸⁸ electrolysis,⁸⁹ and even quantum theory.⁹⁰ Another one is also on philosophy⁹¹ and only a minority of four remained for genuinely mathematical topics.

5.3.2 Physics in the *Mathematische Gesellschaft*

A further indicator of non-monetary nature of the mathematician's investments in physics is found in the topics of the talks at the *Mathematische Gesellschaft* of Göttingen. This society was founded by Felix Klein and Heinrich Weber in 1892 and met every Tuesday evening. Similar societies existed in many university towns, though most of them were not official university institutions, but rather private-run societies or clubs.⁹² Although one should keep in mind the interdependence of the various resources analyzed in this section—for example, one might expect that the physics assistants like other advanced students and researchers of Hilbert's group would very likely become speakers in the *Mathematische Gesellschaft*—the differentiation is far from redundant.⁹³ Looking at the talks in the *Mathematische Gesellschaft* serves to exhibit the broader impact of an elevated interest in physics topics on the part of mathematicians in general, and by Hilbert in particular.

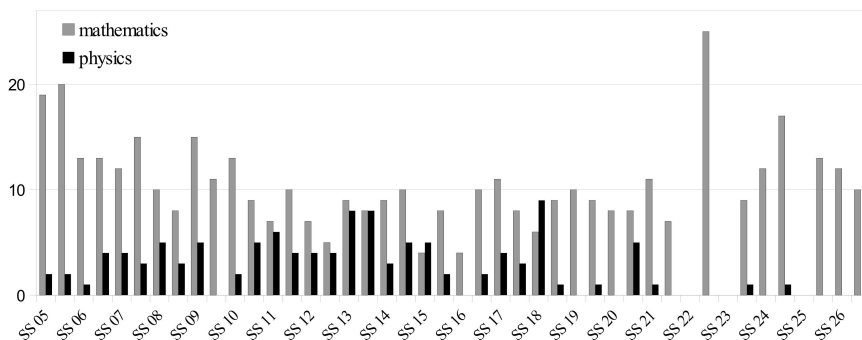


Figure 5.2: Number of talks in the *Mathematische Gesellschaft* on topics on mathematics vs. physics. Source: Lists of topics as reported in (*Jahresberichte DMV*).

⁸⁶E.g. dissertations supervised by Hilbert, written by Weyl on Fourier's integral theorem in 1908 and by Courant on Dirichlet's principle in 1910. Cp. "Verzeichnis der bei Hilbert angefertigten Dissertationen" in (Hilbert 1935).

⁸⁷Dissertation by Föppl in 1912 (viva voce 1 March 1912).

⁸⁸Dissertations by Bolza in 1913 (3 June 1913) and by Baule in 1914 (18 February 1914).

⁸⁹Dissertation by Schellenberg in 1914 (24 June 1914). The theses by Bolza, Baule and Schellenberg seem to have emerged from a seminar on *kinetic theories in physics* (summer term 1913), which Hilbert gave together with Hecke, in which Born, Hertz, von Kármán, and Madelung also took part. Cf. (Lorey 1916, 129), and table of lectures below.

⁹⁰Dissertation of Kneser in 1921.

⁹¹Dissertation by Behmann in 1918.

⁹²Cf. (Lorey 1916), esp. pp. 138–141 and 219–221.

⁹³Actually, Ewald spoke only once at the *Mathematische Gesellschaft* on his dissertation, on 4 June 1912, and Landé not at all; Born, on the contrary, spoke a dozen times between 1910 and 1915 but only once on a topic not related to physics; in this period Hecke gave three talks on mathematics and one on physics.

Interpreting the diagram (5.2) as an indicator for the spending of a special kind of resources, viz. attention, it shows a period of particular high interest in physical problems from 1910 to 1915 and in summer 1918. In summer term 1911, and from winter 1912 to winter 1913, the number of physics talks equaled or almost equaled that of mathematical ones, while in the summer terms 1915 and 1918 physics, in fact, outnumbered mathematics.⁹⁴

A good example of the role of the society in the exchange of news is the Göttingen discussion about the outcomes of the first Solvay Conference, which took place in Brussels in fall 1911 without participation from Göttingen. In two sessions on 25 February and 4 March, 1913, Born and von Kármán obviously gave an in-depth report on the recently published French version of the proceedings. As Born later published a brief report in *Physikalische Zeitschrift*, one can catch a glimpse of the Göttingen reception. Born staged the “Brussels quantum congress,” as he once called the meeting in *Naturwissenschaften* (Born 1913b), here as a fight between two groups or “parties,” the “quantum friends” and their foes. To a much greater extent than usual in the scientific literature, the individual personalities come to bear on the discourse, “for as long as the foundations of quantum theory are so barely clarified as today, the favorable or unfavorable attitude to the new theory depends significantly on personal preferences and views.” The party of the quantum friends, however, was dominant, while Lorentz stood above both directions.⁹⁵ In this way the Göttingen mathematics community was well-informed about the developments of the new quantum theory.

When in 1921, however, discussions on physics died down in the *Mathematische Gesellschaft*, only Born would return for single talks in winter 1923 and winter 1924, respectively, as after the advent of quantum mechanics, physics was no longer a topic. Ironically, Born’s last talk on 9 December 1924 was on “atom mechanics and other things,” but not on any actual advances, which occurred half a year later.⁹⁶ This incident points out a crucial fact about Göttingen’s history of quantum physics: ironically, the mutual interaction between mathematics and physics was reduced at precisely the height of the search for a quantum mechanics.

Regarding the relationship between physics and mathematics as academic fields in the university, the indicators analyzed demonstrate a certain kind of disciplinary imperialism by Göttingen mathematics. The question is whether what happened was actually a displacement of disciplinary borders, or whether it would be more accurate to speak of a mutual penetration or overlap of interest.⁹⁷ A conceivable test could be to compare the debates of the mathematical society with those of the physical society, which also existed and met regularly. Unfortunately, no records comparable to those for the mathematicians have been found: only occasionally are talks reported or can be reconstructed from notes and recollections. The sparse information available, however, does not suggest a symmetrical relation.

⁹⁴The summer 1911 physics talks were by von Kármán (hydrodynamics), Weyl (radiation, discussion of Lorentz paper), Born (X-rays), Wiechert (relativity), and Ph. Frank (statistical mechanics). The physics talks at the “plateau” of 1913 are mainly about radiation, quantum theory, atoms and atomic lattices, and relativity. In summer term 1915 talks by Einstein and Sommerfeld are to blame for the predominance of physics, as are those of Max Planck in 1918. Cf. (Jahresberichte DMV).

⁹⁵“[...]]; denn so lange die Grundlagen der Quantentheorie noch so wenig geklärt sind, wie heute, hängt die Stellungnahme für oder wider die neue Lehre in nicht geringem Maße von persönlichen Neigungen und Anschauungen ab” (Born 1914b).

⁹⁶Mentioned in (Jahresberichte DMV, 34, 1926, p. 103), “Atommechanik und anderes.”

⁹⁷The thesis that the border between mathematics and physics became permeable is attributed to (Sigurdsson 1991).

Neither did the physicists start to talk mathematics, nor did the mathematicians start joining them. Hilbert, for example, presented a paper on the problem of specific heats in the physical society in January 1913, which I will discuss below; here Hilbert carefully tried to adapt to the physicists style, for example, by comparing predictions with measurements and by giving explicit quantities.⁹⁸ Only few additional cases are known, which concerned atomic models. Debye discussed Rutherford's nuclear atomic model in spring 1914, while Born presented a paper "On the stability of Bohr's atomic model" in December of the same year.⁹⁹

5.3.3 Hilbert's Physics Teaching Instruction

In his article "Hilbert and physics" written on the occasion of the sixtieth birthday of his teacher, Max Born explained that one should look at Hilbert's teaching record in order to appraise his efforts in dealing with physics. For many years he had sacrificed much time and work to deal with quanta and statistics; however, "he never published his results, but introduced them to his students in his lectures."¹⁰⁰ To what extent Hilbert actually acted like a physicist can thus be assessed by looking more closely at his teaching at the university, but also by considering his communication with physicists, his research notes and even some publications which did in fact appear. The methodological reorientation of the axiomatization program towards topical physics problems turns out to be apparent in his lectures as well, most of which were carefully recorded by students, and used by generations of students in the library of the mathematics institute and which are preserved.

The question of the extent to which Hilbert's interest in physics was concerned with quantum problems in particular has to be addressed with respect to the fields and problems, which at this time were linked to Planck's constant. Quantum physics thus was mainly radiation theory and the discussion of physical phenomena related to some fundamental discontinuity—as the title of Thomas Kuhn's book captured precisely: "Black-body theory and the quantum discontinuity." This, in fact, characterizes well what actually interested Hilbert in his teaching, for example, in his summer 1912 lectures on radiation theory.¹⁰¹ However, the series of lectures on physics was longer, and those on radiation theory constituted just one step in an ongoing program.

As Hilbert wrote in his autobiographical sketch:

Decisive for my work was the closest possible connection between research and teaching. [...] It was my principle not to present in the lectures and all the more in the seminars generally accepted [eingefahren] and as evenly as possible polished knowledge [Wissensstoff], which would have made it easy for the students to keep clean notebooks. I have rather always tried to illuminate the problems and difficulties and to build a bridge to the topical questions. Not only on rare

⁹⁸Manuscript "Bemerkungen zum Nernstschen Wärmesatz" of talk at *Physikalische Gesellschaft* on 15 January 1913, Hilbert Papers, folder 590, Bl. 1–16.

⁹⁹Born's full paper can be found in the Hilbert Papers, Mappe 690, Nr. 1, Bl. 1–10, while there is only one page on Debye's presentation, Mappe 693, Nr. 2, Bl. 9.

¹⁰⁰"Auch Hilbert hat [...] seit seinem ersten Eindringen in die Gedanken der modernen Physik den Schwerpunkt der Fragestellung in den Problemen der Statistik und der Quanten gesehen. Mehrere Jahre hat er viel Zeit und Arbeit darauf verwandt, diese Gebiete mit der Schärfe seiner Logik zu durchdringen und erhellen; aber er hat seine Resultate niemals publiziert, sondern nur seinen Schülern in Vorlesungen bekannt gemacht; [...]" (Born 1922, 91).

¹⁰¹(Hilbert 1912c), published and commented by Arne Schirmacher in (Sauer and Majer 2009b, 435–501).

occasions it happened that during a semester the program of the subject matter [stoffliche Programm] of an advanced lecture course was essentially altered, because I wished to deal with topics that I just worked on as a researcher and that by no means had taken a definite form. Advanced lecture courses of this kind resulted in a close interaction with the listeners who on their part reacted with criticism and their own ideas.¹⁰²

Hilbert's 1911 lecture on mechanics, a standard subject in the mathematics curriculum as it may seem, is a good example of how early he extended the scope of his lectures onto physical ground. After sections on vector analysis, small deformations of continuous media, elasticity, and hydrodynamics, genuine physics topics take up more than half of the lecture notes. They are: thermodynamics, the theory of relativity as well as electrodynamics and the problems arising from their combination, viz. Born's theory of the rigid body (which already refers to atomism) and relativistic thermodynamics, which also deals with black-body radiation (Hilbert 1911). (Interestingly, for the same term Runge listed a course on mechanics as well, so that it may be concluded that the privilege to ignore the listed topic was mainly Hilbert's;¹⁰³ in addition, it was up to Voigt to deal with the mathematics necessary for physics students in a course on "partial differential equations in physics."¹⁰⁴)

While in summer 1911 Hilbert was mostly reporting about established knowledge and some recent work of others, one of Hilbert's "most remarkable" courses of lectures followed (Born 1975, 128). His winter 1911/12 lectures on the "kinetic theory of gases" are both the attempt to reformulate a well-established physical field with a unifying mathematical framework (integral equations) and the first step towards atomic and quantum physical questions (Hilbert 1912b). He eagerly tried to show how the mathematically more rigorous framing of physical theory gives rise to criticism and hence improvement of the physical theory itself. In his 1912 book on integral equations and their applications to physics he employs a traditionally sober tone; in his lectures, however, a more programmatic attitude is unmistakable (Hilbert 1912a). On the very first page of the introduction he makes his standpoint clear: the phenomenological point of view must be discarded because it has no unifying power, it merely fragments the whole of physics into many single chapters, each with its own principles.¹⁰⁵ Delivering these lectures to an audience of Göttingen mathematics and physics students and later spread in form of lecture notes, they criticized Voigt's well-known views and made clear that Hilbert did not see unification in Voigt's mathematical systematics.

Hilbert argued that on the basis of atomism and axiomatic formulation for the entire physics a better approach is guaranteed.¹⁰⁶ This one he wanted to follow in this lectures.

¹⁰²Document 17.

¹⁰³“Wenn Hilbert an solch einer neuen Sache arbeitete, pflegte er eine Vorlesung mit einem harmlosen Titel, etwa ‘Differential- und Integralgleichungen,’ anzukündigen und dann entwickelte er in diesem Rahmen seine neuen Ideen, oft mit revolutionären Konsequenzen” (Born 1975, 126); “[...] führten Hilberts normale Vorlesungen immer in Neuland” (Born 1975, 128).

¹⁰⁴Cf. Verzeichnis der Vorlesungen auf der Georg-Augusts-Universität zu Göttingen, summer term 1911 ff. Voigt regularly taught on differential equations and on vector analysis. Compared over a number of years, there is considerable overlap between Voigt's and Hilbert's listed courses on the same topics: mechanics, electron theory fundamentals of physics.

¹⁰⁵“(A) Man kann die Mechanik der Kontinua rein phänomenologisch behandeln. Die ganze Physik wird dabei in viele einzelne Kapitel zerlegt [...]”

¹⁰⁶“(B) Wesentlich tiefer eindringend kann man die theoretische Physik auf Grund der Atomtheorie behandeln. Hier ist das Bestreben, ein Axiomensystem zu schaffen, welches für die ganze Physik gilt. [...]” [Gastheorie, Strahlungstheorie]

The best approach, moreover, were a theory of the molecular structure of matter, which was just becoming understood, and which he announced to be the topic of a forthcoming lecture, which he actually delivered in winter 1912/13 as “molecular theory of matter.”¹⁰⁷ To highlight the programmatic character of the three approaches mentioned in his winter 1911/12 lectures, they are suggestively juxtaposed in table (5.4).

| A | B | C |
|--|--|---|
| “phenomenological” | “on the basis of atomic theory” | “a theory of the molecular structure of matter” |
| “all of physics is fragmented into single chapters: thermodynamics, electrodynamics, optics etc.” “each field builds on specific basic assumptions” “ <i>Notbehelf</i> ,” “a first step in understanding”, “urgently to be left behind in order to penetrate into the actual sacred objects [eigentlichen Heiligtümer] of theoretical physics” “partial differential equations” | “single point of view for all phenomena” “attempt to find a system of axioms valid for all physics” | “goes much beyond B” ? [deeper foundation, axioms = reality] |
| ? [Voigt] | mathematical method “entirely different,” e.g. probability calculus, “not yet fully developed” “this lecture” (summer 1911) | ? [new mathematics] “one of the next semesters” (winter 1912/13) |

Table 5.4: Hilbert’s three points of view.

Obviously, the notion of axiomatic treatment is reserved for a unifying structure that captures “all physics” and is put in parallel with atomistic theory. In this way atomistic reductionism of the physical reality, in Hilbert’s thinking, was probably to be related to logical reductionism on the level of theory.

The larger aim was a theory of molecular structure of matter, a theory that not only referred to atomistic models but actually allowed all physical properties to be deduced from something even deeper than the system of axioms for all physics sought in the approach (B). But what should this be? As this scheme illustrates, Hilbert left three slots open: There is no example given for the phenomenological approach A, which can easily be filled in with

¹⁰⁷In letzter Linie kann man das Hauptziel der Physik betrachten: die Theorie vom molekularen Aufbau der Materie. Dies geht noch erheblich über B) hinaus. In einem der nächsten Semester werde ich Gelegenheit nehmen, Ihnen das, was man heute von dieser Frage weiß, ausführlich darzulegen.” [WS 1912/13]

Voigt for our purpose. The other two empty slots for the best of all approaches concern Hilbert's motive for dealing with physics. The scheme may suggest the answers: After a "first step in understanding" by phenomenological means (A) and a successful axiomatization and "on the basis of atomic theory" (B), the objective was clearly to give a relationship between the basic notions employed in the axiomatization and the actual physical objects (C). As partial differential equations are the toolkit for phenomenologists, mathematics like probability calculus is the means of a convinced atomist. Thus in the realm C, aimed at people like Hilbert—who believed in a pre-established harmony in nature, there was the hope that advanced or novel mathematics would in some way reveal the identity of the axiomatic model and physical reality.

This specific conceptualization of the relationship between mathematics and physics, which defined a kind of physical mathematics Hilbert pursued for the next couple of years, was, in fact, established before he had acquired a physics assistant or a physics tutor. It seems that Hilbert initially planned to proceed with establishing a comprehensive theory of the axioms of physics. So, after having given a model by the integral equation treatment of the kinetic theory of gases,¹⁰⁸ he decided to deal with the theory of radiation and quanta in a similar way first, before turning to the general theory of physics axiomatization.¹⁰⁹

It was these lectures with the title "radiation theory" in summer 1912 where the first physics assistant Paul Ewald was put to good work. They had a similar scope as the "mechanics" a year before. The emphasis was on relativity, electrodynamics and in particular on black-body radiation. In the introduction Hilbert openly showed his new interest in "real physics [eigentliche Physik], which comes from the point of view of atomism," pushing the axiomatics of mathematics and of less topical physics like particle mechanics aside, since, encouraged by the news about Laue's experiment: "one can say that no time is more favorable and more challenged to examine the foundations of this discipline than today." This was due in particular to "the atomic theory, the principle of discontinuity, that emerges more and more clearly and is longer a no hypothesis, but like the theory of Copernicus, experimentally proven fact" (Hilbert 1912c, from the introduction, 1–2). Again, here we find all topics at hand that were related to quantum physics at the time!

The announced lectures on the "molecular theory of matter," however, fell short of the envisaged type C approach. As the reduction to molecular processes was still emphasized, a mixture of derivations by mathematical proofs and (phenomenological) definitions from experience was used to account for thermodynamic properties. The kinetic part dealt with ideal gases, the quantum hypothesis and the necessity to use mean values as well as the brand-new Born-von Kármán theory of specific heat. It seemed the interest to learn as much as possible about "real physics" dominated over his programmatic aims.¹¹⁰

In January 1913 Hilbert had virtually become a theoretical physicist. He even gave a talk in the Göttingen *Physikalische Gesellschaft* (although, as we have seen, the *Mathematische Gesellschaft* had almost as many talks on physics as on mathematics that year). His manuscript on the Nernst heat theorem can hardly be distinguished from a physicist's paper,

¹⁰⁸Planned title of lecture mentioned in Hilbert to Sommerfeld, 5 April 1912, "Prinzipien und Axiome der Physik," Document 1, announcement in *Mathematisch-naturwissenschaftliche Blätter* as "vierstündige Gratisvorlesung" on "Grundlagen der Physik."

¹⁰⁹"Allein schon die Fortsetzung, die von-der-Waals-sche Theorie bietet erhebliche Schwierigkeiten, auch ich habe mich daher entschlossen, zunächst die neuere Strahlungs- und Quantentheorie ähnlich weit zu führen" (Hilbert 1912c).

¹¹⁰The version of the lecture notes in the Born papers has more elaborations on quantum problems.

in particular as calculated values for transition temperatures were compared with measured ones.¹¹¹ He only omitted the next obvious step of publication, so that later only few recalled evidence of Hilbert's temporary conversion.

The year 1913 is the heyday of Hilbert's interest in physics, as is well documented in his lectures and papers. One should add the much publicized 1913 Congress in Göttingen and, as another, more subtle example, his letters to physicists who were working on quantum problems requesting new publications in this field. For example, he wrote to Einstein, asking for his papers on the theory of gases and radiation, and to Ehrenfest who sent his latest publications, albeit, fearing that they would not satisfy Hilbert's interests, "But I did not publish anything else on quantum questions this year," he wrote.¹¹²

Disciplinary boundaries had ceased to play a role, at least for Hilbert and Born. At the same time the latter had pushed forward a plan with Ferdinand Springer, who may have attended the *Gaswoche*, to establish a new journal of "physical mathematics," *Zeitschrift für physikalische Mathematik*, as correspondence from May 1913 shows. It was actually a project that had already been proposed some years earlier, but had been rejected, mainly due to objections by Sommerfeld; now it did not work out, either (Holl 1996, 52–54).¹¹³ However, it still exhibits the wide scope of Hilbert's effort to link mathematics and physics.

Hilbert's teaching continued, both to connect with modern theoretical physics in general and with his particular quantum interests, which were now related to statistical mechanics. In summer 1913 Hilbert's lectures show his interest in "real physics" topics like relativity, application of gas theory to electrons and electromagnetic radiation, all the way to new subjects like electronic motion in metals and modifications of Maxwell's equations. Unfortunately, in most cases the teaching and the presentations in the seminars cannot be reconstructed. For the summer term 1913 seminar, however, a report by the *Mathematischer Verein*, a mathematical student corporation, is known. Listed as *Kinetic theories in physics*, but reported as *Seminar on kinetic theory of gases*, Hilbert together with Hecke (not Landé!) let students prepare talks on dimensions and magnitudes in physics, ergodic theory, Brownian motion and its theory, Hilbertian theory of gases, dilute gases, the theory of chemical equilibrium, Sakur's papers, and so on. As participating discussants primarily Born and von Kármán are mentioned.¹¹⁴

As such Hilbert finally found himself under the influence of Ewald and Landé—Göttingen students and Sommerfeld disciples at the same time—and was now teaching

¹¹¹ Manuscript "Bemerkungen zum Nernstschen Wärmesatz" of Hilbert's talk at the *Physikalische Gesellschaft* on 15 January 1913. Hilbert Papers, folder 590, Bl. 1–16. Hilbert: "Eine physikalische Frage, die die Mathematiker besonders interessieren muss, ist die, wie sich die Körper am absoluten Nullpunkt verhalten, wie die Moleküle dort, vom Zustand der absoluten Ruhe ausgehend, die Wärmebewegung beginnen. Das Nernstsche Wärmethorem formuliert hierüber bestimmte Aussagen. Ich werde nämlich erstens einleitungsweise über den wesentlichen Inhalt des Nernsttheorems sprechen, sodann zweitens einige kritische Betrachtungen dazu vorbringen, und drittens werde ich einige Bemerkungen problematischer Art mitteilen, zu denen das Nernsttheorem den Theoretiker anregt." p. 1. "[...] Berechnung aus dieser Formel führt auf die Umwandlungstemperatur 369° bei Atmosphärendruck. Die Beobachtung ergibt 368°." p. 11.

¹¹² Hilbert to Einstein, 30 March 1912, (ECP5, 439). Reply Ehrenfest to Hilbert, 18 June 1913, Hilbert Papers, folder 91. "Ich übersende Ihnen gleichzeitig meine letzte Publikation. obwohl ich fürchte, daß sie nicht Ihrer Anfrage entspricht. Aber irgend etwas anderes über Quantenfragen habe ich in diesem Jahr nicht publiziert."

¹¹³ It seems unlikely that Springer discussed this idea only with Born and not with Hilbert. Holl suggests that three years earlier a similar idea had been put forward without the involvement of Born, who wrote to Springer on 29 June 1913 regarding the plan for such a journal that it "was as hopeless as three years ago; then Sommerfeld turned it down firmly and I am sure that he still holds this point of view." Cited after (Holl 1996, 54).

¹¹⁴ *Bericht des Mathematischen Vereins an der Universität Göttingen*, Sommer-Semester 1913.

genuine physics courses on the basis of molecular gas theory or crystal structure, like on the course “electromagnetic oscillations” in winter 1913/14 covering such topics as dispersion theory, the Zeeman effect and Planck’s formula. The last of these was a newly debated issue, since Einstein, together with Ludwig Hopf and with Otto Stern in 1910 and 1913, respectively, had been deriving the Planck formula without any quantum discontinuity using a simple atomic model. And Planck himself had also tried to redo his theory to eliminate non-classical behavior, a move Born commended, but not without commenting on Hilbert’s successes in clearing away difficulties Planck had “not fully overcome” (Hilbert 1914a, 117).¹¹⁵

As Born had mentioned in the *Vorbericht* to the 1913 Göttingen Congress, ideas had been advanced to explain quantum theory by statistical mechanics. This was probably one of the reasons for Hilbert’s seminar on statistical mechanics, which took place in summer 1914. It appeared to be a quite typical introduction to this subject. Its scope coincided roughly with the respective *Enzyklopädie* article written by Paul and Tatjana Ehrenfest in focusing on the principles of Maxwell(-Boltzmann) and Gibbs. The only unexpected part may have been the presentation of applications of Gibbs’ principle, including a quite concrete one on the dissociation of iodine vapor.¹¹⁶

In summary, one can say that by 1914, before the outbreak of the war, Hilbert no longer hid anymore his teaching in the neighboring. In a letter to the Prussian Ministry of Culture he even declared a change in priority, as he took his “2nd assistant Dr. Landé (theoretical physics) as first assistant, who has become indispensable to the preparation of my theoretical physics lectures and since it appeared necessary to me to raise his remuneration [...]”¹¹⁷

5.3.4 Spreading Hilbert’s Gospel: Seminars with Debye, Lectures by Born and Others

In the summer term 1914 two guest professors contributed to astronomy and physics teaching. Besides Alfred Haar, who had earned his doctorate from Hilbert back in 1909 and was now teaching theoretical astronomy, it was Peter Debye who directly entered the field of new physics prepared by Hilbert. His lectures on atomic models geared up the Göttingen scientists for Niels Bohr’s first brief visit in July 1914.¹¹⁸

As lectures that primarily addressed students were one arena of rather restricted interaction, seminars provided more room for exchanging of ideas. For example, Hilbert had listed a seminar in winter 1912 which comprised talks on the axioms of physics, while in summer 1913, as already mentioned, talks on kinetic theories in physics were presented.¹¹⁹ Hilbert’s lectures on statistical mechanics of the summer term 1914 had been accompanied by a seminar, as well, and at least part of this seminar dealt with molecular phenomena. Among the talks was also a contribution by Debye on electrical molecular momenta.¹²⁰ It was probably

¹¹⁵Cf. editorial comments in (ECP4, 270–273). (Kuhn 1978a; Born 1913a, 501).

¹¹⁶Cf. table of content and section III of notes prepared by Luise Lange, (Hilbert 1914b; P. Ehrenfest and T. Ehrenfest 1911).

¹¹⁷Hilbert to Elster, 6 April 1914, GStA PK 76, Nr. 591, Bl. 210.

¹¹⁸Cf. (BCW2, 331) and Bohr to Oseen 28 September 1914 cited there: “I gave a couple of short talks in the seminars in Göttingen and Munich and had many lively discussions.”

¹¹⁹Verzeichnis der Vorlesungen auf der Georg-Augustus Universität zu Göttingen.

¹²⁰Hilbert took notes on Debye’s talk “Elektrische Molekülmomente” of 4 May 1914, Hilbert Papers, folder 693/1; as for another paper of Weichert[?] “Über den Einfluß der Assoziation der Moleküle auf die Temperaturabhängigkeit der Dielektrizitätskonstanten,” 11 May 1914, Hilbert Papers, folder 719.

here where the famous series of seminars “on the structure of matter” were initiated, as they were called from the next term on. One does not need to believe the anecdote according to which Hilbert opened every session of the seminars on the structure of matter with the words “Tell me, just what is an atom?”¹²¹; however it is known that Born was presenting on the question of stability of the Bohr atom in December 1914.¹²²

Hilbert’s physics assistants were gone during the first year of the war. At the same time Born was offered a position at the University of Berlin. Hilbert, however, continued his mission as *Ersatz* physicist with “lectures on the structure of matter” that followed Born’s theory, which he had published in the same year as the latter’s “Dynamics of crystal lattices” (Born 1915). Previously, Born had taken over Hilbert’s role to bring new life to physics in summer 1914 with a course on “electron theory and the relativity principle.” It was a crash course on *atomistic theories of matter* and electricity that was written down on over 400 pages by a student (Born 1914a). In a similar manner, like Hilbert, Born pointed out that the topics “shall not be restricted to electron theory but moreover furnish the foundation for an atomistic theory of matter in the first place”¹²³ (Ibid.).

Finally, Hilbert’s 1915 lectures and seminar on the structure of matter demonstrated that Voigt’s field had been taken over by others in Göttingen. This might have been tolerable since the author of the enormous volume on *Kristallphysik* was to retire before summer 1915: however, the war, prevented him from going to Harvard as an exchange professor during his last term as planned; instead he taught on until his death in 1919.¹²⁴ The 1915 lecture notes of Hilbert’s course were taken under unfavorable war conditions and are much sketchier than usual. They appeared neither programmatic nor did they challenge Voigt’s *opus magnum*. Voigt in turn made it clear that his results of many years could not be so easily reduced to a mathematically appealing lattice theory, which he found in some respects “rather unconvincing.” Born’s models of displaced electron clouds, he still pointed out in 1918, could hardly account for the “immensely different behavior” of various crystalline substances (Voigt 1918).¹²⁵

From 1915 on, Hilbert reduced his work on questions related to quantum physics and the structure of matter, since he had entered into another physics project: general relativity. Starting from the recent ideas of Gustav Mie, he lectured in summer 1916 on “foundations of physics,” which essentially meant “the modern relativistic ideas” and continued these

¹²¹(Reid 1970, 140).

¹²²Max Born’s talks “Über die Stabilität des Bohrschen Atommodells,” 14 December 1914, notes in Hilbert Papers, folder 690.

¹²³“Der Gegenstand dieser Erörterungen soll nicht auf die Elektronentheorie beschränkt bleiben, sondern diese wird nur die Grundlage für die atomistische Theorie der Materie überhaupt sein.” § 1. In Born’s later lectures of the 1920s this decisive atomistic approach is not longer pushed. The university catalog for summer 1914 lists in addition another course by Born “Konstitution der Materie.”

¹²⁴Cf. (vom Brocke 1981, 142, 145 f.).

¹²⁵“Was Kristalle mit mehreren Atomarten angeht, so erklärt Herr Born in seiner bedeutungsvollen Monographie etwaige Abweichungen von der Cauchyschen Relation durch die gegenseitige Verschiebung der verschiedenen Atomarten; er hat aber, wie mir bekannt, wenigstens bei regulären Kristallen mit nur zwei Atomarten diese Darstellung als nicht haltbar gegenwärtig aufgegeben und zieht dafür ‘Elektronenwolken’ heran, die die Atome umgeben und, als gegen sie verschiebbar, gewissermaßen noch zwei weitere Atomgattungen vertreten. [...] stimmt bei Steinsalz sehr gut [...] die Zahlenwerte von Pyrit geben einen krassen Widerspruch. Dies ungemein verschiedene Verhalten auf die verschiedene Verschiebbarkeit der Elektronenwolken zurückzuführen erscheint wenig befriedigend.” 3f. Cf. also the contrary account by Voigt’s assistant from 1909 to 1914 in (Försterling 1951), who repeatedly emphasized Voigt’s embrace of Born’s theory.

lectures in the following term.¹²⁶ On this topic he would publish rather strongly, and the exchange with Einstein on the priority placed on central contributions to the final formulation of general relativity earned his work on this particular field of physics wide recognition (Sauer 1999; Rowe 2001; Sauer and Majer 2005, 259–276).

¹²⁶(Sauer and Majer 2009b) with a short discussion of the context of the lectures p. 74–78. Hilbert introduced the lectures as follows: “Die Vorlesung, die ich in diesem Semester mit “Grundlagen der Physik” angezeigt habe, soll sich wesentlich mit den modernen relativistischen Ideen beschäftigen [...]” (81). Part 1 comprises e.g. § 26 “Elektrodynamik auf Grund der atomistischen Hypothese” and §§ 27–30 “Die Miesche Theorie,” the notes of part 2 of winter 1916/17 constitute a presentation of general relativity.

| | Hilbert | Debye | Voigt <i>and others</i> |
|----------|--|--|--|
| SS 11 | Mechanik der Kontinua | | Kristallphysik Magnetooptik <i>Born</i> Wärmestr. |
| WS 11/12 | Kin. Gasttheorie | | Geometrische Optik |
| SS 12 | Strahlungstheorie | | Thermodynamik <i>Wiechert</i> Quanten |
| WS 12/13 | Molekularth. d. Materie Axiome der Physik* | | Elektrodynamik |
| SS 13 | Elektronentheorie Kin. Theorien der Physik* | | Theor. Optik |
| WS 13/14 | Elektromagnetische Schwingungen | | Th. des Potentials <i>Hertz</i> Strahlungs- und Quantenth. |
| SS 14 | Statistische Mechanik | guest professor lectures | Kristalloptik <i>Hertz</i> Strahlungs- und Quantenth. |
| WS 14/15 | Seminar über die Fragen der statist. Mechanik ^{a #} Prinzipienfragen d. Math. | Quantentheorie ^b | Part. Differentialgleich. d. Physik |
| SS 15 | Vorlesungen über Struktur der Materie ^c | Vorträge über die Struktur der Materie [#] Kin. Theorie dielektrischer Erscheinungen | Kapillarität |
| WS 15/16 | Seminar über die Struktur der Materie [#] (Differentialgleichungen) | Quantentheorie II Thermodynamik | Optik |
| SS 16 | Seminarübungen über die Grundlagen der Physik | Struktur der Materie Röntgenstrahlen | Elektrodynamik <i>Wiechert</i> Quantentheorie |
| WS 16/17 | Vorträge über die Struktur der Materie Grundlagen d. Physik II | Ausg. Kapitel der math. Physik | Elektronenth. und Relativitätshyp. Spektroskopie |
| SS 17 | Vorträge über die Struktur der Materie (Mengenlehre) | Grenzgebiete der Physik und Chemie ^d | Theorie und Anwendungen des Potentials |
| WS 17/18 | Vorträge über die Struktur der Materie Elektronentheorie | Neuere Erkenntnisse der Quantentheorie | Spektroskopie der Röntgenstrahlen |
| | Vorträge über die Struktur der Materie | | |

| | Hilbert | Debye | Voigt <i>and others</i> |
|--------------------|---|---|--|
| SS 18 | Differentialgleichungen (Thermodynamik) Seminar über die Struktur der Materie | | (Mechanik) |
| WS 18/19 | Über Raum und Zeit | Struktur von Spektrallinien | Optik Mech. d. Kontinua |
| SS 19 | Denkmethoden in den exakten Wissenschaften | Ausgesuchte Kapitel der kin. Gasttheorie | Grundbegriffe der theor. Physik |
| ZS ^e 19 | Math. phys. Seminar über die Struktur der Materie Natur u. mathematisches Erkennen | (Experimentalphysik) | Theor. Physik Thermodynamik |
| WS 20 | (Logik-Kalkül) Mechanik | Kristallstruktur | Part. Differential- gleichungen der theor. Physik ^f |
| | Vorträge über die Struktur der Materie | Kolloquium über neuere phys. Literatur ^f | |
| SS 20 | Mechanik und neue Gravitationstheorie | Mechanik | — <i>Hertz</i> Elektrizitätstheorie |
| | Vorträge über die Struktur der Materie | | |
| WS 20/21 | (Anschaul. Geometrie) Seminar: Struktur der Materie | — | <i>Hertz</i> Relativitätstheorie |

Table 5.5: Hilbert's physics lectures 1911–1920/21 compared with Voigt's and Debye's advanced courses, joint seminars and some related courses on quantum theory of other staff.

*Seminars on physics topics conducted by Hilbert.

#Not listed under Debye's name, but probably done in collaboration.

() Lectures of other topics, only mentioned if no other relevant lecture occurred.

^aPart of the seminar at least dealt with molecules, cf. above.

^bLecture notes Einführung in die Theorie des Planckschen Elementarquantums, AIP Misc. Physicists Collection: Debye, MP 56.

^cAt least part of the seminar dealt with atomic models: Born: Über die Stabilität des Bohrschen Atommodells (14 December 1914) Hilbert Papers, folder 690; Debye: Das Rutherfordsche Atommodell (undated) Hilbert Papers, folder 693/2.

^dListed title only, no lecture notes available.

^eZS is *Zwischensemester*, instead of summer and winter terms for 1919/20 there were three shorter terms SS, ZS and WS.

^fListed title only, Voigt had died 13 December 1919.

Sources: Verzeichnis der Vorlesungen auf der Georg-August Universität zu Göttingen, Lecture notes prepared for the Mathematische Lesezimmer, partly in the papers of Hilbert and of Born. For a complete list of Hilbert's lectures and available corresponding documents see (Hilbert 2004, 609–623).

-
- Mechanik der Kontinua: *thermodynamics, relativity, electrodynamics, Born's theory of the rigid body, relativistic thermodynamics, black-body radiation*
 - Kinetische Gastheorie: ^{*}[#] *hydrodynamics, theory of gases of one or more atoms (all in terms of integral equations)*
 - Strahlungstheorie: ^{*} *relativity, radiation theory by Kirchhoff, Planck's formula*
 - Molekulartheorie der Materie: ^{*} *theory of gases, crystals and radiation, Zustandsgleichung, specific heat (Debye, Born and Kármán), quantum hypothesis*
 - Elektronentheorie: *relativity, theory of gases applied on electrons (radiation, conduction), electron "clouds," electrodynamics and modified Maxwell's equations*
 - Elektromagnetische Schwingungen: *dispersion theory, Faraday and Zeeman effect, magnetism, Planck radiation law without quantum theory*
 - Statistische Mechanik: *principles of Maxwell and Gibbs*
 - Vorlesungen über Struktur der Materie: *kinetic theory of crystals (after Born).*
Die Grundlagen der Physik: ^{*} *special relativity, Mie's theory*
 - Die Grundlagen der Physik II: ^{*} *general relativity, causality*
 - Elektronentheorie: *special relativity*
 - Über Raum und Zeit: *semi-popular lectures on relativity*
 - Denkmethode in den exakten Wissenschaften: *philosophy of mathematics and physics*
 - Natur und mathematisches Erkennen: *philosophy of mathematics and physics*^{**}
 - Mechanik und neue Gravitationstheorie: *mechanics, relativity*
-

Table 5.6: Topics covered in Hilbert's physics lectures 1911–1920:

^{*}Published in (Majer and Sauer 2009, 2015).

^{**}A commented and edited version has been published as (Hilbert 1992).

[#]This is the title of the version of Hecke's notes kept in the Göttingen Mathematisches Seminar; Born's copy of these notes has the title "Mechanik der Kontinua aufgrund der Atomtheorie," Born Papers, folder 1816.

In any case, in order to keep Hilbert's project on quantum physics and the structure of matter moving, new forces were needed to push the issue further. Hilbert had paved the way for this, as he had deliberately redirected funds in the search for a quantum physicist of his liking.

5.3.5 Where Hilbert Acquires Funds: Wolfskehl Funds and Money for “Gastprofessor”

When the Jewish amateur mathematician Paul Wolfskehl of Darmstadt died in 1906 he had decreed by will that 100,000 Marks of his assets, many years’ worth of a professor’s salary, should become a prize to be donated to the person who proves or disproves Fermat’s last theorem. It was up to the Göttingen Royal Society of Sciences to form a commission for setting up procedures and deciding on the award. It assembled of the physician Ernst Ehlers (one of two secretaries of the Society), Hilbert, Klein, Minkowski (after his death Landau) and Runge.¹²⁷ Obviously, Hilbert was in charge, as he signed for all reports and was identified as such:¹²⁸ all accounts of Wolfskehl’s grant support identify Hilbert with decisions, especially while Klein was ill from winter 1911 on and not fit to work for some time.¹²⁹ Even before the prize competition was officially opened in June 1908,¹³⁰ it had already become clear that no quick winner would turn up, so that it turned out to be more reasonable to use the interest of the money to support mathematical work related to the problem.¹³¹

Table (5.7) summarizes the spending of this money together with some additional funds that were requested from the state for guest professorships. As one can easily recognize, only a small fraction was awarded for the purpose of Wolfskehl’s foundation. In the first year of operation less than a third of the available interest was spent for work on the Fermat problem by a mathematician in Münster.¹³² No further appropriate spending was found until ten years later, when a book on the Fermat problem earned 1500 Marks in 1919.¹³³

¹²⁷Bekanntmachung vom 27 June 1908 (ECP5, 502).

¹²⁸According to Günter Frei, Hilbert was president of the prize commission, see (Frei 1985, 136). Cf. also *Bericht der Wolfskehlstiftung* which were published in (Nachrichten) on a yearly basis from 1909 to 1921.; (I. Runge 1949, 149); Debye interview 1962, session II. (AHQP).

¹²⁹Frei (1985) notes: “Klein mußte nach erneuten depressiven Anfällen im August 1911 nach Hahenklee zur Erholung und Ende November ein zweites Mal. 1912 verbrachte er dort insgesamt neun Monate. Auf 31. Dezember 1912 bat er um Entlassung vom Amt in Göttingen.” Weyl took over his lectures on after Christmas 1911. *Mathematisch-Naturwissenschaftliche Blätter* 1912, p. 27. Also (Tobies 1981, 86): “So trat im November 1911 eine so schwere Erschütterung seines körperlichen Zustandes ein, daß er ein ganzes Jahr in einem Sanatorium in Hahenklee im Harz verbringen mußte.” and (I. Runge 1949, 152), mentioned that Klein stayed summer and fall 1912 in a sanitarium. Although Runge acted in his place, he kept organizing educational matters. Cf. also his declining number of contributions to the *Mathematische Gesellschaft*.

¹³⁰The Wolfskehl-Preis was presented to the public on 27 June 1908 [...], cf. (Jahresberichte DMV, 1909, pp. 111–113).

¹³¹This caused problems with the widow, who was against having the prize money divided. Cf. postcard from Minkowski to Hilbert 9 May 1908, in (H. Minkowski 1973).

¹³²(Jahresberichte DMV, 19, 1910, p. 175).

¹³³(Nachrichten, 1919, p.).

| Date | Speaker/grants for | Comments |
|---|---|--|
| 22–29 April 1909 | Poincaré | six talks on integral equations and relativity, 2500 M. |
| 1909 | Wieferich | Mathematician in Münster, 1000 M. for papers on Fermat problem |
| 24–29 October 1910 | Lorentz | Lorentz-Woche, talks “On the developments of our conceptions of ether,” discusses Planck’s radiation formula |
| June 1911 | [Michelson] | lectures by exchange professor [German American professor exchange], no Wolfskehl money spent |
| 1911 | Zermelo | 5000 M. for his works on set theory and as a grant to allow his recover from illness |
| July 1912 | Sommerfeld | 1000 M. for lectures on quantum theory in Hilbert’s class (Laue’s discovery was presented in <i>Physikalische Gesellschaft</i>) |
| 21–26 April 1913 | Planck, Nernst, Debye, Lorentz, Sommerfeld, von Smoluchowsky (Einstein abgelehnt) | Congress, “Gaswoche” on “the kinetic theory of matter and electricity” 4800 M. spent (800 M. each) |
| spring 1914 | [Lorentz planned as model <i>Gastprofessor</i>] | requested 5000 M; not granted by the ministry |
| summer term 1914 | Haar | guest professor for theoretical astronomy (lectures on cosmogony); 2000 M. paid by Wolfskehl funds |
| summer term 1914 | Debye | guest professor for theoretical physics; 1000 M. paid by Wolfskehl funds and 1000 M. support from the ministry |
| July 1914 winter 1914/15 until summer 1916 | [Bohr] Debye | talk and conversations, no funding to raise professorial salary above usual state limit 2000 M. from Wolfskehl fund and 2000 from Voigt annually |
| spring 1915 in summer 1915 | [talks planned] Pohl E. Meyer | lecture series cancelled due to war job talks by experimental physicists for Riecke position |
| June 1915 | Born Sommerfeld Einstein | talk in Math. Ges. on crystal structure, 7 June talk in Math. Ges. on modern physics, 15 June six talks, one in Math. Ges. on gravitation, 29 June |

| Date | Speaker/grants for | Comments |
|-------------|---------------------|---|
| spring 1916 | Smoluchowski | lectures in mathematical physics |
| 4–6 June | Mie | three talks on Einstein's theory of gravitation and matter, had been planned for 1916 |
| 1917 | | |
| 25–29 June | Hecke | lectures on mathematics |
| 1917 | | |
| 11 December | Born | talk on liquid crystals in <i>Math. Ges.</i> |
| 1917 | | |
| [1918] | [Ehrenfest invited] | refused to come |
| 14–17 May | Planck | lectures on the current state of quantum physics |
| 1918 | | |
| 16–19 | Driesch | talks on “organic causality” |
| December | | |
| 1918 | | |
| 1919 | Bachmann | Paul Bachmann for book “Das Fermatproblem” 1500 M. |
| 1920 | | ? |
| 1921 | | no Wolfskehl funds spent |
| 1922 | Bohr | invited for 1921, money for <i>Gastprofessur</i> requested |
| [1922] | [Russell invited] | invited for 1922, money for <i>Gastprofessur</i> requested |

Table 5.7: Overview of the spending of Wolfskehl funds and means for *Gastprofessur*.

Besides its true, albeit neglected, purpose the Wolfskehl funds were, in fact, spent for guest professorships for mathematicians and theoretical physicists, but also for talks by experimental physicists and biologists or philosophers. The first Wolfskehl-funded *Gastprofessor* was Henri Poincaré in 1909. He chose to speak on integral equations and their application to physics and astronomy, in particular on tides and Hertzian waves, as well as on transfinite cardinal numbers. While not concealing the competition between him and Hilbert, at the same he made the event a harmonious demonstration of scientific internationalism.¹³⁴ The *Lorentz-Woche* in the following year was a similarly high-profile event and stood under the title “On the development of our conceptions of the aether.” According to Max Born, Lorentz gave “a good survey on the physics of that time and culminated in the derivation of Planck’s radiation formula.”¹³⁵ While Voigt had been exchanging letters with

¹³⁴Hilbert Papers, folder 579, Nr. 1, Bl. 1–2. “[Poincaré] beabsichtigt über Integralgleichungen und deren Anwendung auf Physik und Astronomie (Flutbewegung und Hertzische Wellen), auch über den Begriff der transfiniten Kardinalzahlen vorzutragen.” Report in (Jahresberichte DMV, 1909, p. 39). Poincaré lectured in German, the lectures were published as Poincaré 1910. For an account of the honored visit cf. (Reid 1970, p. 120). In Hilbert’s opening address on April 22, 1910 he said: “Die mathematischen Fäden zwischen Frankreich und Deutschland sind so mannigfaltig, und stark, wie nirgends zwischen zwei Nationen, so dass wir in mathematischer Hinsicht Deutschland und Frankreich als ein einziges Land ansehen.”

¹³⁵Announced title “Über die Entwicklung unserer Vorstellungen von Äther” (Jahresberichte DMV, 1910, p. 227), published as (Lorentz 1910); it gave a “guten Überblick über die Physik jener Zeit und gipfeln in der Ableitung von Plancks Strahlungsformel” (Born 1975, 207).

Lorentz for decades, in particular on his research on the Zeeman effect and had invited him to Göttingen in 1897—and thus must have known him far better than his mathematical colleague—, it was Hilbert who introduced the guests and spoke about the relationship between physics and mathematics in the opening and closing addresses.¹³⁶

In 1911 the German-American professor exchange program brought German-born American physicist and Nobel laureate of 1907 Albert Abraham Michelson to Göttingen. With him came a prestigious new diffraction grating of his own production, which was to allow Voigt better research on the Zeeman effect. As Michelson thus occupied the spot of the *Gastprofessor*, nobody else was invited on Wolfskehl funding.¹³⁷ For this reason, Hilbert was able to use the available money instead to support Zermelo who was ill and had not succeeded in finding a position.¹³⁸

In the following year no lecture week took place for various reasons. In one rare instance, Sommerfeld received a generous salary for two talks. In 1913 a more ambitious project was realized, an international congress. Paying each of the six guest speakers 800 Marks allowed to finance the congress fully from the roughly 5000 Marks annual interest.¹³⁹ From 1914 on Hilbert tried to change the character of the yearly events from Pentecost lecture weeks to stays for a whole summer term for guest professors. At the same time he applied for state funding for this new format. He had probably taken Nernst as a model, who had, on the one hand, realized with help of the Belgian industrialist Ernest Solvay the influential Solvay conferences, and on the other hand taken advantage of this event to argue for German state as well as private funding.

In June 1913 the Göttingen Society of Sciences officially, and Hilbert in a number of separate letters, applied to the Ministry of Culture for funds to invite a leading scientist in either one branch of pure mathematics or one from a neighboring field like theoretical physics or mathematical epistemology.¹⁴⁰ The *Gastprofessur* would establish a special research forum for theoretical research, “like those received by the experimental sciences to large extent, in particular by the research institutes of the Kaiser Wilhelm society.”¹⁴¹ Hilbert’s call for a kind of Göttingen Kaiser Wilhelm Institute for theoretical research was given weight by reference to the exceptionally modest (“außerordentlich bescheiden”) means needed for it.¹⁴²

The leading scientists Hilbert had in mind in 1913 were Lorentz, Rutherford and Hadamard. Lorentz was first choice for two reasons: Having resigned from university duties and turned completely to research at the age of 58, he would bring in weight and availability. In addition, “as a brilliant model” he would guarantee a good start that could be used in the following years to acquire state funding for further scientists.¹⁴³ Furthermore,

¹³⁶Cf. his notes Nachlaß Hilbert, Mappe 577.

¹³⁷In (Born 1975, 207), Michelson is put in one line of guest professors with Poincaré and Lorentz. He found his lectures, however, less inspiring than his tennis play and family.

¹³⁸Report in (Nachrichten, 1911, p. 125): “Aus den Zinsen der Wolfskehl-Stiftung erhielt Herr Professor E. Zermelo für seine Leistungen auf dem Gebiet der Mengenlehre und als Beihilfe zur völligen Wiederherstellung seiner Gesundheit eine Renumeration von 5000 Mark.”

¹³⁹Document 4.

¹⁴⁰Document 4. For the correspondance on the application cf. GStA PK 76 V c, Sekt. 1, Tit. 11, Teil 9 Nr. 10, Bd. 7.

¹⁴¹Dokument 3.

¹⁴²Document 4.

¹⁴³Hilbert to Krüss, 10 January 1914, Hilbert Papers, folder 494, Nr. 10, Bl. 33. “[...] möchte ich nunmehr alle Mittel anstrengen, um H. A. Lorentz Leiden dieses Frühjahr doch hierher zu bekommen – nicht um mindestens

it was Lorentz who had made the Solvay Conference a successful forum for quantum theory.¹⁴⁴ Rutherford was another leading physicist who had contributed to the atomic debate, host Bohr when he was writing his seminal papers, although he was not fully convinced of the later theory including quantum jumps. Hadamard, finally, had a good record in more standard mathematical fields of great relevance for physics like differential equations.

Hilbert took considerable pains to argue for his proposals. In a long letter to Hugo Krüss, a physicist working for the Berlin ministerial bureaucracy, he did not hesitate to attempt to explain the basics of the theory of relativity to the representative of the *Kultusministerium* in order explain how important this *Gastprofessur* was, particularly for a subject that could only be solved by the joint effort of mathematicians and physicists: the atom.¹⁴⁵ And in a similar way Hilbert reported to *Ministerialdirektor* Naumann about Debye's work on a later occasion.¹⁴⁶ When the Ministry of Finance ultimately rejected the application, Hilbert was shocked and reiterated his claims on a reduced level.¹⁴⁷ Lorentz, however, did not come. The cheaper solution, however, bought two guest professors: Alfred Haar was paid for his lecture on theoretical astronomy from the Fermat fund, while Debye, as *Gastprofessor* for theoretical physics, received the reduced state contribution as well as Fermat money in equal parts.¹⁴⁸

Whether the two guest professors of summer 1914 achieved what Hilbert had promised to the ministry is hard to decide. Parts of the lectures can be reconstructed from two folders in Hilbert's papers. From Haar's lectures Hilbert kept a four-sheet manuscript on Maxwell's theory of the Saturnian ring.¹⁴⁹ A different folder holds manuscripts and notes of three lectures by Debye. Only the first one on electrical dipole momenta is dated, 4 May 1914. The others treated Rutherford's and Bohr's atomic models.¹⁵⁰ It is likely that these talks were given in preparation for Bohr's visit in Göttingen, who was en route to Munich and then for the summer holidays in Switzerland in July 1914.¹⁵¹

auch deshalb um ein glänzendes Präjudiz und Vorbild eines Gastprofessors zu schaffen, das uns später bei dem Kampf um den Etat für 1915 nützlich sein soll."

¹⁴⁴Cf. (Kormos Barkan 1993; Schirmacher 2012).

¹⁴⁵Document 5.

¹⁴⁶Document 11.

¹⁴⁷Refusal from Ministry of Finance to the Ministry of Culture, 11 November 1913, GStA PK 76 V c, Sekt. 1, Tit. 11, Teil 9 Nr. 10, Bd. 7; Hilbert to Krüss about his "Schreck" and the need to get at least 1000 Marks from the overall 4000 needed from state funds, 10 January 1914, Hilbert Papers, folder 494, Nr. 10, Bl. 33.

¹⁴⁸Document 7. "[...] es ist nämlich teils aus der Wolfskehlstiftung, teils aus dem vom Ministerium und der Kgl. Gesellschaft der Wissenschaften bewilligten Mitteln zwei "Gastprofessoren" nämlich Debey[ye]- Utrecht für theoretische Physik und Haar- Klausenburg für theoretische Astronomie zu engagieren gelungen." Report by Hilbert in (Nachrichten, 1914, p. 17): "Die Kommission der Wolfskehl-Stiftung hat für das Sommersemester 1914 Herrn Professor Haar- Klausenburg als 'Gastprofessor' nach Göttingen berufen; derselbe hält hieselbst gegenwärtig eine Vorlesung über Kosmogonie und wirkt durch Vorträge in der Mathematischen Gesellschaft und im mathematisch-physikalischen Seminar. Außerdem hat die Kommission eine Beihilfe von 1000 Mark bewilligt, um die Herberufung von Herrn Professor Debye-Utrecht für das Sommersemester 1914 zu ermöglichen."

¹⁴⁹"Maxwells Theorie des Saturnrings"; Hilbert noted on the envelope "Haar als Gastprofessor S.S. 1914." Nachlaß Hilbert, Mappe 699a, Bl. 1–12.

¹⁵⁰"Elektrische Dipolmomente. Vortrag gehalten am 4. Mai 1914," "Rutherford'sches Atommodell," and "Bohr'sches Atommodell." Nachlaß Hilbert, Mappe 693, Nr. 1, Bl. 1–8, Nr. 2, Bl. 9, and Nr. 3, Bl. 10–15.

¹⁵¹Bohr was in Munich on 15 July 1914, cf. (Eckert 1993, 53). For his impression of Göttingen see letters in (BCW2).

Due to the war the Pentecost workshop was canceled in 1915.¹⁵² Part of the available money now regularly went to Debye to enhance his professorial salary. Funds were also spent to invite Born and Sommerfeld for single talks and Einstein for a lecture series of six talks, which spread over the various seminars at the end of the summer term, thus, in effect, creating an equivalent of the lecture weeks with a moving venue. With Einstein, Born and Sommerfeld and possibly other visitors for the talks, Göttingen demonstrated at the end of the first year of war that scientific life went on.

Another way to spend Wolfskehl money was to invite a number of physicists for the job talks that became necessary to fill the vacancy after Riecke's death, many of which probably would not have taken place. Although in some official reports these talks were counted as scientific exchange, their only purpose was to get to know the capabilities of applicants for Riecke's position.¹⁵³

Hilbert's growing interest in gravitation and Mie's field theory made it desirable to invite the latter for a spring workshop in 1916. This had to be postponed until 1917, when Mie laid out in three lectures, his trilogy on the problem of matter within Einstein's theory of gravitation, which he subsequently published (Mie 1917a; 1917b; 1917c). In 1916 it was Merian von Smoluchowski who held lectures on mathematical physics.¹⁵⁴ 1917 actually saw two lecture series, besides Mie, Hecke also lectured on mathematical problems. And Born came, too, to deliver a talk at the *Mathematische Gesellschaft* that was supported by Wolfskehl money, demonstrating another use of the Wolfskehl funds: maintaining good relations with former scholars.¹⁵⁵ While Ehrenfest refused to give the by then rather prestigious Wolfskehl lectures in 1918, citing as reasons the "continuous rape of Belgium" and the "sympathies with the views of my closer Russian friends,"¹⁵⁶ Planck lectured in the time slot instead; in his case support from the Wolfskehl fund was planned but not realized.¹⁵⁷

The fact that Hilbert also invited Hans Driesch, theoretical biologist and neovitalist who was later known for para-psychological studies, to give "physical-philosophical lectures" "on organic causality" may seem rather strange at first glance. However, in the context of Hilbert's interest in extending his axiomatic thinking step by step to all sciences, however, this appears less of an outlier; Runge and Debye are reported to have inspired Driesch con-

¹⁵²"Der für das Frühjahr 1915 in Aussicht genomene Vortragszyklus ist des Krieges wegen nicht abgehalten worden" (Nachrichten, 1915, p. 14).

¹⁵³Cf. Debye's report in *Chronik der Georg-August-Universität zu Göttingen* 1915, p. 52: "Auf Einladung der Kommission der Wolfskehlstiftung hielten eine Reihe auswärtiger Gelehrte, die Herren A. Einstein, H. [?] Franck, P. P. Koch, Edgar Meyer, C. H. Müller, R. Pohl, C. Schäfer, A. Sommerfeld Vorträge aus den jeweiligen Arbeitsgebieten." Also (Nachrichten, 1916, p. 13): "Vorträge im Rahmen der Berufung im SS von Pohl und E. Meyer stattgefunden, [...] im WS Franck, Schweidler, Geiger, Grüneisen in Aussicht." Debye to ministry; 28 August 1915, GStA PK 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 268–269: "unter Benutzung von Mitteln, welche das Curatorium der Wolfskehl-Stiftung durch Herrn Geheimrat Hilbert in dankenswerter Weise zur Verfügung gestellt hat, die in Betracht kommenden Kandidaten, so weit dieselben in Göttingen unbekannt sind, zu öffentlichen, wissenschaftlichen Vorträgen heranzuziehen."

¹⁵⁴(Nachrichten, 1917, p. 13).

¹⁵⁵(Nachrichten, 1918, p. 40); Mie to Hilbert, 16 May 1917, Hilbert Papers, folder 254, Nr. 6, Bl. 9

¹⁵⁶Ehrenfest to Hilbert, 11 March 1918, Hilbert Papers, folder 91, Nr. 2. "fortlaufende Vergewaltigung Belgiens," "Sympathien mit den Anschauungen meiner nächsten russischen Freunde"; he also writes that he presumably would not get a passport.

¹⁵⁷Hilbert to Sommerfeld, 18 February 1917, DM, Archive "Für die Woche vor Pfingsten d.h. am 21. Mai wird auf Wolfskehl-Einladung Mie herkommen. Planck hat uns für Pfingsten 1918 ein zusammenfassendes Referat über Quantentheorie in Aussicht gestellt und dazu Sie und Epstein 'angefordert'."

siderably during his stay.¹⁵⁸ After the war only one more grant was given, actually for a publication on Fermat's problem, before inflation destroyed this influential asset. A major resource for physics development was gone. The invitation for a guest professorship in 1921 and 1922, which was now fully dependent on state funding, went to Niels Bohr and Bertrand Russell. The renowned *Bohr-Festspiele* (Bohr festival) took place in 1922.

5.3.6 Connecting Göttingen Nationally and Internationally

In the last couple of sections I have tried to quantify various types of investments of resources in a new physics at Göttingen. Through surveying personnel, teaching, talks and invitations to guests, a specific local picture was drawn for a process of interaction and reorganization of physics and mathematics. While the guest professors and further invitations of scholars from Germany and from abroad already exhibited a certain level of cooperation with other scientific centers, I would like to look now briefly at particular efforts to connect Göttingen with a kind of quantum discourse, that had just emerged at national and international meetings, some of them in rather select circles.

As Kuhn recognized in his book on the early quantum theory, a broader discussion and a significant rise in quantum papers occurred in 1911, related to new attention to the field of specific heats, which Arnold Sommerfeld in particular popularized at the 1911 *Naturforscherversammlung* in Karlsruhe. This event was the main annual meeting of physicists and mathematicians alike (Kuhn 1978b, 216–219). A few weeks later, a much more intimate circle gathered in Brussels for the first *Solvay Council* on “radiation theory and quanta,” an exclusive event orchestrated by Walther Nernst and financed lavishly by the Belgian industrialist Ernest Solvay, gathering the top European physicists to confer on finding a solution to the quantum riddle.¹⁵⁹ Clearly, no quick solution was found and the Solvay Conferences became an ongoing endeavor; the second one convening in 1914.

With these two series of national and international events that focused on quantum problems from 1911 on, a relevant context for the Göttingen efforts to connect to this broader discourse can be identified. Two Göttingen initiatives reached out in this way: the invitation of Arnold Sommerfeld in 1912 and the *Gaswoche*, a conference on the topic of kinetic theory of matter (Born 1913b).

The first attempt to get the new quantum discourse to Göttingen was to ask a participant to share his knowledge. This may be the explanation for Hilbert's invitation letter to Sommerfeld in April 1912. Here he gives various, albeit hardly conclusive reasons why no “Fermat weeks” of the usual kind should take place: On the one hand the international congress of mathematicians in England was stealing the show, on the other hand Klein was ill and could not take part. “For these reasons,” he continued, “I have thought of the following alternative of more modest dimension: As I am teaching in this term on principles and axioms of physics [...], what would you think about replacing me in the last two sessions, [...]? This time would fit the Göttingen lecturers and younger mathematicians and physicists presumably best, such that I could guarantee for a well-filled auditorium.”¹⁶⁰ Furthermore, Hilbert offers 1000 Marks from the Fermat fond to finally convince him. As he politely

¹⁵⁸(Nachrichten, 1919, p. 43). For Hilbert's turn to philosophy cf. (Peckhaus 1990). On his Göttingen stay see Schnaxl to Hilbert, 27 December 1918, Hilbert Papers, folder 347.

¹⁵⁹Participants: Nernst, Planck, Rubens, Sommerfeld, Warburg, Wien; Jeans, Rutherford; Brillouin, Curie, Langevin, Perrin, Poincaré; Einstein, Hasenöhrl; Kamerlingh Onnes, Lorentz; Knudson, cf. Solvay 1921.

¹⁶⁰Document 1.

leaves the decision about the topic to Sommerfeld, at the same time he mentions that the theory of radiation and quantum theory would be most welcome.

Hence this letter is a good example that shows a direct effort to redirect financial means meant for mathematics to fields of his own special interest, viz. radiation and quanta. It is, however, hard to relate the investment of more than a year's salary for Hilbert's physics assistant Ewald, or roughly the monthly income of a full professor at that time, with the returns of two concluding talks for a lecture course by Hilbert.

Hilbert's lectures were ultimately entitled "Theory of Radiation" (Strahlungstheorie)¹⁶¹ and, in any case, it was a unusual lecture. This is apparent from a report by the Göttingen *Mathematischer Verein*, which not only stated that members were recommended to register for practical classes in physics at the earliest possible date, with reference to the overcrowded institutes at that time, but also pointed out the special free lectures on the foundations of physics by Hilbert, suggesting that these offered a measure of relief.¹⁶² This is an example of Hilbert's forceful entry into physics teaching. As it was a lecture without the usual fee in economic term it constituted a subsidy for its field of research. As part of this free offer, highly paid Sommerfeld delivered what he had been asked to. Moreover, he used his presence to report to the *Physikalische Gesellschaft* on Laue's discovery of X-ray diffraction with crystals.¹⁶³

A second step to get in touch with the quantum and Solvay people followed, when the *Deutsche Mathematiker-Vereinigung*, in collaboration with the physical section of the *Gesellschaft der Naturforscher und Ärzte*, organized a joint session at the Münster *Naturforscherversammlung* in September 1912. Under the chairmanship of Sommerfeld, Hilbert the only mathematician, had announced a speech "On the foundations of the kinetic theory of gases" but finally gave a talk on "The foundation of elementary radiation theory," while Nernst reported "On the energy content of gases" and von Smoluchowski discussed "Experimentally detectable molecular phenomena that contradict ordinary thermodynamics."¹⁶⁴ Effectively, this program anticipated the later Göttingen *Gaswoche*.

The conference "on the kinetic theory of matter and electricity," as it was officially called, was exceptionally well organized and advertised. Hilbert inquired about work in the field of the theory of gases and radiation a full year before the meeting, invitations were sent half a year in advance.¹⁶⁵ Several weeks before the meeting it was the cover story in *Naturwissenschaften*, which even published abstracts of the talks (Born 1913b; Planck 1913).

The unidentified author of the announcement in *Naturwissenschaften* was Max Born. He wrote on the aims of the congress that was presented as a direct follow-up to the Solvay

¹⁶¹The announced "Prinzipien der Physik" were postponed to the winter term course and seminar listed under these, notes are entitled "Molekulartheorie der Materie."

¹⁶²Mathematisch-naturwissenschaftliche Blätter, 1912, p. 27: "[...] vierstündige Gratisvorlesung über die 'Grundlagen der Physik,' zur Entlastung der überfüllten Institute." For the nearly steady growth of student numbers in mathematics and natural sciences from 1893 to 1913 by a factor 10, see (Lorey 1916, 22); the numbers of staff only roughly doubled in the same period.

¹⁶³Mathematisch-Naturwissenschaftliche Blätter 1912, p. 187. "[Sommerfeld] im Kolleg von Herrn Geh. Rat Hilbert eine Reihe von Vorlesungen über die Quantentheorie hielt. Er referierte in der Physikalischen Gesellschaft auch über die Lauesche Entdeckung der Beugung von Röntgenstrahlen durch Kristallgitter."

¹⁶⁴Cf. report in (Jahresberichte DMV, 1912, p. 158). Hilbert's contribution appeared in (Nachrichten, 1912, pp. 773–789), also in (Hilbert 1935, 217–230). Interestingly, Sommerfeld in turn gave a talk in a mathematics section, cf. *ibid.* p. 154.

¹⁶⁵Cf. e.g. Hilbert to Einstein, 30 March 1912 and Einstein to Hilbert, 4 October 1912, (ECP5, 439 and 502).

meeting, that while the latter “gave the best overview on the state of quantum theory” for the Göttingen congress “the foundation of this teaching [Lehre] comes to the fore and, what is most closely related, the revision of the foundations of statistical mechanics.” The general situation was characterized in the article by the claims that the phenomenological approach had lost its legitimacy, the molecular viewpoint was the truly justified one, and that one must not wonder about “claiming forces and relations for molecules, that are foreign to our macroscopic experience.” “Phenomenological modesty [Begnügsamkeit]” was out and a “striving for a deeper insight in the essence of matter is re-awoken” (Born 1913b, 297 and 299). Moreover, “it is possible to say that for the time being it is the noblest duty of physics to search for practicable assumptions about the physics of molecules.”

Planck’s quantum hypothesis served as prominent model and turned out to have far-reaching applications. But “is it really an expression of a fundamental new law of atom mechanics [Atommechanik] or can it perhaps be explained on the grounds of usual mechanics?” An open question for the meeting was hence, “whether the contradictions, quantum theory exhibits against our other concepts will be cleared up easily by revising the foundations of statistical mechanics,” which would possibly allow classical mechanics to be maintained, or whether the quantum had to be acknowledged as something irreducibly new (Born 1913b, 298 f). In the abstract to his Göttingen contribution Debye employed rather an opposite approach, stating that statistical mechanics probably had to be “corrected” by quantum theory, stressing his firm belief in the irreducibility of this concept (Planck 1913, 140–145).

The proceedings of the Göttingen congress appeared in 1914 at the time when those of the Solvay meeting were being reviewed and the German translation published (Born 1914b; Eucken 1914). It lists the authors as “von M. *Planck*, P. Debye, W. *Nernst*, M. v. Smoluchowski, A. *Sommerfeld* und H. A. *Lorentz* mit Beiträgen von H. *Kamerlingh-Onnes* und W. H. Keesom, einem Vorwort von D. Hilbert und 7 in den Text gedruckten Figuren.” Five of eight contributors to the *Gaswoche* volume (above in my italics) had participated in the Solvay congress, while some had been invited to Göttingen right after this meeting (Poincaré and Lorentz). Einstein, however, who declined the invitation to avoid not distraction from his work on gravitation, further shows how the scientific groups at the two conferences matched. In his preface Hilbert stresses the relevance of the papers to mathematicians. “May this collection of talks,” he wrote, “especially stimulate also the mathematicians to deal with the world of thoughts [Gedankenwelt], that has been created by the recent physics of matter” (Planck et al. 1914, 1).¹⁶⁶

The topic of the second Solvay conference in fall 1913 might well have been chosen by the Göttingen physicists and their *Ersatz* scientist Hilbert, and to some extent Courant’s and Born’s mathematical selves, as it uses exactly their phrase, the structure of matter. The invitation list would have been chosen differently, however—only Voigt, still a convinced phenomenologist, was invited from Göttingen. In the company of Nernst, Grüneisen, Rubens, Sommerfeld, Wien, Einstein, Laue, Weiss, Hasenöhl and others he will have been conceptually isolated. Instead of a planned full-length paper, Voigt read only a brief report on the relation between pyroelectricity and temperature. To please and to pay homage to the

¹⁶⁶“Möge diese Sammlung von Vorträgen insbesondere auch die Mathematiker zur Beschäftigung mit der Gedankenwelt anregen, die von der neueren Physik der Materie geschaffen worden ist.”

congregation he suggested at one point that the quantum hypothesis might play a role in its explanation.¹⁶⁷

More in the center of the program was clearly Laue, who was to report on X-ray interference by three-dimensional crystal lattices, as well as Grüneisen, who gave a talk on the molecular theory of solids in which he refers to Einstein, to Born's and von Kármán's theory, and in particular to Debye's *Gaswoche* speech. Interestingly, there is an *Solvay-Gaswoche* interaction in both directions, as Debye in the later *Gaswoche* proceedings also refers to Grüneisen's Solvay contribution of 1913.

Due to the topics and interaction one could call the Wolfskehl congress "Solvay 1.5," since both the field (quanta and structure of matter) and the speakers overlapped. What the Solvay trust was to the Brussels meetings, the Wolfskehl donation was to the Göttingen activities. Actually, in both cases the initial motivations for the bequests were quite different from the later results: As Wolfskehl wanted to tie his name to the centuries-old puzzle of Fermat's theorem and would never have wished to support physics research, Solvay either had the promotion of chemistry in mind or had intended to discuss his own rather ominous pet theory with the European science elite.¹⁶⁸

5.3.7 Hilbert and Voigt—Resource Differences

The discussions about Hilbert's use of resources and investments in physics call for a comparison. What were the plans and actions that came out of the physics staff at the same time, and how did they invest their resources? Clearly, a comparison with Voigt is in order. However, the differences and contingencies that make such a comparison of limited significance must not be ignored; for instance, the fact that in 1912 Hilbert was 50 years old while Voigt was already 62. This difference had less to do with youth versus proficiency, than with the risk that Hilbert could be lost to another university, while this was unlikely for Voigt.

Voigt's disciple presumed to show the highest potential was Paul Drude. McCormmach speculated that Drude would have become a leading quantum physicist if he had not committed suicide in 1906 (McCormmach 1982, 100f). Voigt's assistant during the prewar period, Gustav Rümelin, might also have become an interesting example for following a different path into modern physics. He completed his doctorate with Nernst at Göttingen in 1905 and worked with Rutherford on radioactivity in 1906/07. Although he later looked into a problem originating from this in his habilitation, which he finished in 1911, he never generated much publicity, having inherited a certain reserve from Voigt. Yet neither Rümelin nor Voigt's other assistant Werner Planck had a real chance to excel: Voigt found himself writing obituaries for his assistants, both of whom were killed in action in 1915 (Voigt 1915a).¹⁶⁹ Nonetheless, it remains obvious that both the role of the assistants and their scientific development were quite different from those of Hilbert. This was mainly due to Hilbert's ability to mobilize resources when needed, while Voigt looked back on a long record of unfulfilled wishes.

¹⁶⁷(Institut Solvay 1921), for a concise description cf. (Mehra 1975, 74–112). "Voigt gave a brief report on the relation between the pyro-electricity of certain crystals and temperature, based on research at his institute in Göttingen. He thought that the results were of interest not only for crystals but also for the quantum hypothesis." p. 88.

¹⁶⁸Solvay had prepared a re-publication of his work, and supplied it to the participants of the first Conference (Solvay 1911).

¹⁶⁹It is striking, however, that the personalities sketched here appear to be quite different from e.g. Hilbert's assistants.

On a cognitive and psychological level, Voigt and Hilbert differed radically. When Voigt described the character and aims of his institute in 1913 he wrote

Only we do not engage in pioneering work on new, unsolved fields, but pursue tasks that are related to the testing and further development of the theories of already established fields and measure especially the numeric values that appear in the theories. From the outside this may seem a rather plain occupation, it is, however, nevertheless necessary and useful. This activity, as I have pointed out previously, is closely related with higher scientific education in physics.¹⁷⁰

For Hilbert there was no need to care of educational needs in this way. His use of lectures and seminars to test and expand current research turned out to be very fruitful for prospective researchers, who probably did not always achieve the full spectrum of education in mathematics and physics.

There are few instances where one can more or less directly compare Hilbert's and Voigt's access to resources. While Hilbert took pains to connect both to the German physical community, which he joined at the 1912 Münster meeting, and to the Solvay circle of eminent European scientists, whom he invited to Göttingen—a group which did not consider admitting him, despite the membership of the French mathematician Poincaré, Voigt participated in the second Solvay conference. He also requested support from the Solvay Foundation, which had been established shortly after the 1911 meeting; coincidentally, his long-time friend Lorentz was dealing with the applications. Nonetheless, the language may be telling, as Voigt writes “I kindly ask you to excuse the pestering. You know *how difficult* it is for me to satisfy the demands of the many men, who work in my institute” when he asks for financial support for an apparatus for his assistant Dr. Rohn, which would cost 600–1000 Francs.¹⁷¹ As he had to realize early, his chances for getting reasonable support from the ministry were dim. Instead Klein's *Göttinger Vereinigung* provided some help, although his institute was not dedicated to applied physics and hence was, strictly speaking, outside of its scope. Nonetheless, he could be grateful for “repeatedly having been granted welcomed help.” The last time had been two years before, when it contributed to the procurement of an “modern first-class lattice spectrometer.”¹⁷²

In this way Voigt was well equipped for optics and crystal physics, his main areas of interest. Some of the equipment he had received directly from industrialists, and a few objects were funded by the *Göttinger Vereinigung* or even foundations like Solvay's; hence,

¹⁷⁰Voigt at “Tagung der Göttinger Vereinigung zur Förderung der ang. Physik und Math.” Protokoll vom 21.–22. November 1913 zu Göttingen, GStA PK 76 V a, Sekt. 6, Tit. X, Nr. 4 Adbih., Heft VII, p. 46 f. “Nur beschäftigen wir uns nicht mit der Pionierarbeit in neuen, unaufgeklärten Gebieten, sondern verfolgen Aufgaben, die mit der Prüfung und Ausgestaltung der Theorien in bereits erschlossenen Gebieten zusammenhängen, und bestimmen speziell die Zahlenwerte physikalischer Konstanten, die in den Theorien auftreten. Das ist eine nach aussen einigermassen unscheinbare Arbeit, die aber nichtsdesstoweniger notwendig und nützlich ist. Diese Tätigkeit steht auch, wie ich bei einer früheren Gelegenheit ausgeführt habe, in engem Zusammenhang mit dem höheren wissenschaftlichen Unterricht in der Physik.”

¹⁷¹Voigt to Lorentz, 5 June 1913, AHQP/LTZ4. “Ich bitte Sie freundlichst, die Belästigung zu entschuldigen. Sie wissen, wie schwer es mir wird, die Ansprüche der vielen in meinem Institut arbeitenden Herren zu befriedigen.”

¹⁷²Cp. Voigt's statement at the meeting of the *Göttinger Vereinigung*, November 21–22, 1913, protocol p. 47, GStA PK 76 V a, Sekt. 6, Tit. X, Nr. 4 Adbih., Heft VII: “Die Göttinger Vereinigung hat meinem Institut, trotzdem dasselbe nicht zu ihrem eigenlichen Unterstützungsgebiet gehört, wiederholt ihre willkommene Hilfe angedeihen lassen. Zum letzten Male vor zwei Jahren, wo sie zusammen mit der Unterrichtsverwaltung die Mittel zur Anschaffung eines modernen erstklassigen Gitterspektroskopes gespendet hat. [...]”

the only option was for him to pay out of his own lecture fees or salary. Ironically, quite often a share of his income would subsidize for years a project, that had been initiated by Hilbert.

5.4 An Einstein for Göttingen: Peter Debye

As we have seen, there were many reasons, to carry out a reorganization of Göttingen physics through installing a new leader for the field of modern quantum and atomic physics. And the competition had already started, fueled in part by exciting research work like that of Einstein and von Laue, in part by the reputation granted by institutions like the Nobel committee, the Solvay circle, or by both. The main impetus came from the rise of theoretical physics as a field that grew out of the shadows of experimental physics into the light of spectacular advances as in the case of relativity or atomic structure.

Theoretical physics developed as a separate branch of physics with its own chairs—with the exception of singularities like Planck or Lorentz—to become a common field mostly represented by associate professors [*Extraordinarius*, or *außerordentlicher Professor*] around 1900; it consolidated, and thus became a matter of full professors, only shortly before World War I (Jungnickel and McCormack 1986, ch. 25). Berlin became the place that set new standards for representing the new field appropriately. First with Planck, as a new type of theoretical physicist, who could do without instruments and experiments, and decidedly did,¹⁷³ and later with Einstein and his novel theories far away from everyday experience and experiments.¹⁷⁴ As Göttingen had equaled Berlin in academic weight in mathematics early in the century, and surpassed it a few years later, it clearly recognized Planck's extraordinary effort to lure Einstein to Berlin with an attractive offer.

The letter from the Dean of the Göttingen philosophical faculty to the ministry that asked for the creation of an additional chair was, in fact, meant to keep Debye in Göttingen when his term as *Gastprofessor* had ended. It demonstrates at the same time who was considered important on the market in 1914: After unspecified reference to recent “revolutions” [*Umwälzungen*] in the field of theoretical physics, the Dean wrote:

A university that intends to be adequately represented in theoretical physics must fully recognize this new field in teaching and research, if necessary by calling in new staff. To illustrate the movement that has emerged, it is pointed out, that Berlin, richly endowed in general and particularly with in respect to young talent, has won and tries to win two of the most outstanding representatives of the new field most recently: Einstein for an academy and Laue for a university position, respectively.¹⁷⁵

¹⁷³Cf. a letter from the Prussian minister of finance to the minister of culture, July 26, 1914, concerning requests from Göttingen for funds to equip a planned divided institute with laboratory facilities for the theoretical section as well, where the ministry of finance argues, that “the theoretical physics does not need at all own separate laboratory and experimenting premises according to the express declaration of its outstanding exponent Dr. Plan[c]k.” GStA PK 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 90. “[...] als die theoretische Physik nach den ausdrücklichen Erklärungen ihres hervorragenden Vertreters Dr. Plan[c]k eigener abgesonderter Laboratoriums- und Experimentieräume an sich überhaupt nicht bedarf.” See also (Schirmmacher 2010).

¹⁷⁴For Einstein's appointment and conditions see contribution of Goenner and Castagnetti to this volume.

¹⁷⁵Dekan Körte to Minister, 28 May 1914, GStA PK 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 74. “Eine Hochschule, die in theoretischer Physik vollwertig vertreten sein will, muss diese wichtige neue Richtung in Lehre und Forschung ausgiebig berücksichtigen, eventuell durch Heranziehung neuer Mitarbeiter. Zur Illustration der hierdurch entstandenen Bewegung sei darauf hingewiesen, dass das ganz allgemein und auch gerade bezüglich jüngerer Kräfte so reich ausgestattete Berlin in allerletzter Zeit zwei der bedeutendsten Vertreter der neuen Richtung gewonnen hat, resp. zu gewinnen sucht: Einstein in eine akademische, Laue in eine Universitätsstellung.”

Laue, after his famous experiments with Friedrichs and Knipping, had become associate professor and successor to Debye (and thus to Einstein) at Zurich in 1912. However, he did not go to Berlin but finally accepted a position in Frankfurt, which earlier had also been offered to Debye.¹⁷⁶ Here the physical society and an academy for commerce and social sciences had just been transformed into a full-fledged university with a physics department. At the same time, Born was offered an appointment in Berlin from Planck.¹⁷⁷

Debye thus belonged to a prestigious group of sought-after physicists. The Dutch physicist also had hopes for succeeding Lorentz in Leiden, who wanted to dispose of his university duties to concentrate on research. Presumably this convinced Debye to go to Utrecht as a tactical move once Einstein had declined the offer, after learning from Lorentz that his chances to move up to Leiden were low. Despite the efforts of Sommerfeld and Einstein, it was Ehrenfest who became Lorentz's successor in fall 1912, leaving Debye to seek other opportunities.¹⁷⁸ He soon received appointment offers from Tübingen, Frankfurt and Zurich and was considered as a candidate in Heidelberg, and presumably elsewhere too.¹⁷⁹

5.4.1 Getting a Professor Without a Chair

The fact that Debye came into the focus of Göttingen was still rather coincidental. Initially, Einstein had been invited as one of the central contributors to the *Gaswoche*. He, however, declined Hilbert's invitation.¹⁸⁰ Probably at the "Pre-Gaswoche," i.e. the session of the 1912 Münster *Naturforscherversammlung*, Hilbert may have mobilized Sommerfeld to make Einstein rethink his decision. Sommerfeld, however, had soon to report the final decline and promoted Debye as alternative.¹⁸¹ Debye, however, wrote in a letter to his teacher that he had not heard before of this meeting, of which he

absolutely [did] not know what it shall be like or when it shall take place. That you have tried to get me there has pleased me and I really thank you very much and am doing so gladly. You must not take it amiss when I now have a feeling

¹⁷⁶Voigt to Naumann, 20 May 1914 (document 8) and Dekan to Minister, 28 May 1914, GSStA PK 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 71–73 and 74–76.

¹⁷⁷It turned out that Laue nevertheless preferred Berlin, but was too late to communicate this by Planck. Cf. (Born 1975).

¹⁷⁸Cf. (M. J. Klein 1970, 181ff). For an example of Einstein's assessment of Debye cf. (Debye 1954, vol. V, 374) (Debye to Julius 18 December 1911) and note 11 on p. 376 reporting of Einstein's statement "This man is more able than I am; he has greater talent and more knowledge, but up to now he has been less lucky." There is an account by Debye about the hopes involved in a letter to Sommerfeld of 3 November 1912, see document 2.

¹⁷⁹Debye Interview 1962, session II, p. 21 (AHQP); Dekan Körte to Minister, 20 May 1914 and 28 May 1914 (= document 9), *ibid.* Bl. 71 and 75; Sommerfeld to Wien, 29 November 1913: "Laue habe ich inzwischen (neben Debye u. Mie) auch für Frankfurt vorgeschlagen. Wachsmuth hatte sich bei mir nach Debye erkundigt."; Wachsmuth to Wilhelm Wien, 5 January 1914, DM, Wien Papers, box 001; Lenard to Sommerfeld, 25 September 1913, DM, Sommerfeld Papers HS 1977–28/A, 198: "dass Manche, die ich für sehr wünschenswert gehalten hätte schon in Stellungen sich befinden, die uns Verzicht auferlegen. [...] dass ich bei meinen Briefen aus Garmisch gar sehr auch an Debye dachte, den ich mir aus Ihren Schüler vorstellte, bis ich vorgestern von Zeeman, der hier war, erfuhr, er sei ord. Professor in Utrecht! Das hat mich so betroffen, wie wenn Jemandem gezeigt wird, wie gering doch seine Macht ist."

¹⁸⁰Einstein to Hilbert, 4 October 1912, (ECP5, 502). "[...] Leider bin ich aber nicht in der Lage, dieselbe anzunehmen. Denn einerseits weiß ich zu den behandelten Problemen durchaus nichts Neues vorzubringen, andererseits bin ich mit anderen Dingen bereits vollauf beschäftigt, [s]odass ich der Angelegenheit nicht die nötige Zeit widmen könnte."

¹⁸¹Sommerfeld to Hilbert, 1 November 1912. Appended to letter from Einstein to Sommerfeld, 29 October 1912, (ECP5, 505f.)

better not to appear there suddenly as an uninvited guest. [...] I do not want to be the intruder who belongs into the kitchen but has now come into the parlor where he is then tolerated kindly.¹⁸²

It seems that Debye was in a process to become independent of his physics father Sommerfeld, who urged him to go after Lorentz's position. From the letter it appears that he did not think that he belonged into the parlor of the physics community peopled with Planck, Einstein and Laue. But what did Debye stand for at this time, and what made him particularly interesting for Göttingen?—Basically, it was the fact that he was a quantum physicist.

Debye's reputation was definitely linked to his contributions to the quantum discussion from around 1912 until 1914. He got interested in quantum problems in 1910, when he became *Privatdozent* in Munich: “[...] the first course I gave was radiation theory. Then I made this whole business of Planck and so on,” Debye recalled later. Asked by Thomas Kuhn about his feeling about the quantum at that time, Debye responded that “Planck was illogical” and his main aim was to “get rid of this illogical part” and to find a real probabilistic derivation, which would be more widely accepted. At this time before 1911, Sommerfeld had appeared to him not really interested in these problems.¹⁸³

When he succeeded Einstein at the University of Zurich, he not only took over Einstein's position but also some of his topics like specific heats: “I considered that Planck's formula should be applicable to any kind of vibrating system. Such was in my opinion a solid or a lattice of atoms [...]”¹⁸⁴ In Utrecht Debye had planned his teaching topics for summer 1913 already before his involvement with the *Gaswoche* meeting. “My lectures proceed as usual,” he confided to Sommerfeld, “I teach mechanics since it is necessary and the kinetic theory of magnetism and dielectrics since I enjoy it [...] For the second term I have listed 1) thermodynamics 2) Nernst's theorem and quantum theory.”¹⁸⁵ Comparing this to Hilbert's interests at the same time—his presentation of a paper on the Nernst theorem to the physicists in January 1913, the purpose of his physics assistant Landé, and his teaching topics—the parallels are striking: Hilbert must have been surprised that Debye had anticipated all his interest. It must have been for this reason that Debye was actually invited to the Wolfskehl Congress upon Sommerfeld's suggestion as a replacement for Einstein. In this way Debye became the proponent of an Einsteinian topic, while the initiator was deeply absorbed in a theory of gravitation.¹⁸⁶

¹⁸²Document 2.

¹⁸³Debye 1962, session I, (AHQP), p. 9, 10, 11. TSK: How did you feel about the quantum in this period? About the energy discontinuities for instance? DEB: Well you can see this from the one thing which I published [1910 f) ?] [...] You know this thing, quantization of the space? My whole business was, Planck was illogical; on the other hand it looks as if the whole thing is very good. Can we get rid of this illogical part? That was the real reason. [...] I can come out with my quanta. [...] I also really learned, from S., never take anything from anybody else. Really start with the things from the beginning. [...] Thermodynamics was something which he [Langevin] did not like very much.

¹⁸⁴(Debye 1965), Debye Interview 1962, session I, p. 11 (AHQP). “In the meantime I had succeeded Einstein at the University of Zürich. I considered that Planck's formula should be applicable to any kind of vibrating system. Such was in my opinion a solid or a lattice of atoms, since by using proper coordinates this mechanical system could be described as an agglomeration of a number of independent vibrating systems, each with its proper frequency.”

¹⁸⁵Document 2.

¹⁸⁶Sommerfeld to Hilbert, 1 November 1912, (ECP5, 506): “Einstein apparently is so deeply mired in the gravitation problem that he turns a deaf ear to everything else [...]” “Einstein steckt offenbar so tief in der Gravitation, dass er für alles andere taub ist[...].”

Debye was by far the youngest speaker at the Göttingen conference in 1913. He contributed the longest paper to the proceedings, and he offered an open research program that employed the quantum hypothesis, for which he presented a general formulation to tackle problems of solid state properties like heat conduction. He assumed that the atomistic picture of solids “corresponds in all essential points with reality” and that “at the current, quite uncomplete state of our knowledge about quanta and the reason for its occurrence, it seems desirable to test this extension [of physical theory] experimentally, if possible, [...]” (Debye 1914, 21 and 29).¹⁸⁷ His paper “Partition function and quantum hypothesis with an appendix on heat conduction” thus combined quite attractively atomistic reasoning and quantum theory with well-established, more classical fields of research like thermodynamics and heat conduction. This made him not only the only available quantum physicist for Göttingen, but also the ideal mediator between Riecke and Voigt on the one hand, and the other mathematically trained researchers invading the territory of physics like Hilbert, Born, Courant and others. At the same time, he also stood in contrast with his teacher Sommerfeld, who insisted on publishing of a joint paper that attempted to explain quantum effects in classical terms; “Sommerfeld thought it was a possibility. I was convinced it was no possibility [...]” he recalled later.¹⁸⁸ As Walther Gerlach had suggested, he said that it was “very probable that Debye had decisive influence on Sommerfeld’s quantum physical views” and found traces thereof in their joint publication on the photoelectric effect of 1913. Debye’s role was rather decisive for the history of quantum physics (Gerlach 1967).

To sum up, we find that Debye (like Born) was an independent quantum physicist, not put on track by his master but responding first and foremost to the indisputable failures of classical theory. The freedom and need to occupy new fields of investigation after habilitation and in teaching subsidiary subjects and special fields made these young physicists take Planck’s hypothesis seriously and seek exploratory applications. For Hilbert, who, as we have seen, tried to fill a gap in Göttingen physics and was driven by a mixture of personal interest, disciplinary imperialism and interdisciplinary collaboration, Debye must have appeared the best candidate for this. It was he, who promised to bring the quantum into the whole of physics, both theoretically as well as experimentally, and to allow the influence of mathematics to persist in this field, thus securing the disciplinary encroachment.

Although it may seem obvious that Voigt and Riecke had to be replaced, due to their advanced age and their complaints about the teaching workload, there was no position to be filled in 1914. Instead, a position was created for Debye. Who was instrumental in this step?

All recollections agree with the fact that Hilbert and nobody else was responsible for the decision to get Debye. Walther Gerlach, for instance, recalled that in

¹⁸⁷“Wir werden im folgenden versuchen, wenigstens andeutungsweise, in das Wesen dieser Gesetze einzufordern in der Überzeugung, daß das Bild, welches wir uns von einem festen Körper machen, in allem Wesentlichen der Wirklichkeit entspricht.” p. 21. “[...] aber bei dem jetzigen ganz unvollkommenen Stande unserer Kenntnis der Quanten und der Ursache ihres Auftretens scheint es wünschenswert, diese Erweiterung wenn möglich experimentell zu prüfen, um so mehr, als in dieser Weise nicht das veränderliche Energiequantum, sondern das universelle Wirkungsquantum die führende Rolle übernimmt.” p. 29.

¹⁸⁸Debye 1962, session I, p. 16 (AHQP), with regard to joint publication 1913 (Debye 1913, 12); “kind of a frustration [...] I would never have published it if Sommerfeld had not insisted,” and on the attempt to explain the quantum with classical things, “Sommerfeld thought it was a possibility. I was convinced it was no possibility [...]” p. 16.

1914 David Hilbert succeeded in getting Debye called to Göttingen, although both professorships of Riecke and Voigt were occupied—Debye’s 1913 ‘Wolfskehl’ talk had filled him so with enthusiasm, and Woldemar Voigt created the material possibility [äußere Möglichkeit] by clearing part of his institute. (Gerlach 1967, 224)

The result, according to Friedrich Hund, was that

With Debye’s coming as a "personal" and full professor for the winter term 1914 Göttingen moved up to one of the prime positions in Germany concerning physics, to the rank of theoretical physics as defined by Planck, Einstein, v. Laue, Sommerfeld and Debye. A rare example for seizing an opportunity by a faculty without an opening available. (Hund 1987, 57)¹⁸⁹

The organization of this unique undertaking was not simple, as Debye seemed to have realized his value and recovered his self-confidence during his term as guest professor. When it was rumored that Zurich would offer a salary of 20,000 Marks, Göttingen took pains to offer a lucrative salary, yet remained far from meeting this level. Eventually, the amount for a Prussian associate professor was supplemented by a contribution from the Wolfskehl fund and from a private donor. In August 1914 Debye signed an agreement that made him budgetary associate professor and personal full professor for theoretical physics, co-director of the mathematical physical seminar, and head of its mathematical section, with its budget doubled and extended by a position for a technician.¹⁹⁰

The private donor who made Debye’s coming possible turned out to be Woldemar Voigt, who, in fact, sacrificed half his base salary.¹⁹¹ Looking at his relation to Debye’s prospective activities, one can only find rather unspecific reasons: Debye’s vigor “needs for its work free room” and he would see Voigt’s remaining as head of the department “naturally as a restriction.” As far as specific suggestions were concerned, Voigt wrote:

Concerning the wishes the new director will raise, I would like to to ask for the following: *to create a theoretical physics institute for the university that conveys the broadest possible contact between theory and observation, in a manner according to the ideals of my teacher Franz Neumann.* Clearly, such an institute cannot exist at *any* university [...]¹⁹²

However, there is no indication, that Debye felt obliged to follow Neumann’s school rooted in mid-19th century physics in any way. As to finances, Voigt added the need for a constant supply of funds to retain the standing of the institute that had required so much pain and sacrifice in the past, “hardly a new director will be in the position and willing to spend similar contributions as I did in the interest of establishing and improving the institute.”¹⁹³ A

¹⁸⁹“Durch Debyes Kommen als persönlicher und o. Professor zum Wintersemester 1914 rückte Göttingen bezüglich der Vertretung der Physik an eine der ersten Stellen Deutschlands auf, in den Rang der theoretischen Physik, der durch Planck, Einstein, v. Laue, Sommerfeld und Debye bestimmt war. Ein seltenes Beispiel dafür, daß eine Fakultät zugriff, ohne daß eine Vakanz vorhanden war.”

¹⁹⁰Agreement between Elster and Debye, 21 August 1914, GStA PK 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 92–93.

¹⁹¹Klein and Hilbert to Minister, 29 January 1916, see document 15.

¹⁹²Dokument 6a.

¹⁹³Document 8.

number of other instances could be found of Voigt's charities to his own field; for example, he had personally stepped aside for Runge, and paid Walter Ritz when he fell ill after his doctorate.¹⁹⁴

As mentioned, the Wolfskehl funds provided an equal contribution to Debye's salary, since his offer from Zurich entailed a salary of about 20,000 Marks while the ministry could not offer more than 12,000 Marks.¹⁹⁵ The problem was remedied by the Göttingen professors, in provision of at least half of the difference from other sources, viz. Voigt and Fermat. In this way, Debye did even better than Laue in Frankfurt, where the young Nobel laureate had just secured some 14,000 Marks.¹⁹⁶ When in 1916 Voigt was no longer able to further sacrifice his share of the hidden support to his successful colleague, who was not even half his age, both Hilbert and Klein had to write to the ministry confessing what had happened. The colleagues had taken measures to keep Debye after his guest professorship in Göttingen despite a better offer from Zurich, and this was achieved "as has probably not explicitly been communicated to your Excellency" in the way described above. And things were even more complicated, as Debye did not know anything about this deal since the money was transferred through the curator; yet in principle, it was his duty as the executive director to negotiate such changes in remuneration.¹⁹⁷

As it took the efforts of three mathematicians to convert Debye's appointment into a regular one in 1916; by now, it should have become clear how central for and how much in line with Hilbert's interests it had been to hire Debye in 1914, and how little his work overlapped with the research programs of Voigt or other physicists' in Göttingen.

Despite the fact that Debye was hired as a theoretician and director of the section of mathematical physics of the physics institute,¹⁹⁸ he recalled that the main reason for accepting the Göttingen offer was his access to the laboratory.¹⁹⁹ This was of relevance for him because also his theoretical work was mostly preparatory with respect to experimental investigations, or, as Friedrich Hund characterized his approach, with reversed emphasis: "His experiments were planned from theory" (Hund 1967, 63).²⁰⁰ At least this fact, that Debye was able to combine theory and experiment in a way that promised a certain continuity of the Göttingen tradition may have given the physicists some hope.

¹⁹⁴Iris Runge, for example, reported that Voigt voluntarily gave up his "etatmäßiges Ordinariat" (budgetary/permanent chair) for Runge to be called on such a position from the beginning (I. Runge 1949, 124). This, however, would have precluded later consideration of his position as an ordinary professorship, which it surely did not. It was also Voigt who should have participated in bringing financial support to the ill Ritz (Born 1975, 146). Cf. also (Försterling 1951).

¹⁹⁵Elster to Hilbert, 27 July 1914, Hilbert Papers, folder 93, Nr. 9.

¹⁹⁶Agreement of Elster and Laue, 7 August 1914, University Archive Frankfurt, Personal-Hauptakte v. Laue, Abt. 14, Nr. 140, Bl. 1–2.

¹⁹⁷Document 15. It took a second letter from Constantin Carathéodory to the minister of 15 March, 1916 before the ministry acted, GStA PK 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 438–440.

¹⁹⁸Letter of appointment, Minister to Curator, 19 September 1914, UAG Debye Personalakte 4/V b/278. "[...] die theoretische Physik in Vorlesungen und Übungen zu vertreten; zugleich bestelle ich Sie zum Mitdirektor des Mathematisch-Physikalischen Seminars, sowie—von Sommer-Semester 1915 ab—zum Direktor der Abteilung für mathematische Physik des Physikalischen Instituts."

¹⁹⁹Debye 1962, session I, p. 2 (AHQP): "I went to Göttingen because I could have the laboratory there." Also in his Utrecht appointment his claim for the laboratory was known and "feared." Cf. (ECP5, note 6 on p. 348): "In its recommendation to the Curators the Faculty had already expressed its fear that Debye would not accept an appointment unless he were given a laboratory."

²⁰⁰"Seine Experimente waren von der Theorie her geplant."

5.4.2 From Hilbert's Plans to Debye's Atomic Program

There is no indication that Hilbert considered his engagement in physics to be temporary, he did not intend to serve as a stopgap physicist, but rather as a new kind of physical researcher equipped with mathematical tools that had yet to be exploited. Therefore, the rather long-term perspective of bringing mathematics to fruition within the physical sciences was one driving power in a process, at work in Göttingen, one which I will treat in more detail in this section.

Hilbert's talk on Nernst's theorem at the *Physikalische Gesellschaft* in January 1913, which was mentioned above, can be seen as his personal expression to work seriously in physics, while the April *Gaswoche* appears to be the public manifestation of his commitment to advancing physical theory, or an area he just called "theoretical research fields." Hilbert used this term when he applied to the ministry in July 1913 for funds to institute the exchange of guest professorships in order to promote "theoretical research" as distinct from the "experimental sciences," which were already energetically patronized by the Kaiser Wilhelm Society at this time.²⁰¹ The fields in question were, besides mathematics, specifically mathematical astronomy, theoretical physics and mathematical epistemology. More generally, Hilbert was concerned with "the cultivation of the theoretical side of the natural sciences" which had stressed as a "necessity and duty just as important as the exploration of the experimental facts of the natural sciences."²⁰²

Hilbert's interest in progress in quantum problems was clearly directed to understand the structure of the atom, as one can see from a letter to Hugo Andres Krüss in the Berlin ministry, which he sent in October 1913, and thus before Bohr's new ideas had drawn attention to atomic physics. After sketching the great success of relativity theory, Hilbert turned to the writings of Poincaré and Picard that "give continuous demonstrations that the most important progress in the natural sciences is achieved by connecting the bordering fields." As Picard had written that certain works can only be done by a mathematician in collaboration with a physicist, Hilbert points out that he clearly sees such a question:

And if I should name such a task to you that can only be solved by mathematicians and physicists together than it is the analysis of the structure of the atom, a great problem that until recently appeared almost inaccessible but to which all lines of thought [Gadankenfäden] now seem to lead, although its definitive solution is probably still far away.²⁰³

The mentioning of Poincaré and Picard—who according to Mary Jo Nye were those who made atomism "acute" (Nye 1972, 38–40)—on the one hand, and the statement that "the important works on the structure of matter are currently appearing in particular in England

²⁰¹ Document 3; separate remarks on this issue by Hilbert, Hilbert Papers, folder 494, Nr. 8, Bl. 19–20 (also in GStA PK 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXVII, Bl. 156–157): "Actually, I consider the 5000 Marks requested from the Royal Society of the Sciences only as a basis for the promotion of the theoretical research fields; I hope that once we have the respective sum in the budget of the society it will be possible to win foundations from private sources, as the use of the Fermat funds proved a success [...]" "Überhaupt betrachte ich die von der Gesellschaft der Wissenschaften beantragten 5000 M. nur als einen Grundstock zur Förderung der theoretischen Forschungsgebiete; ich hoffe, dass, wenn wir erst die genannte Summe im Etat der Gesellschaft haben, es möglich sein wird, Stiftungen von privater Seite zu gewinnen—hat doch die Verwendung der Fermatgelder sich so gut bewährt [...]"

²⁰² Document 3.

²⁰³ Document 5.

and Holland,” on the other, demonstrated the “yawning gap [klaffende Lücke] in our current scientific enterprise.” It was the task for the guest professorships to guarantee that Göttingen would not fall back in the international competition.²⁰⁴

The net of connections Hilbert had woven with physics proved very efficient when his physics assistant Paul Ewald, who had witnessed the discussions about Bohr’s model at the meeting of the British Association for the Advancement of Science, right away drew Hilbert’s attention to Bohr’s publication in December 1913. “What’s your opinion of Bohr’s atomic model?” Ewald asked, commenting “At least, it is extraordinarily interesting [...]” He also advised Hilbert to consult “the new article by Moseley in *Phil. Mag.*” in which characteristic X-ray spectra were discussed, which quickly were taken as confirmation of Bohr’s model.²⁰⁵ Interestingly, Niels Bohr also had an informer in Göttingen, who told him back in fall 1913 that the only one in Göttingen who might have dared to consider his ideas “objectively right” was Hilbert—so wrote his brother Harald, who had become a frequent visitor in Göttingen after Landau had first invited him in 1909.²⁰⁶

At this time, Debye was not yet on Hilbert’s mind. In January 1914, Lorentz was the only candidate for the guest professorship for whom Hilbert had been fighting. Lorentz would be “a shining example and precedent for a guest professor,” and this would be most helpful “in the upcoming battle for the 1915 budget.”²⁰⁷ After Einstein declined to participate in the *Gaswoche* and Sommerfeld promoted his own Dutch disciple, things changed quickly to Debye’s favor. A few days after he had begun his term as guest professor, Hilbert was already using his presence as ammunition in his battles with the ministry. He promoted his physics assistant with the argument that due to the guest professorships

I have to draw on my assistants to a particularly high degree: By funds appropriated partly from the Wolfskehl foundation, partly from the ministry and the Royal Scientific Society, we succeeded in obtaining two visiting professorships, namely Debye [sic!]- Utrecht for theoretical physics and Haar- Klausenburg for theoretical astronomy.²⁰⁸

Six weeks into his Göttingen visit, Debye was negotiating in the Berlin ministry for a permanent position. Together with Voigt he sat face-to-face with the director of the ministry of culture Otto Naumann, who had succeeded legendary Gustav Althoff in 1907, and *Oberregierungsrat* Ludwig Elster. Here the maximum salary of 12,000 Marks had already

²⁰⁴Document 5.

²⁰⁵Ewald to Hilbert, 18 December 1913, Nachlaß Hilbert, Mappe 95, Nr. 2: “Was sagen Sie zu dem Bohr’schen Atommodell? Es ist zumindest ganz ungewöhnlich interessant, vergl. auch die neue Arbeit von Moseley in *Phil. Mag.*” Ewald had also reported in Munich on it 19 November 1913. (Eckert 1993, 52).

²⁰⁶Harald Bohr to Niels Bohr, undated (fall 1913), (BCW2, 126 and 567).

²⁰⁷Hilbert to Krüss, 10 January 1914, Hilbert Papers, folder 494, Nr. 10, Bl. 33. “[...] möchte ich nunmehr alle Mittel anstrengen, um H. A. Lorentz Leiden dieses Frühjahr doch hierher zu bekommen – nicht um mindestens auch deshalb um ein glänzendes Präjudiz und Vorbild eines Gastprofessors zu schaffen, das uns später bei dem Kampf um den Etat für 1915 nützlich sein soll.”

²⁰⁸Hilbert to Elster, 6 April 1914, GStA PK 76, Nr. 591, Bl. 210. “Ich gestatte mir noch zu bemerken, dass ich im kommenden Sommersemester meine Assisten[ten] in besonderem Grade in Anspruch nehmen muss: es ist nämlich teils aus der Wolfskehlstiftung, teils aus dem vom Ministerium und der Kgl. Gesellschaft der Wissenschaften bewilligten Mitteln zwei “Gastprofessoren” nämlich Debye [sic!]-Utrecht für theoretische Physik und Haar-Klausenburg für theoretische Astronomie zu engagieren gelungen.”

been negotiated.²⁰⁹ A few days later Voigt offered the ministry to hand over to Debye his directorate of the section B of the physics institute,

because Debye, who is the most wanted candidate of the faculty, had been offered [...] a significant independent institute from Zurich and now also from Frankfurt, and will not be attracted under any other conditions [...] A strong creativity, as we find with Debye without doubt, necessitates room for development [...]²¹⁰

In order to make the case for creating a position for Debye even more plausible, various kinds of resources were mobilized. Voigt, so the dean, had declared that he would not be “able to do justice to the new profound developments, particularly as his research areas lie in other directions,”²¹¹ while Voigt himself used his planned participation in professor exchange scheme with Harvard, where he was scheduled to go in fall 1914, as leverage; only if the Debye deal worked out properly would he go to America.²¹² Eventually, the whole package worked out and Debye was able to finalize his arrangement on 27 July, 1914, when he went to Berlin, together with Hilbert.²¹³ Two points still had to be fixed, however, as the ministry insisted on the initial agreement of 16 May, which it kept resending to Debye and Hilbert in order to establish the salary maximum and to ensure that there was no room for renegotiating institutional issues.²¹⁴ These were more than subtleties, ultimately left to the Göttingen faculty and resulting in the extraordinary and partly secret arrangements.

The successful contest for Debye was finally reported by the minister to the Kaiser, in this way taking on a certain national significance which, actually would become more visible when Debye was later lost to Switzerland. But now Wilhelm II was told that a second chair for theoretical physics had been established in Göttingen and that Debye's research had already produced results that had “caused quite a stir,” proved by his election to the Academy of Sciences in Amsterdam. After his term as guest professor, the Göttingen philosophical faculty now welcomed the inclusion of the “ingenious scholar” among its professors.²¹⁵ While this verdict on the part of state bureaucracy appears mainly motivated by the need to justify the creation of the new position for Debye, soon Hilbert, too, had nothing but praise for his new colleague, whom he called in letter, drafted for Ludwig Elster in the ministry, “a Newton of Chemistry,” which he then corrected to “a Newton of molecular physics.”²¹⁶

²⁰⁹Meeting on 16 May 1914, cp. Minister to Curator 25 May 1914, GStA PK 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 73, Elster to Hilbert, 23 July 1914, Hilbert papers, 93, Nr. 9.

²¹⁰Document 8.

²¹¹Dean (Körte) to Ministry, 28 May 1914, GStA PK 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, 74–76, 74v.

²¹²Voigt to Lorentz, 5 July 1914, Lorentz Correspondence V (AHQP/LTZ-5).

²¹³Elster to Hilbert, 23 July 1914, UAG Hilbert papers 93, Nr. 9.

²¹⁴Elster to Debye, 21 August 1914, and Elster to Hilbert, 22 August 1914, Hilbert Papers, folder 93, Nr. 10 and 11.

²¹⁵Minister of Culture to Majesty, 2 September 1914, GStA PK 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 98. “haben seine Forschungen zu Ergebnissen geführt, die allgemeines Aufsehen hervorgerufen haben. [...] Die Philosophische Fakultät daselbst würde es begrüßen, den genialen Gelehrten dauernd in ihren Lehrkörper aufnehmen zu können.”

²¹⁶Document 10. Cp. also (Schirmacher 2003) p. 16. The hope for a “Newton” who could solve the puzzle of the spectroscopic lines of the atoms had been mentioned at Göttingen by Carl Runge in 1912. He drew a parallel with Newton, who had put Kepler's astronomy on a firm basis, but now with respect to the great step atomism was waiting for. (C. Runge 1912), cited in (I. Runge 1949, 153). “The present situation of spectroscopy can be compared with that of astronomy before Newton's discovery of the laws of gravitation. Kepler's laws have been found but the proper mathematical-physical hypothesis is absent. Without doubt, it will appear and will wrap a band around chemistry, physics, and mathematics.”

Debye's research agenda, on which he embarked in fall 1914, can be characterized, roughly speaking, as a combination of Bohr's theory and Laue's experiment. Unlike other experimental physicists, in particular James and Franck and Gustav Hertz, who had conceived their concurrent research in order to disprove Bohr's theory (Hon 1989), Debye was the first in Göttingen besides Hilbert to take Bohr's model seriously. He immediately conceived an experimental verification (as Born did shortly afterward in Berlin). As he summarized in a paper on the constitution of the hydrogen molecule, which Sommerfeld presented to the Bavarian Academy of Sciences on 9 January, 1915 (Debye 1915a, 1).

Through the newest work by Rutherford, Nicholson, and especially Bohr it has become very probable that atoms are 'planetary systems' in which nuclei of extremely small dimensions and considerable mass on the one hand and the usual electrons on the other hand revolve around each other.

However, this new conception could not be reconciled with the older theories like the dispersion theory of Kettler, Helmholtz and Drude. For this reason Debye wrote, he "would like to show that the [...] planetary system hypothesis suffices completely to explain the entire optical properties of hydrogen." At this point he believed already that a combination of celestial mechanics and the quantum hypothesis, which accounted for the discreteness, was the right way to understand microphysics. Furthermore, he found it "remarkable" that the perturbation calculations for the hydrogen molecule "can entirely be done exactly as one would expect from the mechanical laws [...] for a real planetary system." And therefore, he viewed his results on dispersion to be an "indisputable success," calling for "as many fields of phenomena as possible" to be calculated in this way. "Of course," he added, "one can hope to get involved in contradictions with the experiment from time to time and thus achieve some valuable illumination of the nature of the quantum of action" (Debye 1915a, 2, 3, 24).

Debye's approach anticipated to some extent the later Göttingen strategy initiated by Born to press classical theory to its limits in order to find a way leading towards quantum mechanics. However, there were two fundamental differences that cannot be ignored. At the time, when Born started his program he was already quite sure that the Bohrian picture could not hold, and neither did he believe that experiments would help at this stage.

On the occasion of the presentation of the Max Planck medal to Debye *in absentia* in 1950, Sommerfeld pondered on the "sensational model" for the hydrogen molecule, which emerged from Debye's paper. "This model, like my own generalizations [...] in 1915, has since joined the dead," he confessed, while "Today it is probably only of interest for showing how naive-optimistically one dealt with atomic and molecular models" (Sommerfeld 1950). Or as Debye recalled later, "everybody was really convinced after you saw how the Balmer theories came out [of Bohr's model]." And he justified his research program, saying that at "that time there was no question that if you wanted to do something more modern [...] you had to apply [the] quantum."²¹⁷ It was, in fact, Sommerfeld who followed the lead of his former pupil in 1915. He took up his ideas for the hydrogen molecule and expanded upon them; hence, at the meeting of the German Physical Society in January 1916 Walther Nernst would speak on the "Bohr-Debye model"; only later did the combination Bohr-Sommerfeld become canonical.²¹⁸

²¹⁷Debye Interview 1962 session II, p. 5 (AHQP).

²¹⁸Max Planck discussed the model in Prussian Academy of Sciences (Planck 1919).

Hilbert was also able to capitalize on this increased standing for Debye and his research within the physics community also, in particular when addressing state officials. Two letters Hilbert sent on 1 January and 24 December, 1915, communicate the favorable results Debye's arrival had created. While in early 1915 the very letter in which Hilbert called Debye a "Newton," the central improvement was claimed to be better cooperation between mathematicians and physicists, through Debye the mathematical seminar had become the place of discussion where "almost all mathematics and physics staff participate" and the debates became "scientific feats." Debye, who for Hilbert had become a kind of substitute for the late Minkowski, had created "in particular due to his latest discoveries from around Christmas [1914] the foundation of a new *mathematical* chemistry long sought in vain, and believed to be still a long way off."²¹⁹ In the course of the year, however, Debye's theoretical feats had been surpassed by his experimental ones. The main aim was now, Hilbert explained to the Berlin Ministry, "the *experimental* investigation of matter down into the interior of the single atom." This was done, as Hilbert had already communicated in an earlier letter, by something like a "ultimately refined microscope," which works with X-rays instead of light and which had allowed the distance between two atoms within the benzene molecule to be measured as "0.000 000 62 m.m." In this way, "an unforeseeable new field of *experimental* research is opened up"; Hilbert added that one could dare say that "from now on no question with regard to the structure of matter can remain unsettled, no matter how great the effort necessary to this aim."²²⁰

The fact that Hilbert did not even shy away from presenting a specific measurement value in his communication with the state authorities, demonstrates that Debye, who was hired to fill the position of a theoretical physicist, had not only moved more and more toward experiments, but also saw that experimental insights were the key to the physics of quanta and atoms.

The research involved a special apparatus, a kind of camera, which Debye built together with Paul Scherrer in 1915 or 1916, and which served to photograph—in a sense—the electron rings within the atoms, which Niels Bohr had proposed. This "ultra-microscopy of the interior of the atom" was, clearly, related to the previous results and theoretical interpretation of X-ray scattering in the Laue experiment. Similarly, Debye was convinced, that "it must be possible in this way to establish by experiment the particular arrangement of the electrons in the atoms." However, he added, "Whether, experimentally, rings are actually photographed or a continuous deviation from the scattering laws for dipoles is established"

²¹⁹Document 10, emphasis added. "Durch den Beitritt[?] Debye hat das math. Seminar hier ein sch.. Höhe.. daß fast alle math.-phys.ischen Lehrkräfte daran teilnehmen und die Dispute [?] dann gestalten sich zu wissens. Taten. Debye ist erweist sich [als] der Newton der Chemie Molekül Physik und wir haben jetzt insbes. durch seine neuesten Entdeckungen um Weihnachten [1914] herum die solange vergeblich gesuchte in weiterer Ferne geglaubte Grundlage einer neuen math. Chemie. So ist zugleich für mich persönlich in wissenschaftlicher Hinsicht Debye wirklicher Ersatz für Minkowski geworden."

²²⁰Document 12, emphasis added. "Das Ziel desselben ist die experimentelle Erforschung der Materie bis in das Innere des Einzelatoms hinein. Die zur Erreichung dieses Ziels verwandte Versuchsanordnung habe ich in meinem neulichen Briefe als ein höchst verfeinertes Mikroskop bezeichnet. Damit hat es folgende Bewandtnis: [...] es wird die von chemischer Seite schon als Hypothese angenommene Ringanordnung der sechs Kohlenstoffatome festgestellt und der Abstand zweier Atome voneinander zu 0,000 000 62 m.m. gemessen. Es liegt schon nach diesen ersten Proben auf der Hand, daß damit ein unabsehbares neues Feld der Experimentalforschung eröffnet ist, und ich möchte – mit etwas Kühnheit – sagen, daß mir eigentlich von nun an keine auf die Struktur der Materie bezügliche Frage mehr unbeantwortet bleiben kann, so groß auch noch die bis zu dem Ziel notwendig zu leistende Arbeit sein mag."

could not be told, but it did not matter so much as long as “it appears to be essential that [...] we are in a position to measure from observations of the scattered radiation, the electron arrangement inside the atoms in centimeters” (Debye 1915b, 811, 823). Eventually, no Bohrian rings could be made visible and by an elegant reinterpretation of the whole experimental setting, as on the one hand, a means to study atoms within molecules rather than the interior of single atoms, and on the other, a technology that would allow Laue's kind of X-ray analysis for powdered specimens and not only for larger crystals.²²¹

If one tries to relate the various influences on Debye's research program and thus on Göttingen physics in general, a complex picture emerges. It incorporates, first, Debye's research interests, which, as we have seen, were somewhat related to Hilbert's, but also his personal style of work and his expectations with respect to atomic theory and speculation. Together these comprise a field of cognitive resources. Yet it also includes, the institutional framework in which a certain division of labor within the physics community and also local experimental opportunities played further important roles.

The difficulties with finding a replacement for Riecke and thus for experimental physics allowed Debye to step into this domain and plan experimental research in quantum and atomic physics. When the director in the Prussian Ministry of Culture, Otto Neumann, finally asked his adviser Wilhelm Wien: “Who else is the right experimental physicist for Göttingen?” he suggested moving Debye to the experimentalist's position, while leaving the theory to Voigt, and thus reducing the problem of hiring a new experimental physicist to that of filling a subordinate position with a younger candidate. In this way Debye and, for example, Robert Pohl, could alternately read experimental courses.²²² Wien replied to the Ministry that theoretical physics had not only managed to occupy most of the laboratory space in Göttingen, it would also be difficult to establish an experimentalist “alongside the incomparably more significant Debye [...]” However, if he “were inclined to take over the professorship of experimental physics,” Wien concluded, the gridlock could be resolved by hiring another theoretician instead.²²³

Although theoretical physics had become a matter of full positions at German universities in the years before the Great War (Jungnickel and McCormach 1986, 303), still more students, more concrete equipment and higher lecture fees lay in the experimental field. When Felix Klein was trying to fix the secret money transfer from Voigt and the Wolfskehl fund to Debye in 1916, he began by pointing out “something about Debye's current financial conditions.” Even during wartime, and only by giving theoretical lectures he would already earn the pay Klein himself had been guaranteed by the Ministry, “and his income from lecture fees will, of course, increase considerably, when he takes over his part of experimental physics.”²²⁴

For experimentalists further sources of funding were opened by the new Kaiser Wilhelm Institute for Physics, which did not yet have any facilities of its own, but was supporting research elsewhere. Encouraged by Born and Sommerfeld, Debye requested a grant of roughly 16,000 marks for X-ray equipment in 1918, at a time when he still believed that

²²¹ Cf. (Schirmacher 2009b, 140–143) for an overview and the articles (1916; 1917; 1918).

²²² Document 13.

²²³ Document 14.

²²⁴ Document 15.

“particularly in the case of crystals we are in a fortunate position to experimentally test the key idea of electron rings for validity.”²²⁵

The decision to pursue X-ray experiments that might demonstrate the reality of electron orbits within the atom and thus literally prove Bohr’s atomic model, however, was influenced by various factors. Besides those relating to the availability of local resources, and thus to laboratory conditions, there were others that were more related to a kind of division of labor within the physics community. Debye’s collaborator Paul Scherrer belonged, so to speak, to the local resources, as he had come to Göttingen in 1912 and was a doctoral student of Voigt’s working on the interplay between the optical and mechanical properties of media known as the Faraday effect. He actually studied it for the case of the hydrogen molecule and thus linked up well with Debye’s interests.²²⁶ Given this preparation, and the fact that the effects of magnetic fields on spectral lines constituted a major issue in atomic physics, one may wonder why the field of research on the Zeeman effect did not suggest itself for joint work by Debye and Scherrer. It would even have established a bridge to the older work of Voigt on this topic and might have provided (or constructed) some line of continuity, cp. e.g. (Voigt 1911a; 1911b). According to Debye’s later recollection, however, there was a rather clear priority for these matters with Sommerfeld:

I had the feeling that Sommerfeld did not like it at all, that I was doing things on quantum theory at the time when I was in Göttingen [...] I was talking about the Zeeman effect and such things. He did not like that. He wanted to have that for himself. So I decided well all right, I won’t do that any more.²²⁷

For this reason, Debye did not continue in this direction after presenting a paper on quantum theory and the Zeeman effect to the Göttingen Academy in March 1916, in which he outlined that, after Bohr’s new model had proved successful to explain the similar Stark effect, the older theories of Voigt and Lorentz for the Zeeman effect could also be replaced by a quantum theory.²²⁸

In the course of the experiments with the Debye-Scherrer camera, which was initially thought to be able somehow to record an image of the interior of the atom, a method emerged that proved paramount for studying the “arrangement of atoms within crystals,” as it had the “advantage of not requiring any larger crystals. It suffices when the substance is present in form of a very fine powder or as microscopic aggregate.” This new interpretation of their research program was demonstrated successfully by Scherrer for the case of aluminum (Scherrer 1918, 23).²²⁹ Debye, too, turned from the idea of an “ultra-microscopy of the interior of the atom” to the structure of molecules in terms of their composition of atoms (Debye 1915b, 811; 1920). Ironically, Hilbert had been right when he had called Debye the Newton

²²⁵Cf. Goenner and Castagnetti in this volume. (Debye and Scherrer 1918, 474). “Gerade bei Kristallen sind wir aber in der glücklichen Lage, die Grundidee der Elektronenringe auf ihre Richtigkeit direkt experimentell zu prüfen; [...]”

²²⁶His dissertation was presented to the Göttingen Academy of Sciences by Hilbert on 5 July 1915 and published as (Scherrer 1915).

²²⁷Debye interview 1962, session I, 21 (AHQP).

²²⁸Presented to the Göttingen Academy of Science on 3 June 1916, published as (Debye 1916).

²²⁹“Die von Debye und mir angegebene Methode zur Bestimmung der Atomanordnung in Kristallen hat den Vorzug, keiner ausgebildeten größeren Kristalle zu bedürfen. Es genügt, wenn die kristalline Substanz in Form eines sehr feinen Pulvers oder als mikrokristallines Aggregat vorliegt. Eine Substanz, von der niemals größere Kristallindividuen beobachtet wurden, liegt im Aluminium vor, und es ist daher unsere Methode in diesem Falle die gegebene.”

of molecular physics, but still he had rather hoped for an axiomatics for the structure of the atom itself.

5.4.3 Teaching

While by now the great extent to which Debye's theoretical and experimental research established quantum and atomic physics in Göttingen has become apparent, and that this came about in a way that could not have emerged from a continuation of the former research programs there, the question remains to be answered as to how much this turn extended to the teaching of physics. Given the fact that Debye's stint in Göttingen from April 1914 to April 1920 was largely restricted to the years of war and revolution, one might even wonder whether his impact on educating a new generation of students was rather negligible, since most students were called up to war duty. However, as we have learned from Klein's letter of January 1916, Debye still attracted quite a number of students. Moreover Debye himself, who had automatically received German citizenship to become a professor in Germany, without, however, giving up his Dutch passport, was considered to live up to the duties of a true German citizen. As Hilbert mentioned in a letter to Elster in January 1915, Debye had received a summons to appear for a medical examination for military service. Debye's assistant Erich Hückel had even described to him the procedure.²³⁰ In the end, Debye had no obligation at all; however, he and the Swiss citizen Paul Scherrer volunteered to contribute to the efforts of Runge and Courant to create an earth telegraph in order to fix the chaotic communication problems Courant had himself experienced first-hand in the trenches on the French front (Reid 1976, 54–56).

Despite war and diminished university life, quantum physicists made scientific progress and delivered some of their more influential theories. So did Sommerfeld, with lectures that he later turned into *Atombau und Spektrallinien*, the postwar bible of atomic theory, and so did Hilbert with general relativity. Similarly, within the four years of war Debye held three full lecture courses especially on quantum theory, and the field also played a role in additional courses (figure 5.5). Later Debye would claim that he taught these topics partly in response to his students' desire to learn about quantum physics.²³¹

Two typewritten copies of notes on his winter 1914/15 lectures about “quantum theory” have survived in the library of the Göttingen mathematics institute.²³² More than 300 pages record, first, an introduction to statistical mechanics (entropy, probability), Planck's theory of black-body radiation and the theory of specific heat treated by quantum oscillators. Here he even presented both his own theory and the treatment of Born and von Kármán, and further referred to not yet published results of Born.²³³ At the end of the lectures, and thus at the time when his paper on the constitution of the hydrogen molecule was published, he treated “a phenomenon that shows that the quantum considerations in the case of rotation must be essentially true.” He was able to demonstrate that the distance of doublet lines can be determined correctly by means of Planck's quantum of action using precise knowledge

²³⁰Hilbert to Elster, 25 January 1915, Hilbert Papers; Hückel 1972.

²³¹Debye, interview 1962, II, p. 2 (AHQP): “I had time for the laboratory and time for some selected topics, but I always picked them by asking the students ‘What would you want?’”

²³²Quantentheorie (winter 1914/15), Mathematical Institute Göttingen, also of AHQP, on microfilm 24.

²³³Ibid. p. 289.

about the moment of inertia from his molecule model, which was known to a precision of two percent.²³⁴

In the following year, “Quantum theory II” was already offered, discussing Sommerfeld’s extension of the Bohr model as well as “Debye’s atomic model,” which was closely related to the experiments he and Scherrer were pursuing to image the Bohr orbits with help of X-rays. He discussed at length how, analogous to the Laue experiments with crystal lattices, the spatial distribution of electrons within the atom should become visible. In particular, he explained why the rapid movement of the electrons would still not wash out the interference patterns. “Whether, in fact, observations of the diffraction of X-rays can be used for the quantitative determination of the molecular dimension, especially for the distances between the electrons in the atom,” he concluded, only “further experimental pioneer work will teach.”²³⁵

Presumably, the lecture courses in the academic year 1916/17 also had some relation to the ongoing quantum effort, as they covered “X-rays” (summer term) and “selected chapters of mathematical physics” (winter term). The third course with a quantum title took place the following year. The existing lecture notes on “new results of quantum theory” were not only more sketchy and less extensive than in the previous years, they also exhibited a more cautious approach and explicitly emphasized the failure of mechanical images to explain the radiation frequencies of the Bohr atoms.

Once one has quanta, frequencies should not be related with mechanical oscillations. This cannot be understood from the point of view of classical mechanics and electrodynamics.²³⁶

Besides acknowledging controversy in interpretation among the different researchers, this state of affairs now demanded a more modest and experimentalist approach:

Today, our point of view is the following: One reflects on theoretical issues in the same way as one does experiments. One uses and starts from the old. The more something proves successful, the more it appears to be true; each new case is a touchstone for the theory. One must no longer think that a frequency

²³⁴Ibid. p. 308–313.

²³⁵Quantentheorie II (winter 1915/16), Mathematical Institute Göttingen, also AHQP, on microfilm 24, p. 146. “Wenn es möglich wäre, Atome, von deren Bau man sich schon ein bestimmtes Bild macht, in vielen Exemplaren hintereinander aufzustellen und die von ihren Elektronen ausgehenden Strahlen interferieren zu lassen, so könnten die Interferenzbilder direkt einen Maßstab für das Atom abgeben. Zunächst liegt die Vermutung nahe, daß die Bewegung der Elektronen die Interferenzerscheinung verwischen würde; aber die Regularität, daß die Abstände der verschiedenen Elektronen auf einem Ringe voneinander erhalten bleiben, genügt, um trotz aller Regellosigkeit eine Interferenz von der Ordnung der Beugung zu liefern, die man an einer angehauchten Glasplatte beobachten kann.” p. 143. “Die Debyeschen Rechnungen erschließen nun die räumliche Intensitätsverteilung des Strahlenbündels.” p. 144. “Diese Tatsache legt den Gedanken nahe, eine Verbindung zwischen diesen und den Friedrichschen Ringen anzunehmen, die dann umgekehrt als Beleg für die regelmäßige Anordnung der Elektronen im Atom angeführt werden können.” p. 145f. “Ob tatsächlich Beobachtungen über die Zerstreuung von Röntgenstrahlen zur quantitativen Bestimmung der Molekdimensionen speziell der Elektronenabstände im Atominnern verwandt werden dürfen, muß weitere experimentelle Pionierarbeit lehren.” p. 146.

²³⁶Neuere Ergebnisse der Quantentheorie (winter 1917/18), Mathematical Institute Göttingen, also of AHQP, on microfilm 24, p. 76. “Sobald man Quanten hat, soll die Frequenz nicht mit einer mechanischen Schwingung in Verbindung gebracht werden. Man kann dies vom Standpunkt der klassischen Mechanik und Elektrodynamik nicht verstehen.”

of an oscillating electron corresponds to each frequency of light. We abandon Maxwell's equations, for they lead to this.²³⁷

In this way the high hopes of (or in) a Newton of molecular physics and a theoretical revolution had scaled back to the much humbler pursuit of tentative steps and experimental corroboration. As there is no direct link between the fall of the German forces and this sort of concurrent capitulation by a research program at a German university it seems as if some general atmosphere was imparted from the political to the scientific realm, be it through the (unknown) student who took the notes, or the scholars themselves, who were fond of framing their scientific feats in a national and revolutionary narrative.

In Debye's postwar—and, with respect to science, probably also “post-revolutionary”—time at Göttingen, his relevant teaching covered not only the general lectures on experimental physics, which he regularly provided since 1916, but also the “structure of spectral lines” (winter 1918/19), “selected chapters of the kinetic theory of gases” (summer 1919) and “crystal structure” (spring 1920).

When he went to Zurich, where he was soon joined by Scherrer, he continued his experimental plans. However, instead of the Kaiser Wilhelm Society, whose grant of X-ray equipment did not arrive in time to be of use to Debye in Göttingen, it was the Solvay Institute that provided funding.

5.4.4 From Local Resources to National Assets: Losing Debye

In spring 1920, Debye left Göttingen for Zurich, where he took over a chair of experimental physics at the Eidgenössische Technische Hochschule. When it came to the stage of losing Debye after the Great War, a broad effort was mobilized to keep him at Göttingen. The context of this episode, which may appear rather straight forward considering the living conditions and remuneration offered, takes on special meaning from a more symbolic and national perspective.

The pride in Debye was obvious. In the words of the Göttingen dean, Debye's work was “known and appreciated by the physicists of all cultural nations.” The fact that Göttingen was able to win him “against the competition of Zurich during the first year of war was a major success for the Prussian educational administration, his loss to the very same competing university after the war, however, would be judged in the whole scientific world as an even greater failure.”²³⁸ This assessment came only ten days after Debye had informed the university about his plans in early November 1919; another ten days later Freiherr von Richthofen, member of the *Verfassungsgebende Preußischen Landesversammlung*, the precursor of the

²³⁷Neuere Ergebnisse der Quantentheorie (winter 1917/18), Mathematical Institute Göttingen, also of AHQP, on microfilm 24, p. 76f. “Unser Standpunkt ist heute der, man stellt die theoretischen Überlegungen genau so wie bei experimentellen Untersuchungen an. Man verwendet und geht vom Alten aus. Je öfter ein Ding sich bewährt, desto richtiger scheint es; jeder neue Fall ist ein Prüfstein der Theorie.” “Man darf nicht mehr glauben, daß einer Frequenz des Lichtes eine Frequenz eines schwingenden Elektrons entspricht. Wir geben die Maxwellschen Gleichungen auf, denn diese führen darauf.”

²³⁸Dean (Hartmann) to Minister, 17 November 1919, UAG Debye Personalakte. “Herr Debye ist durch theoretische und experimentelle Arbeiten bei den Physikern aller Kulturvölker bekannt und geachtet. Seine Gewinnung für Göttingen [...] [gegen] Konkurrenz Zürich im ersten Kriegsjahre stellte einen bedeutenden Erfolg der Preußischen Unterrichtsverwaltung[?] dar; sei Verlust an eben dieselbe konkurrierende Hochschule nach dem Krieg würde in der ganzen wissenschaftlichen Welt als ein um so größerer Mißerfolg bewertet werden.”

later Prussian parliament, put forward an official inquiry asking the government what it intended to do in order to maintain the high scientific rank of the mathematics and science faculty at the University of Göttingen, which had been recognized in the world, in particular in the light of the offer to the Nobel laureate (which Debye was not at this time).²³⁹ Minister of Culture Konrad Haenisch replied that he was willing to try to hold Debye and added that there was no danger Debye would leave for more money.²⁴⁰ After Debye negotiated with the Ministry in late 1919, things became even more urgent and rhetorically laden.²⁴¹ In January 1920 Haenisch wrote to Matthias Erzberger, Vice-Chancellor and Minister of Finance, discussing the national dimension of Debye's impending departure from Germany. It would be a "duty" to do the "utmost in order to avoid the threatening intellectual impoverishment and the resultant elimination in the intellectual contest of the nations." It was a matter of "national respect" and the whole German student body had pledged that it wanted Debye to stay. When Debye informed the ministry that he would not accept their offer, claiming that the decision to go to Zurich was "dictated out of consideration for my scientific work" as well as the "compelling external conditions," but also that he was explicitly asking to be allowed to relinquish German citizenship, thus rubbing salt in the wounding.²⁴²

Although Hilbert observed that Germany was falling into an abyss for six years, while he was writing Sommerfeld on Debye's departure, he himself remained true to his university,²⁴³ though fears had spread, not without reason, that if Debye left the danger would arise that "the famous Göttingen mathematician" Hilbert might also leave.²⁴⁴ His remaining was, however, facilitated by the concessions the ministry made to preclude any further loss.²⁴⁵

Debye left and Hilbert stayed at a crucial time for both the German nation and German science. As Planck had pointed out to the Berlin Academy of Science directly after the lost Great War: "When the enemies have deprived our Fatherland of its military and might, when severe crises hit the interior and probably even more severe ones are approaching, there is one thing that no exterior or interior enemy has taken: it is the standing that German science holds" (Planck 1918). For universities like Göttingen this actually meant more than a general act to console national feelings; rather it was the beginning of new efforts for the betterment of science. Student numbers thrived. For example, the registration for the mathematics

²³⁹Debye to Curator, 7 November 1919, UAG Debye Personalakte; "Kleine Anfrage Nr. 314 des Abgeordneten Frhrn. v. Richthofen," 17 November 1919, GStA PK 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXVI, Bl. 136. "Was gedenkt die Staatsregierung zu tun, um auch in finanzieller Beziehung dafür Sorge zu tragen, daß die mathematisch-naturwissenschaftliche Fakultät der Universität Göttingen den hohen wissenschaftlichen Rang zu behaupten vermag, den sie bisher anerkanntermaßen in der Welt eingenommen hat?"

²⁴⁰Haenisch to all ministers, 5 December 1919, GStA PK 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXVI, Bl. 135.

²⁴¹Meeting on 30 December 1919, see Becker to Debye, 24 December 1919, GStA PK 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXVI, Bl. 159.

²⁴²Debye to Becker, 6 February 1920, GStA PK 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXVI, Bl. 208–209. "Im Zusammenhang mit meinem Entschluss möchte ich Sie bitten, mir die Entlassung aus meinem Dienste zum 1. April 1920 gewähren zu wollen. Wie Ihnen bekannt sein wird, wurde mir nach Uebernahme meiner Stellung in Göttingen im Jahre 1914 eröffnet, dass ich nunmehr preussischer Staatsangehöriger geworden sei. Meine niederländische Staatsangehörigkeit behielt ich daneben bei. Ich möchte Sie nunmehr bitten, mir mit Auflösung meines Dienstverhältnisses zum preussischen Staate auch die Genehmigung zum Wiederaustritt aus diesem Staatsverbände erwirken zu wollen." "Mit der Hoffnung, dass auch mein heutiger, durch die zwingenden äusseren Umstände und mit Rücksicht auf meine wissenschaftliche Tätigkeit diktiert Entschluss ein gütiges Verständnis bei Ihnen finden möge, verbleibe ich [...]"

²⁴³Hilbert to Sommerfeld, 21 January 1920, DM HS 1977–28/A, 141.

²⁴⁴Document 16.

²⁴⁵See (Sauer 2000).

reading room—where one could study the lecture notes that had been taken on Hilbert’s but also on Debye’s courses in particular—rose from 88 in winter 1918 to 211 in summer 1919. Before his departure, Debye himself had reported to the Göttingen Association for the Promotion of Applied Physics and Mathematics that “in the lectures and classes there is a unprecedented number of participants,” while Pohl added that attendance in his practical classes, which served not only physicists but also chemists, medical students and others, had increased eightfold.²⁴⁶ Evidently, Pohl could not compensate for the loss of Debye, even though Runge, Wiechert and some *Privatdozenten* attempted to increase their efforts. Two days before his official duties ended at Göttingen, Debye asked the Curator to make sure that, until a successor has taken office, Runge would look after his doctoral students who stayed behind, so that they “are protected against unjustified encroachments.”²⁴⁷ While Debye left behind personnel resources, deliberately or not, he at least cared about material resources, suggesting to Einstein in June 1920 that the X-ray equipment financed by the Kaiser Wilhelm Society (and hence by the German state and industry during the war) should now be transferred to Zurich.²⁴⁸

At this time the search for a physicist who could fill the void left by Debye was already almost complete. It had quickly become evident that, considering the “wish for as adequate a replacement as possible” for Debye “it would be natural to think of Sommerfeld in Munich and Mie in Halle.” However, it was very clear to the dean that there was no point to hope for Sommerfeld, and instead of Gustav Mie—who probably seemed more old-fashioned due to his approach to physics than for his age—a younger person should be preferred.²⁴⁹

²⁴⁶Protocol Göttinger Vereinigung zur Förderung der angewandten Physik und Mathematik, 31 January 1920, GStA PK 76 V a, Sekt. 6, Tit. X, Nr. 4 Adbih., Heft VIII, p. 55. [Debye:] In den Vorlesungen und Uebungen herrschte eine nie dagewesene Frequenz. Die Aufrechterhaltung des wissenschaftlichen Betriebes war infolge der Geldentwertung nur unter grösster Anstrengung und Verzichtleistung auf manches Wünschenswerte möglich. [...] [Pohl:] Nach Beendigung des Krieges sind in der Abteilung eine allgemeine Vorlesung über Experimental-Physik und ein physikalisches Praktikum eingerichtet worden, die insbesondere für die Bedürfnisse von Chemikern, Medizinern und Naturwissenschaftlern zugeschnitten sind und diesen einen Ueberblick über die moderne Experimental-Physik vermitteln sollen. Die gemachten Aufwendungen mussten die staatlich zur Verfügung gestellten Mittel erheblich überschreiten, doch gelang es, einer gegenüber der Friedensfrequenz auf das achtfache gesteigerten Studentenzahl einigermaßen gerecht zu werden [...]

²⁴⁷Debye to Curator, 29 March 1920, UAG Personalakte Debye. “Ich würde dann wissen, daß bis zur Ernennung meines Nachfolgers die Interessen des Insituts sowohl wie die meiner zurückbleibenden Doctoranden gegen ungerechtfertigte Übergriffe geschützt sind.” In fact, a number of doctoral students were adopted by Emil Wiechert, who served as doctoral adviser for Helmut Schering, Hermann Ebert, Hans Falkenhagen and Erich Hüchel, who, for example, worked on “Zerstreuung von Röntgenstrahlen durch anisotrope Flüssigkeiten.” Only Adolf Krückeberg and Herta Sponer finished in time under Debye, while further theses which were initially be supervised by him came from Franz Wever, Erna Brockmann, Margarethe Meier, Hans Weigt and Luise Lange. Cf. Hochschulschriftenverzeichnis, Library of Humboldt University Berlin.

²⁴⁸Debye to Einstein 7 June 1920, MPGA MPG, I Abt., Rep. 34, Nr. 1, folder Debye.

²⁴⁹Dean to Ministry, 22 March 1920, GStA PK 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXVII, Bl. 73 verso. “Der Wunsch nach einer möglichst vollwertigen Be..zung der durch den Fortgang von Prof. Debye erledigten Professur legte es uns nahe, an Professor Sommerfeld-München und .. Mie-Halle zu denken.”

5.5 The Born “Schools” in Berlin, Frankfurt and Göttingen

Traditional accounts of the history of quantum mechanics often relate events to a few leading figures and their schools, viz. the Sommerfeld school, the Born school and the Bohr school. As suggestive as this picture may be, it tends to oversimplify the more complex state of affairs, in particular when one claims that there was, more generally, a “Göttingen school” or a “Copenhagen school,” since the approach places only the case of Munich in the proper school framework. In Copenhagen we find something more like a postgraduate institute, as the activities of Bohr’s institute depended heavily on constantly changing combinations of postdocs.²⁵⁰ As we have seen for Göttingen, various directions of physical and mathematical, experimental and theoretical, phenomenological and reductionist efforts coexisted and competed. Göttingen physics and mathematics exhibited several smaller learning and research collectives; therefore we can find important groups of scientists that both coordinated their research and fought to extend their spheres of influence.

In the school categorization Peter Debye was a disciple of Sommerfeld. However, he grew more and more independent of his Munich teacher in Göttingen, as he did elsewhere later on.²⁵¹ Similarly, Max Born would turn to new directions at Berlin and Frankfurt after leaving Göttingen in 1915. But, to greater extent than Debye, he managed to remain part of a group of researchers who followed similar lines of investigation, and these collaborations also exhibited very different qualities and forms, which were the result of circumstances much different from Debye’s and Hilbert’s comparatively calm and peaceable Göttingen.

Born took up the extraordinary professorship of theoretical physics in Berlin, which Planck had made possible not without some complications.²⁵² In his memoirs he later wrote a vivid account of his military service in a special group of scientists that was trying to improve German sound ranging efforts. Apart from much anecdotal fare, two main points are apparent. First, Born did not interact much with Berlin colleagues and students in the field of physics, instead he maintained collaborations with friends and colleagues from Göttingen and Breslau, where he also had studied. Secondly, the war experience and his scientific war effort brought him into contact with experimental and applied physics.²⁵³ Hence, like Debye, he turned to experimentation during the war and in its immediate aftermath, although for very different reasons. In any case, when he returned to Göttingen in spring 1921, Born had become a physicist who was also engaged in measurements and empirical research, and he even brought some experimental physics equipment as well as students of experimental physics to Göttingen. Still, he was happy not to be the “full” physicist, which still loomed large as the ultimate goal of every physics scholar and which Debye had so conspicuously represented by jumping more or less effortlessly from theory to experiment.

For Born’s and thus for Göttingen’s route to quantum mechanics, personal experience in experimental physics was an important ingredient, as was the close monitoring of ex-

²⁵⁰ See Alexei Kojevnikov’s contribution to this volume.

²⁵¹ A declaration of independence from his teacher is apparent from an incident in 1916 when Debye angered Sommerfeld by publishing in his domain (Eckert 2013, 52f).

²⁵² Planck withdrew the invitation to Born after Laue had claimed his interest and priority with respect to the Berlin position; however, as Born ultimately failed to convince the ministry of this rather informal bargaining, he was not able to finish his first lecture course—not only were his students called to the front, he himself decided to serve his country before the term had ended (Born 1975, 164)

²⁵³ For the context of Born’s group in World War I and the relation of war science and basic research cf. (Schirmmacher 2009c, 168–172).

perimental results relating to quantum effects in atomic physics like the Ramsauer effect, which was widely discussed in Göttingen (Ramsauer 1921). It clearly meant a return to more mathematical work when Born embarked on for the application of celestial mechanics to the atom from 1922 on, first with Wolfgang Pauli and then with Werner Heisenberg. However, comparison of the results with experimental measurements remained key. In a letter inviting Sommerfeld to join the Bohr talks in May 1922, which were paid by the Wolfskehl fund, Born entered the contest with the Munich school: “And otherwise, I let my students quantize, just to compete a little bit with you.”²⁵⁴ Here, on the one hand, he would soon outdo his Munich colleague in mathematical sophistication—using the rich theories Hilbert and his associates had prepared for him; on the other hand, it was Born who led his younger Göttingen collaborators back to experimental realities, by introducing the probability interpretation and relating the abstract quantum theory to scattering experiments. In the end it proved successful that he had kept rather close contact with experiments, very much so with his friend and colleague James Franck, for whom he obtained the experimental part of the Göttingen position which he had initially been expected to take on, too. In this way Born was able to invest most of his time and effort in theory, but he maintained close attention to experimental findings and concepts, among them, the concept of collisions in particular. As late as 1924 Born supervised a student on molecular beam experiments employing and refining equipment he brought from Frankfurt, which he immediately had reinstalled in Göttingen.²⁵⁵

5.5.1 The Berlin and Frankfurt Outstations: Born with Madelung, Landé, Stern, etc.

Born’s time in Berlin was dominated not by his university teaching and research, but by work—military and scientific—in a special army unit. As for many scientists of his generation it turned out necessary to adjust to the conditions of war and to explore ideas on mobilizing science for its efforts. In the same way as Richard Courant, who had pulled together his Göttingen colleagues to help develop new telephonic equipment after witnessing the inadequate gear of the young soldiers on the battlefield first hand, Richard Ladenburg realized that he would be more effective in the German war effort if he could create a way to improve shelling accuracy rather than become cannon-fodder himself. He convinced his military superiors to establish an office for scientific measurement within the *Artillerie-Prüfungskommission*, a department for testing army equipment. This group became a larger project for sound ranging, with the aim of improving technologies to locate enemy heavy artillery (Born 1975, 238; Froben 1972, 22–25). Clearly, this effort had a double rationale, and the longer and deadlier the war became, the more Ladenburg tried to use his laboratory well off the battlefield to save scientific talent. Born, an old friend of his, had initially joined a radio brigade and he became the first Ladenburg requested for his group. At the time of the outbreak of war, Alfred Landé was Hilbert’s physics assistant and immediately volunteered as a paramedic. Erwin Madelung also came from Göttingen, where he had gradually developed from an applied physicist who did his dissertation with Hermann Simon, to an experimental physicist who had worked on the atomism of solids as Riecke’s assistant, to a

²⁵⁴Born to Sommerfeld, 13 May 1922; (Sommerfeld 2004, 118f.) “Auch sonst lasse ich meine Leute quanteln, um Ihnen ein wenig Konkurrenz zu machen.”

²⁵⁵(Fritz 1925) summarizes the Göttingen dissertation of 1924.

theoretician of crystal lattices, a field where he met and inspired Born.²⁵⁶ These Göttingers as well as Ladenburg's Breslau colleagues like Fritz Reiche and Erich Waetzmann could be saved by requesting them for the sound ranging group;²⁵⁷ for others like Born's student Heinrich Herkner the order became effective when it was too late. A further part of the group consisted of experimental psychologists, since most devices ultimately relied on a listener who had to be taken into the equation.²⁵⁸

The group of physicists that gathered here is of interest for the history of quantum physics in at least two respects. First, its members were predominantly early quantum physicists who could be associated with two centers, Göttingen and Breslau (rather than Berlin). Reiche prominently published on quantum theory before and after the war; his habilitation work was on spectral lines (Reiche 1913a; 1913b; 1921). Ladenburg himself had coauthored publications with Born and Reiche, and would contribute to the solution of the dispersion puzzles of quantum theory after the war, while his colleague Waetzmann was one of the few who had actually specialized in acoustics, a field pertinent to the war problem (Born and Ladenburg 1911; Reiche and Ladenburg 1912; Ladenburg 1921). And secondly, when a routine had been established after some hectic periods in the beginning, there were "hours without serious work," which were eventually filled with scientific discussions and even research work. In this way there existed an atomic physics group in military service during the war, which was more productive in this field than those colleagues who remained at the University of Berlin.

"As soon as I had settled down in the A.P.K.," Max Born recalled, "I took up some scientific work" (Born 1978, 179). His book on the dynamics of crystal lattices had just appeared in 1915, and he extended his interest to a theory of optical activity, which was important for chemists to distinguish asymmetric (chiral) molecules and liquid crystals. In fact, this field was the perfect bridge between Hilbert's reductionist atomism and Sommerfeld's quantum rules. As Born explained in his book, it was Hilbert's insight that the route from the "enormous number of equations of single processes in molecular physics" to the "few phenomenological laws" was manageable, since "the phenomenological laws appear to be the conditions under which the equations for the molecular processes are solvable." This method of reductionism, Born added, "reveals its power in the most beautiful way for the explanation of optical activity" (Born 1915, 13).²⁵⁹ In the same way this field also inte-

²⁵⁶Voigt, however, had dismissed his work on the atomism of solids, cf. Sommerfeld to Wien, 1 June 1916 DMA Sommerfeld papers 10.

²⁵⁷Whether Otto Stern also joined the group late in the war remains unclear. This is claimed in (Segre 1973, 219), but there is no mention by Born, who remembers first becoming acquainted with Stern in Frankfurt after the war. Cf. for others like Born's student Heinrich Herkner, for whom the order became effective after it was too late (Born 1918a; 1978, 190).

²⁵⁸Among these were Max Wertheimer and Erich Hornbostel, who worked on the psychology of listening (von Hornbostel and Wertheimer 1920; cf. also Hoffmann 1994).

²⁵⁹"Der Weg, der wohl überall in der Molekularphysik von den ungeheuer vielen Gleichungen der Einzelvorgänge zu den wenigen phänomenologischen Gesetzen führen wird, ist zuerst von Hilbert in seiner Gasttheorie [1911/12] angegeben worden. Die phänomenologischen Gesetze erscheinen nämlich als die Bedingungen dafür, daß die Gleichungen der Molekularvorgänge auflösbar sind. Das ist eine Art Eliminationsprozeß, der die ungeheure Variablenzahl des Mikrokosmos auf die geringe Variablenzahl der sichtbaren Welt in strenger Weise reduziert. Es ist mir gelungen, diese Methode auf die Gittertheorie anzuwenden. Am schönsten offenbart sich ihre Kraft bei der Erklärung der optischen Aktivität." This point was also stressed by Sommerfeld in (Sommerfeld 1915b, 669f.), who was pointing out Hilbert's influence to the readers of his review. "Der Verf. hat sich mit den mathematischen Gedanken erfüllt, die Hilbert in der Theorie der Integralgleichungen [...] geschaffen und in seiner Darstellung der Gasttheorie zur Anwendung gebracht hat. Die beobachtbaren Gesetze erscheinen hier als die Bedingungen dafür,

grated the new quantum theory of Sommerfeld, which extended Bohr's theory and allowed the rules of quantum theory to be applied to atoms of higher order like oxygen and nitrogen (Sommerfeld 1916a; Born 1918b; 1918c). In one of his next papers Born evaluated experiments with infrared radiation in order to verify Sommerfeld's ring atom models, which he considered more or less adequate. This led him to the new idea to theoretically construct crystals out of Bohr-Sommerfeld atoms (Sommerfeld 1917; Born 1918b; 1978, 180).

This line of research interest, which Born was able to investigate after "office hours" at home, since his military duties evolved into practically an eight-hour-a-day white-collar job (aside from some trips to the front), was soon extended by in-office scientific collaboration. "At this time Madelung used to sit opposite me at a big desk with many drawers", Born recalled, "Each of us emptied a drawer of all military papers and filled it with scientific books and notes." Naturally, as both Born and Madelung were interested in crystals they started to collaborate on this topic (Born 1978, 181). This work produced what later became known as the Madelung constant, and the papers of Born and Landé in which they tried to calculate the compressibility of crystals, which had yielded some values experimentally; but was explained only theoretically by phenomenological theories, by now applying first principles of atomic constitution, thus realizing Hilbert's program in physics.²⁶⁰

In the course of these works by a scientific team in military disguise²⁶¹ Born arrived at a major conclusion: the elastic properties of crystals turn out to be completely inaccurate when deduced from Bohr-Sommerfeld ring atoms. Therefore, the electrons must be distributed three-dimensionally in the atoms within the crystal. The simple flat model thus failed in this case,²⁶² and the problem of how electrons could move in orbits that fill three-dimensional space foreshadowed the concept of electron clouds. In Born's recollection this is the precise starting point of a program that eventually led to the establishment of quantum mechanics (Born and Landé 1918c; 1978, 183). His contemporary reaction, however, was slightly different. In a letter to Hilbert, Born described the events of the German revolution that he witnessed in Berlin in November 1918 and about which the newspapers had not given the full picture; as it appeared to Born, it was not yet clear whether things would tend to the better. Another revolution, so-to-speak, had, however, turned out unequivocally, that is, he had established in his calculations of compressibility that atoms in the form of rings cannot hold true. They turned out to be much too soft, and "one can infer from measurement with great precision [...] that each single atom has a cubic symmetry."²⁶³ It was Landé who would excessively exploit the theory of cubic atoms before turn to the Zeeman effect in order to distance his work submitted for habilitation from that of his superior Born, but this had already happened when both were working at the University of Frankfurt.²⁶⁴

daß die Gleichungen für die Molekularvorgänge mit ihrer ungeheuren Zahl von Unbekannten auflösbar sind. Es ist dem Verf. gelungen, diese Methode auf die Gittertheorie zu übertragen. In diesem Sinne bildet die Widmung des Buches an David Hilbert einen sinnvollen Schmuck der Theorie."

²⁶⁰(Madelung 1918; Born and Landé 1918a; 1918b; Born 1978, 183).

²⁶¹It should be stressed that the work on sound ranging did go on at the same time, as it also posed many challenges.

The historical scholarship here is still not sufficient and the historian faces the problem that most (German) scientists did not report on their military efforts in the same detail as they did on scientific work. Cf. (Schirmacher 2009c).

²⁶²Strictly speaking, Sommerfeld's extension of the Bohr model had already given rise to the expectation that for many-electron atoms "space quantization" would require the now elliptic orbits to lie in different planes, cf. (Eckert 2013, 33).

²⁶³Born to Hilbert, 14 November 1918, UAG Hilbert Papers, folder 40A, Nr. 18.

²⁶⁴From Landé's many publications on the cubic atom cf. e.g. Landé 1920b. For Landé's situation in Frankfurt and the context of his work on the Zeeman effect, see the detailed account by Forman 1970a, esp. p. 165, where

Without doubt, Berlin was much more strongly affected by war and revolution than Göttingen or Frankfurt. This extended to scientific life at the universities, as well. In Berlin and particularly at the physics institute, university life was taking place in the middle of the political center. It is known that Born was able to speak at the Prussian Academy of Science at least twice during his war duty and that he found time to cultivate his friendship with Einstein at this time.²⁶⁵ For many reasons, however, it turns out to be particularly difficult to reconstruct the teaching of physics and its impact on the student body in a special field like atomic and quantum physics. It is still instructive to consider the relevant lectures, at least, as they were announced in the university catalog, although they often did not take place (see table 5.8).

| | | |
|----------------|-----------|--|
| Winter 1912/13 | Nernst | Neuere Atomistik (für Studierende aller Naturwissenschaften) |
| Winter 1913/14 | Westphal | Die Gesetze der Wärmestrahlung und das ultrarote Spektrum |
| Summer 1914 | Franck | Experimentelle Grundlagen der Atomistik |
| Winter 1914/15 | Eucken | Die Quantentheorie |
| | Born | Atommodelle |
| | Reiche | Neuere Probleme der theoretischen Physik |
| Winter 1915/16 | Nernst | Neuere Atomistik (für Studierende aller Naturwissenschaften) |
| | Born | Molekulartheorie der Kristalle |
| Summer 1916 | Born | Kinetische Theorie fester Körper |
| | Reiche | Kinetische Theorie der Gase |
| Winter 1916/17 | Weinstein | Physik der Atome, Moleküle, Elektronen und Quanten |
| Summer 1917 | Reiche | Ausgewählte Kapitel der Quantentheorie |
| Winter 1917/18 | Einstein | Statistische Mechanik und Quantentheorie |
| | Reiche | Moderne Probleme der Atomdynamik auf quantentheoretischer Grundlage |
| Summer 1918 | Born | Atomistik |
| Summer 1919 | Franck | Experimentelle Grundlagen der Quantentheorie |
| Summer 1920 | Franck | Experimentelle Grundlagen der Atomistik und der kinetischen Gastheorie |
| | Franck | Atomtheorie |
| | Reiche | Quantentheorie |

Table 5.8: Announced Lectures on Atomic and Quantum Physics at the University of Berlin, 1913–1920

Forman explains his change of topic as “a response to the exigencies of German academic careers.” On Born’s advice to pursue a research field with some distance to Born’s work, cp. Born to Landé, 27 January 1919, SPKB Landé papers, box 1, folder 5. “[...] ; nachdem Sie so lange mein wissenschaftlicher Compagnon waren, empfiehlt es sich, daß Ihre Habilitationsschrift deutlich und sichtbar ohne meine Mitwirkung entsteht.”

²⁶⁵30 November 1917 and 28 June 1918.

In the last years before the outbreak of the Great War, Walther Nernst was lecturing on “atomistics” to a general audience of science students, while younger *Privatdozenten* like disciples of his and Rubens, Arnold Eucken and James Franck, dealt with theoretical and experimental foundations of quantum and atomic physics. During the war, lectures on atomic and quantum physics were announced only by members of two groups: those unaffected by war like Einstein, due to his Swiss nationality, and Max Weinstein (1852–1918), formally still a *Privatdozent* for geography and physics, who, however, sacrificed his work to topics of natural philosophy, and was simply too old to serve the Fatherland at the time; and those at the *Artillerie-Prüfungskommission*, viz. Born and Reiche. And even when hostilities had ended, it was the former assistants of Planck and Rubens, Reiche and Franck, who would teach the experimental foundations and the theories of atoms and quanta, with Born already in office at Frankfurt.

The Prussian ministerial bureaucracy of the Kaiserreich was famous for its well-planned use of academic personnel, and its workings became known as the Althoff system (vom Brocke 1980). Even a year before Friedrich Althoff’s death in 1908, this monolithic authority governing German professors had been distributed among four successors. War, defeat and revolution had, this system and creating more leeway for those trying to steer a new course for higher education in a democratic society in the early postwar years. Both of Max Born’s changes of universities profited from this situation. In 1919 the privately devised plan of switching the positions of Born and von Laue in Frankfurt and Berlin did not meet with any opposition (as had been the case four years earlier, when Laue tried to claim priority over Born for the Berlin position); in 1921 the ministry would again show much flexibility to meet Born’s demand for an additional post for Franck.

In December 1918, the Dean of the Frankfurt faculty asked von Laue to provide an assessment of Born. Given the fact that the latter had some interest in the matter of appraising Born, he nevertheless provided an elucidating account of Born’s strengths and interests, which he describes to cover three different areas. First, Born had demonstrated his authority in the field of general relativity and had even defended the theory successfully against attacks as from Ernst Gehrcke. Secondly, his publications on quantum theory had proved his “ability to identify modern scientific problems and the means for their solution on his own.” The theory on the specific heats of solids that he had established together with Theodore von Kármán, although it had found a competitor in Debye’s different treatment, had to be judged superior, “since it accounts for the constitution of the body out of atoms. It proves the good physical eye of Born that he had founded his considerations on the arrangement of atoms in space lattices, at a time when the space lattice theory of crystals had not yet been demonstrated by *X*-ray interference experiments,” i.e. Laue’s.²⁶⁶ And, thirdly, Born would do research in a field Laue called “electron theory,” by which he was referring to Born’s recent works on optical activity and liquid crystals. While Laue did not mention that this field also had a quantum background related to the scope opened up by Sommerfeld’s extensions of Bohr’s atom, he did stress “that already a number of younger researchers (Stumpf, Landé) had joined this research and Born himself was planning experimental investigations of these

²⁶⁶Laue to Curator, 10 December 1918, UAF Main personnel file Born, p. 1–3. “[...] zeigen Borns Arbeiten über die Quantentheorie seine Fähigkeit, zeitgemäße wissenschaftliche Probleme und die Mittel zu ihrer Lösung ganz aus sich heraus zu finden. [...] Es kennzeichnet den guten physikalischen Blick Borns, dass er damals schon, als die Raumgittertheorie der Krystalle noch nicht durch die Röntgenstrahlinterferenzen sichergestellt war, doch schon die Anordnung der Atome nach Raumgittern seinen Ausführungen zu Grunde legte.”

matters.”²⁶⁷ Born’s intentions to turn to experiment had thus materialized and became known in the physics community.

When Born had completed negotiations with Frankfurt, he had not only become a full professor for the first time, he also had “achieved everything [he wanted]: 3,000 M budget for my institute, an assistant and a mechanic,” as he wrote Landé; at the same time, he wrote, that “I necessarily need an assistant, who is not only able to do experiments with prepared equipment, but also can create new apparatus. Therefore, I won’t be able to give this position to you; we have to make sure that we find funds for a theoretical assistant later.”²⁶⁸ The theoretical, or even mathematical physicist, as which Born had widely been recognized, was now becoming a “full” physicist who naturally dealt with experiments, and he valued his experimental assistant, whom he still had to find, more highly than a theoretical one, who was standing by—in a similar way as Hilbert had temporarily transformed from a pure mathematician into a theoretical physicist and had once treated his mathematics and physics assistants.

Otto Stern would become one of the new partners in this field for whom Laue had already arranged a lectureship; another was Walther Gerlach, the assistant to Richard Wachsmuth, director of the physics institute in Frankfurt and founding rector of the University of Frankfurt, which was established in 1914. Gerlach quickly felt more attracted by Born’s physics than by the activities of Wachsmuth, who remained a classical physicist in all his topics and did make no contact with relativity nor quantum theory and its experimental effects. With Stern, who came from Silesia like Born, who had studied in Breslau like him and had also worked with Einstein, Born quickly became close friends.²⁶⁹ Landé, who earned his living as a music teacher at a reform school near Frankfurt immediately after the war, joined Born even without a regular assistant’s position, as Born had promised to facilitate his habilitation here.²⁷⁰ Landé’s “independent” work was on the “quantum theory of the helium spectrum,” (and the Zeeman effect); as such, he was probably the first of the quantum theorists to study astronomical books like Charlier’s on celestial mechanics in order to apply perturbation theory to atomic structure (Landé 1920a).²⁷¹

The impression that Born had brought quantum physics to Frankfurt is quickly confirmed by the teaching activities.²⁷² While Born extended his attention to Wachsmuth’s research through a joint colloquium, no lectures on modern themes would come from his older colleague. Born and Stern introduced a modern curriculum right from start with courses on

²⁶⁷Laue to Curator, 10 December 1918, UAF Main personnel file Born, p. 1–3. “Ich will bemerken, dass sich an diese Untersuchungen Borns schon manche Arbeiten jüngerer Forscher (Stumpf, Lande) angeschlossen haben und dass Born selbst experimentelle Untersuchungen über diese Gegenstände plant.”

²⁶⁸Born to Landé, 27 January 1919, Born to Landé, 27 January 1919, SPKB Landé papers, box 1, folder 5. “Auch sonst habe ich in Frankfurt alles erreicht: 3000 M Etat für mein Institut, Assistent und Mechaniker. Das Institut ist noch öde und leer; ich muß notwendig einen Assistenten haben, der nicht nur mit fertigen Einrichtungen experimentieren, sondern neue Einrichtungen schaffen kann. Darum kann ich diese Stelle Ihnen leider nicht geben; wir müssen zusehen, daß sich später ein Fond für einen theoretischen Assistenten findet.”

²⁶⁹Their common Jewish background may not have played a role at this stage.

²⁷⁰However, it took half a year before Born could write Landé that Wachsmuth and the mathematician Schönfliess had no objections, Born to Landé, 6 June 1919, SPKB Landé papers, box 1, folder 5. His thesis was accepted in Okt. 1919, Wachsmuth to Curator, 28 October 1919, UAG Main Personal File Landé, folder 1.

²⁷¹Landé interview 1962, AHQP, p. 4. This time however, Sommerfeld’s private assistant Adolf Kratzer may also have worked on perturbation theory, cf. (Kratzer 1920).

²⁷²During Laue’s tenure only one lecture course on quantum theory is known during summer term 1917, just merely one hour long and probably meant for a general student audience. Vorlesungsverzeichnis Frankfurt. In winter 1917/18 there was also a course on relativity (two hours long).

quantum theory, molecular theory, a joint seminar on quantum theory, which was later joined by Landé as well, and lectures on relativity and “atomistics” for a broader audience. From the classical fields, mechanics and the theory of heat dominated (see table 5.9).²⁷³ After Born had left for Göttingen Gerlach and Madelung were replacing him in part, the former in the joint colloquium with Wachsmuth and the latter in the seminar with Stern and Landé.²⁷⁴

Parallels to the teachings at Göttingen at the same time are striking, both with respect to the topics Born (and Franck) announced in Berlin and Born and Stern lectured in Frankfurt, and with respect to the formats and cooperation employed. Quantum and atomic physics courses had entered the regular curriculum, as had molecular theory. The Göttingen seminar on the structure of matter run by Hilbert and Debye found its parallel in the seminars on quantum theory (or on problems of modern physics), which were held jointly by Born, Stern and Landé. Like Debye, who held a joint physics colloquium with Voigt from summer 1916 on, when he officially became a professor of experimental physics – which was later also joined by Pohl, in Frankfurt Born and Wachsmuth met for physics colloquia fortnightly. Clearly, there were also important differences between Frankfurt and Göttingen, in particular when it came to the involvement of mathematicians who did not, or at least not visibly, collaborate with the physicists. If one digs a bit deeper, however, some relationships were still present on a much smaller scale. Ernst Hellinger, who got an extraordinary professorship in 1914 when the university was founded, turns out to have become Born’s “tame mathematician” after 1919, as he resided in the attic of Born’s house, and, more than that, he was an old friend from school in Breslau and his student days at Göttingen, where he was a student assistant to Hilbert and the like (Born 1978, 190). His expertise, in particular on quadratic forms and infinite-dimensional matrices, would become a crucial resource only later, however, when Born was working out matrix mechanics.

²⁷³For Stern’s (intended) teaching as advertised in the university catalog during the war, including an “Introduction to Quantum Theory” in summer 1916, cf. (Schmidt-Böcking and Reich 2011, 39).

²⁷⁴In summer 1921 Gerlach also lectured on “Atom- und Molekülbau (für Hörer alle Fakultäten)” as well as on “Höhere Experimentalphysik.”

| | (Wachsmuth) | Born | Stern | (Landé) |
|-------------------|----------------|--|---|--|
| Spring 1919 | | Einf. theoretische Physik | Einf. Thermodynamik | |
| Summer 1919 | | Analytische Mechanik Quantentheorie Einf. theoretische Physik* | Molekulartheorie I: Kinetische Gastheorie | |
| Winter 1919/20 | Physikalisches | Kolloquium Mechanik II Übungen zur Mechanik II | Molekulartheorie II: Statistische Mechanik Thermodynamik | |
| | | Seminar über Quantentheorie | | |
| Spring 1920 | Physikalisches | Kolloquium Vektoranalysis | | |
| | | Seminar über Quantentheorie | | |
| Summer 1920 | | Theorie der Wärme Relativitätstheorie in elementarer Darstellung (1) | Analytische Mechanik + Übungen | Theorie der elektrischen Schwingungen |
| | Physikalisches | Kolloquium Seminar über Probleme der modernen Physik | | |
| Winter 1920/21 | | Theorie der Elektrizität Atomistik (1) | Mechanik der Continua | Einf. in die math. Behandlung der Naturwiss. |
| | Physikalisches | Kolloquium Seminar über Probleme der modernen Physik | | |

Table 5.9: Lectures by Born, Stern and Gerlach as well as joint seminars with Wachsmuth, and Landé at the University of Frankfurt, 1919–1921

* particularly for war veterans, free

(1) one hour long.

It was no easy task for Born to find an experimental assistant with the abilities described in his letter to Landé. After Göttingen graduate Hans Kost, who did his doctorate with Riecke, Breslau graduate Hedwig Cohn, who received her degree from Lummer, and Reinhold Fürth from Prague, student of Philipp Franck, had declined, Born wrote to Stefan Meyer in Vienna for help. Eventually, the position was filled by Elisabeth Bormann, who had just completed her doctoral examination. She belonged to a new generation of female physicists were given the opportunity to enter a scientific field mainly due to the war deploy-

ment of their male fellow students.²⁷⁵ Having attended various practical classes with Franz Exner and Stefan Meyer (one for beginners, one on radioactivity and also an advanced one) and having written her thesis “on the experimental methodology of decay fluctuations” she had a thorough background in experimental work.²⁷⁶

With Stern and Bormann as key collaborators, plus the mechanic Adolf Schmidt and Gerlach visiting, in 1919 Born had established a strong experimental group, while Landé remained the only pure theoretician at his department, with whom Born did not publish any further joint work from this point on. Four papers appeared during the Frankfurt years co-authored with Bormann, two with Gerlach and one with Stern. Their research work corresponded to three areas of physical problems which Born tried to push forward. First, his November revolution of the failure of plane atomic models led to experimental work investigating real lattice structures of simple substances, in particular zinc blende (ZnS).²⁷⁷ Second, Born supported Stern’s idea to realize molecular rays, which allowed for various experimental investigations of atomic and molecular physics. Like Born, Stern, too, had spent most of his time on theory, but now he was fully committed to examining fundamental theoretical concepts through experiments. Stern initially tried to verify Maxwell’s velocity distribution and to measure mean velocities, and he entertained plans for a molecule spectrograph. Born and Bormann adopted his technology and hoped to detect cross sections of molecule collisions, in order to determine, for example, the diameter of the silver atom. These experiments occupied Born to great extent, as his wife Hedwig confided to Einstein: “Max is very hard-working, his experiments [...] finally work and he sits in the institute until 8 p.m. and performs measurements.”²⁷⁸ Born, of course, did not convert fully to experimental physics and his immersion did not get as far as Debye’s, who took over the experimental physics position in Göttingen, but he made every effort to count as a “full” physicist who could combine theory and experiment for their mutual enrichment.

With the improved mastery of molecular rays, of silver in particular, Stern devised a test of a theory Sommerfeld had put forward to account for atomic states in his quantum theory of atoms and which implied “space quantization,” i.e. a peculiar behavior in the presence of inhomogeneous magnetic fields, which, if true, would result in splitting up a molecular ray into components. After Stern had checked the feasibility of an experimental test in a theoretical calculation, he was, with the help of Walther Gerlach and his experience with vacuum technology, eventually able to prove that the effect was true (Stern 1921; 1922). At the time of this fully successful Stern-Gerlach experiment, however, Born had already moved to Göttingen.

Born also collaborated with Stern and Gerlach on various other topic related to gases and crystals (Born and Stern 1919; 1921a; 1921b). In addition, all his students were doing experiments, a fact Born admitted to Felix Klein in Göttingen: “there are a number of doctoral students here engaged in experimental work, which I had suggested and whom I have to supervise. Strangely enough, I don’t have any theoretical doctoral student; as it seems,

²⁷⁵Born to Landé, 12 February 1919, SPKB Landé papers, box 1, folder 5; Born to Meyer, 31 March 1919 and 28 April 1919.

²⁷⁶Lebenslauf, 19 July 1919, UAF Personal-Hauptakte Bormann, Bl. 3–4.

²⁷⁷The relevant papers from Born are (Born and Landé 1918c; 1919; 1919a; 1919b; 1921b).

²⁷⁸(Born 1920b; Stern 1920a); Born to Sommerfeld, 5 March 1920, (Sommerfeld 2004, 74–75, Born 1978, 195); Hedwig Born to Einstein, 31 July 1920, (Born and Einstein 1969, 55).

people avoid this direction, at least here in Frankfurt.”²⁷⁹ However, it does not seem far-fetched to conjecture that not only Born, but also the students experienced a certain war effect, which, at least for some years pushed experimentation, although on a larger scale the rise of theory would soon reclaim ground.

Besides this research work Born also published more general surveys. On the advances and aims of the quantum theoretical treatment of the atom he had contributed three articles to the journal *Die Naturwissenschaften*, which later were combined into a book, and he published a popular introduction to the theory of relativity (Born 1920a; 1921a). Actually, without relativity the Stern-Gerlach experiment could have hardly been financed at Frankfurt, as Born had used the public curiosity about this theory after spectacular reports of the corroborating eclipse expeditions in order to raise funds needed for experimental equipment. As he confided to Sommerfeld: “The talks, which I gave in January raised 6.000 M for my institute. With this money I really got my institute going. Stern’s deflection experiment has finally succeeded nicely.”²⁸⁰

The recent foundation of the university generally necessitated in a reliance on special funds. On the one hand, there was still the *Physikalische Verein*, which had been the nucleus for the physics institute and which continued to provide support,²⁸¹ and, on the other hand, there were philanthropists of the wealthy city of finance, which could provide relief even—or rather particularly so—in times of inflation and financial crisis. The Moritz and Katharina Oppenheim foundation had funded the university chair in theoretical physics, which after Laue now Born occupied.²⁸² In the study on Einstein and the Kaiser Wilhelm Institute in this volume, Born’s audacious application for funds is discussed in some detail, where he essentially asked for funding to set up an entire laboratory. Here, he even spelled out a much wider experimental program, including experimental investigations about the predictions of theories of mixed crystals and metals. Despite the “large sum” he had requested, Einstein let Born’s wife know that he was optimistic to raise the funds sooner or later.²⁸³ While Debye did not take advantage of the equipment he requested from Einstein’s institute, since he left Göttingen before it arrived, Born apparently managed to get funds from other sources more quickly.

To sum up, not only Göttingen with Debye but also Frankfurt with Born, Stern and Gerlach became a key place of quantum research in the war and immediate postwar years. With the reshuffling of positions in the early 1920s some weights shifted, for example from Frankfurt to Hamburg, where Stern and Pauli pursued strong, new research fields. With respect to history and resources, Göttingen and Frankfurt were quite different: tradition vs. recent innovative foundation, private funds vs. state funding, rural (academic) seclusion vs. urban (economic) bustle, etc. The apparent consensus with regard to the leading fields and figures of scientific research and its overall importance, however, proves the extent to which

²⁷⁹Born to Klein, 21 November 1920, UAG Klein papers, folder 5D, p. 79. “Ich habe viel zu tun, weil mir sehr viel daran liegt, eine experimentelle Arbeit vor meinem Fortgehen fertig zu stellen. [...] ; es sind hier mehrere Doktoranden mit Experimentalarbeiten beschäftigt, die ich angeregt habe und überwachen muss. Merkwürdiger Weise habe ich aber keinen theoretischen Doktoranden; es scheint, dass die Leute diesen Weg scheuen, wenigstens hier in Frankfurt.”

²⁸⁰Born to Sommerfeld, 5 March 1920, (Sommerfeld 2004, 74).

²⁸¹For the role of the Verein in establishing and supporting the physics institutes see (Fricke 1974).

²⁸²For details see papers of UAF, Katharina und Moritz Oppenheimsche Universitätsstiftung, for the many further foundations cp. (Lustiger 1994).

²⁸³See Goenner and Castagnetti in this volume.

science had attained general, even national value at a critical time in German history when it had to symbolically compensate for lacking standing in economic and military strength after a lost war.

5.5.2 Born's Grand Plan for Göttingen

How could the Göttingen faculty make up for Debye? In their first meeting on drawing a list of candidates they agreed that there was but one chance to heal the debacle stylized so strongly in national terms: Arnold Sommerfeld. Although further names appeared in the minutes, Born and Mie, it was agreed to approach Sommerfeld first.²⁸⁴ Göttingen chemistry Nobel Prize winner Otto Wallach, who also happened to be a distant cousin of the renowned Munich physicist, had the task of asking Sommerfeld. "Understandably enough, the wish exists to compensate for the loss, we have sustained by Debye's leaving.", Wallach wrote, and he reckoned that he would "probably be completely out of reach." However, if he were willing to come, he would not only be the first name submitted to the government, he would be the only one, and he would be "greeted with greatest rejoicing." Otherwise, Göttingen would resign itself to Sommerfeld's decision. The only incentive they had to offer was a free choice in naming an extraordinary professor for his support, which he "presumably" would get. In case of refusal, Wallach asked him for his advice on a suitable successor of Debye.²⁸⁵

As in the very same meeting a commission was put in charge of a "general reorganization" of the physics professorships—there were three of them to be recast: Debye's, Simon's and Voigt's—and it was apparently at this stage that the solution first appeared that an appropriate successor for Debye could be found only if a second professorship were added, which the desired candidate might fill to his liking. Sommerfeld did not decline, but the faculty sensed both that the chances of his accepting were slim and that it would take too long. As in the first session, Hilbert attended the meeting, and this time Debye also became involved.²⁸⁶ Again, discussions about Born and Mie emerged, as well as about Wilhelm Lenz, Madelung, von Kármán and Erwin Schrödinger. Probably, it was Debye who had pushed the last of these, as he was asked to obtain opinions on Schrödinger from Planck, Einstein, von Laue and Sommerfeld.²⁸⁷ Ten days later, letters to the ministry were drafted, one for the Simon succession and one for that of Debye. Now Born was named first, with Madelung and Lenz second and third. In fact, Debye, Runge and Wallach had composed the letter for the dean to send to the ministry.²⁸⁸ In this way, *Regierungsrat* Erich Wende, senior

²⁸⁴Faculty minutes, 21 February 1920, UAG Phil. Fak. II PH 36 e I.

²⁸⁵Wallach to Sommerfeld, 21 February 1920, DMA Sommerfeld papers, NL 89, box 019, folder 5.7. "Begrüßlicherweise besteht der Wunsch, den grossen Verlust, den wird durch Debye's Fortgang erleiden, wett zu machen. Da ist natürlich, dass sich der Blick auf Sie richtet – aber, das kann doch nur mit einem gewissen Zagen geschehen, denn es herrscht die Empfindung vor, dass Sie für uns wahrscheinlich ganz unerreichbar sind. [...] Aus diesen Überlegungen heraus wollen Sie unsere unbequeme Anfrage beurteilen. Sollten Sie uns Ihre Bereitwilligkeit unzweideutig zuerkennen geben wollen, einem Ruf hierher Folge zu leisten, so würden Sie Zweifels nicht nur an erster, sondern an einziger Stelle der Regierung als zu Berufender vorgeschlagen werden und Ihren Entschluss würde man hier allgemein mit grösstem Jubel begrüßen. Besteht aber keine Aussicht, Sie hierher zeihen zu können, so müssen wir eben resignieren."

²⁸⁶Hilbert's influence on the decisions remains unclear; however he was also asked to join the commission for the succession of Simon, as a connection to Born's call, might arise. Faculty minutes, 24 June 1920, UAG Phil. Fak. II PH 36 e.

²⁸⁷Faculty minutes, 3 March 1920, UAG Phil. Fak. II PH 36 e I.

²⁸⁸Faculty minutes, 13 March 1920, UAG Phil. Fak. II PH 36 e I. The draft was modified by corrections in more than one hand, one of which appears to be Hilbert's, faculty minutes, Phil. Fak. II Ph. Nr. 36 d, XV.

executive officer in the Ministry of Culture, actually received two long letters concerning Debye's succession and the reorganization of Göttingen physics in the spring of 1920, the other being Born's about erecting what his colleagues from the Göttingen faculty had called a *Pflanzstätte*, a nursery, for mathematical and physical sciences, albeit according to his views.²⁸⁹

First of all, Debye, Runge and Wallach told Wende that only Sommerfeld might offer an adequate substitute for what Debye had established. Trying to win him, however, was futile. Instead of Mie, who might also have been an obvious candidate, a younger scientist should be preferred. Therefore, Born was the more or less natural choice, with Madelung, who initially came from the field of technical physics and who had just taken over an ordinary professorship in Kiel, and Lenz, a longtime assistant of Sommerfeld, who had not yet published much, additional candidates. Born, the authors wrote, had excelled early in those fields of theoretical physics "for which a very thorough understanding of mathematical means is required," such as relativity with its application to moving electricity. After 1912 he had shifted towards employing his "sharp eye for the needs of physics" as in the field of specific heats where the mathematical problems appear only as secondary issues. In molecular theory, which became his major interest, he treated crystal structure, optical activity, anisotropic liquids and the diffraction of light in gases. Steadily progressing along this line of research, which suggested, for example, that elastic forces in crystals were of electrical nature, he eventually opened up many directions for experimental research.²⁹⁰

Debye and his coauthors thus drew a suggestive picture of Born's qualification as a "full" physicist, who, like Voigt, would engage in experimentation and in the investigation of experimental data; they nevertheless stressed that it would not be possible for Born to replace Debye since "this was grounded in the rare double qualification of the outstanding

²⁸⁹Draft Debye, Runge and Wallach, before 22 March 1920, faculty minutes, Phil. Fak. II Ph. Nr. 36 d, XV. "Wir haben den dringenden Wunsch, dass die erledigte Professur durch einen theoretischen Physiker ersten Ranges besetzt wird. Das ist seit Jahrzehnten hier der Fall gewesen, und die so erfreuliche Entwicklung der mathematisch-physikalischen Fächer an unserer Hochschule, die dadurch eine Pflanzstätte für diese Wissenschaften geworden ist, wurde nur dadurch ermöglicht [...]" The term is usually ascribed to Sommerfeld, who employed it in an autobiographical sketch written in 1919, which was, however, published only much later, (Eckert 1993, 38).

²⁹⁰Dean to Wende, 22 March 1920, GStPKB. Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXVII, 73–75, 74–74 back: "Seine ersten Arbeiten behandeln solche Gebiete der theoretischen Physik, zu deren Beherrschung eine sehr weitgehende Kenntnis der mathematischen Hilfsmittel erforderlich ist, darunter insbesondere die Relativitätstheorie in ihrer Anwendung auf bewegte Elektronen. Diese Periode seines Schaffens reicht von 1907 bis 1912. Von da an richtet sich sein Interesse mehr und mehr solchen Problemen zu, deren Erfassung den Besitz eines geschärften Blickes für die Bedürfnisse der Physik zur Voraussetzung hat und zu deren Behandlung die zu überwindenden mathematischen Schwierigkeiten erst in zweiter Linie Anlass geben. Solche Arbeiten der neuen Periode, welche Borns Namen in weiten Kreisen bekannt gemacht haben, sind z.B. seine mit v. Karman veröffentlichter, für die Theorie der spezifischen Wärme wichtiger Satz über die Verteilung der elastischen Eigenschwingungen von Raumgittern und seine zusammen mit Courant verfasste Theorie der Temperaturabhängigkeit der Oberflächenspannung. [...] Sein Hauptinteresse aber ist der Molekulartheorie und dabei besonders der Kristallstruktur zugewandt. Er veröffentlichte Arbeiten über die Drehung der Polarisationssebene in Kristallen und amorphen Körpern, über anisotrope Flüssigkeiten, über die Zerstreuung des Lichtes in Gasen. 1915 fasst er seine Ansichten über die Dynamik der Kristallgitter zusammen und lässt sie in Buchform erscheinen. In diesem Arbeitsgebiet stetig weiter fortschreitend versucht er, über die Natur von elastischen Kräften in Kristallen Klarheit zu gewinnen und es gelingt ihm, sehr wahrscheinlich zu machen, dass jene Kräfte elektrischer Natur sind. Mit Hilfe dieser Hypothese gelangt er zu einer gegenseitigen Verknüpfung vieler Kristalleigenschaften und zu manchen Fragestellungen, welche heute noch einer experimentellen Bearbeitung harren. [...]"

scholar.” It was in particular “a combination, which becomes untenable with the exit of professor Debye.”²⁹¹

Interestingly enough, nobody discussed the option of just hiring a theoretician, given the fact that Pohl had established a splendid teaching of experimental physics. This is surprising, for Debye himself entered the Göttingen faculty through precisely this access opened up by a new and confident field of theory. We have seen how Hilbert never had tired of promoting this field of “theoretical research” for which the guest professorship was established. Now, only six years later and with considerable success of theory, no traces of similar efforts can be found on Hilbert's part. The wind had shifted, mostly due to war experiences and postwar needs, but, clearly, changes in the faculty had also occurred. While Born had learned his lessons of applied science, theory testing and experimental insight during his war work, and in this way shaken off his former distance to experiential research, and had even started supervising work by students in this field, in Göttingen proponents of applied mathematics and their supporters from industry became more outspoken. As a consequence, Born would quickly find himself appeasing Felix Klein, to whom he always had a strained relationship, rather than mobilizing Hilbert as a local supporter.

After Born received the Göttingen offer in May 1920 he went to see Pohl first and had “intensive discussions” in order to define a “plan for the arrangements for the teaching of physics” that he then proposed to the ministry.²⁹² In a nine-page letter he took great pains to include all arguments and to devise a full architecture of posts and duties. Recognizing Pohl's great experimental lectures, which he performed with his personal apparatus, supporting his demand of promotion to an ordinary professorship and acknowledging his priority in using the large lecture theater, he, nonetheless, made clear that “the theoretician [also] has the right to lecture one or two hours every week in this theater, for there are occasionally lectures that include demonstrations for which the small lecture room is not sufficient.”²⁹³

Concerning the second position that Born wanted to give to James Franck, he had to walk a fine line between the claims of Pohl, as the master experimentalist, and the Göttinger Vereinigung championing technical physics. On the one hand, Franck would bring in his own approach to modern experimenting and with this was largely able to bridge theory and experiment; in addition, he had shared much of his career with Pohl and they had become

²⁹¹ Draft Debye, Runge and Wallach, before 22 March 1920, faculty minutes, Phil. Fak. II Ph. Nr. 36 d, XV. “Der jüngst hier verstorbene Prof. Voigt war ein Vertreter der theoretischen Physik von größtem Ruf. Wenn der als sein Nachfolger [...] bekannte Prof. Debye nach dem Tode des etatmäßigen ordentlichen Professors Riecke unter gleichzeitiger Anstellung des außerordentlichen Professors Pohl, in jenes Ordinariat einrücken und einen Teil der Aufgaben auch des Experimentalphysikers übernehmen konnte, so war das durch die seltene doppelte Qualifikation des hervorragenden Gelehrten bedingt und eine Kombination, die nun mit dem Ausscheiden des Professors Debye unhaltbar wird. In diesem Augenblick muß die Fakultät mit dem allergrößten Nachdruck die Meinung vertreten, dass die Professur für Experimentalphysik, die ausnahmslos an allen Universitäten durch einen etatmäßigen ordentlichen Professor verwaltet wird, hier in ihrer Bedeutung keine Herabminderung erfahren darf. [...] Andernfalls würde bei einem etwaigen Freiwerden der Professur für Experimentalphysik ein erstklassiger Vertreter für sie überhaupt nicht zu gewinnen sein [...]”

²⁹² Born to Dean (Stille), 4 July 1920. “Bei meinem Besuch in Göttingen kurz nach meiner Berufung (Anfang Mai) hatte ich mit Prof. Pohl in ausführlichen Besprechungen einen Plan für die Ordnung des physikalischen Unterrichts und die Verteilung der Institutsräume und Lehrmittel entworfen, den ich dann dem Ministerium als Grundlage für die Lehraufträge vorschlug, die mir und den anderen, zu berufenden Physikern erteilt werden sollten.”

²⁹³ Born to Wende, 15 Mai 1920, SBB Born papers, folder 1826, p. 2–14, on 2. “Ich gebe gern zu, daß der Experimentalphysiker Vorrecht auf den Hörsaal hat, weil er zum Aufbau seiner Versuche volle Freiheit haben muß. Aber ich halte es für gut, daß der Theoretiker das Recht hat, wöchentlich ein oder zwei Stunden in dem Hörsaal zu lesen; es gibt gelegentlich Vorlesungen mit Demonstrationen, für die der kleine Hörsaal nicht ausreicht.”

a good friends despite many differences in character.²⁹⁴ Therefore, Pohl welcomed Born's suggestion in great unison with Hilbert, Runge and Prandtl. On the other hand, however, Klein raised objections: "he fears that technical physics would come off badly if a pure physicist is called for Simon's position," Born confided to Erich Wende of the ministry on Klein's opposition, "and the Göttingen Association for the Advancement of Applied Mathematical Physics could get one into trouble." Instead of drawing an artificial line between technical and pure physics, a distinction that is of no use when attempting to provide a broad education for their students, Born goes on, one should recognize: "There is only good and bad physics." And when it comes to finances, their support is "almost ridiculously small," so that he himself was quite confident to "to raise, if necessary, larger sums from industry off my own bat."²⁹⁵

Partly to appease Klein and the applied mathematics camp, partly to allow more personnel to be included, Born developed a detailed architecture both to span all fields of applied and experimental physics and to explain his elaborate space requirements. It involved Max Reich's interimposition in applied electricity (though it hardly rendered him qualified to fill this position appropriately); Otto Stern to follow him suit from Frankfurt, if he did not become his successor there, in this way extending their productive collaboration; and Heinrich Rausch von Traubenberg, a lecturer who had shown a good command of teaching in some major experimental courses in 1917 and 1918. Born stated that "In the last years he gave the main experimental physics lectures—in part alone, in part parallel to with Pohl—in fact, with considerable success." and he concluded that, in this way, a "very promising order of instruction would result," which he specified meticulously with respect to teaching fields, duration, audience and suggested personnel (*ibid.* on p. 6 and see table 5.10).²⁹⁶

²⁹⁴Cf. for the long relation between Franck and Pohl e.g. (Ebner 2013).

²⁹⁵Born to Wende, 15 Mai 1920, SBB Born papers, folder 1826, p. 2–14, on 3. "Nur Geheimrat Klein hatte Bedenken; er fürchtet, daß die technische Physik durch die Berufung eines reinen Physikers an die Simonsche Stelle schlecht wegkommen, und daß die Göttinger Vereinigung zur Förderung der angewandten mathematischen Physik Schwierigkeiten machen würde. Ich halte diese Einwände aber nicht für stichhaltig. Die Unterscheidung von reiner und technischer Physik sollte meiner Meinung nach an den Universitäten nicht gemacht werden. [...] Es gibt eben nur gute und schlechte Physik. [...] Was die Göttinger Vereinigung anbetrifft, so erscheinen mir ihre Geldbeträge fast lächerlich gering; so lange sie nicht entsprechend dem gesunkenen Geldwerte auf das Vielfache erhöht sind, braucht man darauf keine Rücksicht zu nehmen. Ich traue mir zu, auf eigne Faust, wenn nötig, größere Summen aus der Industrie aufzubringen."

²⁹⁶"Man könnte daran denken, Professor Rausch von Traubenberg mit dieser Stelle zu betrauen. [...] Er hat in den letzten Jahren die große Experimentalvorlesung teils allein, teils parallel mit Pohl gelesen, und zwar mit beträchtlichem Erfolge."

| | | | |
|----|--|--|--|
| 1. | Große Vorlesung Experimentalphysik (2 Sem., 4 St.) | für alle Hörer | Ordinarius für Experimentalphysik (Pohl) |
| 2. | Höhere Experimental- physik (2 Sem., 2 St.) | Solche Kapitel der Experimentalphysik, die sich für den allgemeinen Hörerkreis der großen Experimentalvorlesung nicht eignen, aber für Physiker und Mathema- tiker notwendig sind | Ordinarius für angewandte Elektrizitätslehre (Franck) |
| 3. | Hauptvorlesung für theoretische Physik mit Übungen (5 St.) | | Ordinarius für theoretische Physik (Born) |
| 4. | Vorlesungen aus der angewandten Elektrizitätslehre (2 St.) | | Ordinarius für angewandte Elektrizitätslehre (Franck) |
| 5. | Elektrotechnik (2–3 St.) | | Extraordinarius für Elektrotechnik (Reich) |
| 6. | Spezialvorlesungen über theoretische oder experimentelle Physik (3–4 St.) | | Extraordinarius für Physik (Stern bzw. Trautenberg) |
| 7. | Radioaktivität und dergl. (2 St.) | | Lehrauftrag für Radioaktivität (Trautenberg) |

| | | | |
|----|---------------------------------|-------------------------|---|
| 8. | Praktika | | |
| | (jedes Semester 4 St.) | | |
| | a. für Nichtmathematiker | | Ordinarius für |
| | (Mediziner, Pharmakologen etc.) | | Experimentalphysik (Pohl) |
| | b. für Physiker und | - Elektrizität | Ordinarius für |
| | Mathematiker | - Optik – Wärme | Experimentalphysik (Pohl) |
| | | - Atomistik, Elektronik | Extraordinariat bzw. Lehrauftrag (Stern bzw. Trautenberg) |
| | | | Ordinarius für angewandte Elektrizitätslehre (Franck) |
| | c. für technische Physiker | | Extraordinarius für Elektrotechnik (Reich) |

Table 5.10: A *Pflanzstätte* for mathematical-physical fields. Born's comprehensive 1920 plan for physics teaching in Göttingen, quotes arranged in table format (ibid. on p. 6f.).

In the course of this differentiation of various kinds of experimental and applied physics Born again began to downplay his own experimental aptitude. "I have never given experimental instruction, and I was able to get into this only when I abstained completely from my research work for several years," Born had explained to the ministry in his first long letter.²⁹⁷ This was, however, only the first sketch for a complete new program of physics teaching in Göttingen and marked the beginning of half a year of negotiations between Born, the two universities and the ministry.

Pointing to the Göttingen offer, which he reported to the head curator of the University of Frankfurt (who happened to be the mayor of Frankfurt, Georg Voigt, the one who had opened the university in 1914), Born listed his demands. They appear modest compared to those for Göttingen—Born had imparted earlier that he would "only reluctantly leave this fine city"²⁹⁸—but they still required a new full professorship for Stern, which was the only means to retain him as collaborator, a salary matching the Göttingen offer, and guarantees for subordinate personnel and funds for reorganization that were independent of "the fate of the university of Frankfurt," meaning provided by private funds or philanthropy.²⁹⁹

The young university, which had set up Laue's and thus Born's chair through the philanthropy of the Jewish jewelery trader and amateur astronomer Moritz Oppenheim, tried to meet all of Born's requirements but one, a position for Stern.³⁰⁰ While no clear reason for

²⁹⁷Born to Wende, 15 Mai 1920, SBB Born papers, folder 1826, p. 2–14, on 5f.

²⁹⁸Born to Vorsitzenden des Kuratoriums der Uni Frankfurt, Oberbürgermeister Voigt, 18 May 1920, UAF Personal-Hauptakte Born, Bl. 4.

²⁹⁹Born to Voigt, 7 June 1920, UAF Personal-Hauptakte Born, Bl. 5.

³⁰⁰Voigt to Born, 3 July 1920, UAF Personal-Hauptakte Born, Bl. 12f.

this refusal can be found in the official correspondence and documents, Born gave Einstein an unequivocal explanation. Richard Wachsmuth, chair of experimental physics, rejected Stern because of his “corruptive, Jewish intellect,” an attitude Born judged to be “at least open anti-Semitism,” thus leaving much room for a variety of more veiled forms.³⁰¹ So, even in a rather liberal city like Frankfurt, Jewish participation hit its limits, which discriminated rather idiosyncratically between Born and Stern and their Jewishness. Things were not much different in Göttingen either, with Born and Franck entertaining a “ridiculous luxury of a private religion,” so Pohl’s view on Franck as conveyed to his mother in 1916 (Ebner 2013, 172). Born had to realize that he could neither win the uphill battle in Göttingen. Eventually, Stern became full professor in Hamburg, an even younger university than Frankfurt, and, strangely enough, Frankfurt made some efforts to call Einstein to the Oppenheim chair, when open hostility against the creator of general relativity was staged in Berlin and had acquired anti-Semitic undertones.

Although Born told Voigt in Frankfurt back in early July 1920 that he was unable to turn down the Göttingen offer, it still took until November for him to sign the official agreement with the ministry on his Göttingen position and resources.³⁰² In this process a number of elements of the Göttingen architecture of teaching and personnel also shifted.

Without consulting Born, the Göttingen dean pushed to fill the vacant professorship in applied electricity. As neither Reinhold Rüdenberg nor Heinrich Barkhausen was willing to come, only Max Reich was left; and this position was essential to maintain the support of the *Göttinger Vereinigung* for the university. Excusing his scant publication record with wartime secrecy that prohibited him from publishing his “most important and best work” as well as with his “grand modesty,” however, the dean’s request to appoint Reich was received with “slight surprise” by the ministry, for he had been added to the list only “with marked distance” to the aforementioned, and also because Born had not been involved. As officer Wende wrote to Dean Stille, Born had already made different suggestions for the successor of Simon’s position.³⁰³ Stille, however, was able to reply that in the meantime Born himself considered this question “irrelevant” so that Reich should be appointed.³⁰⁴

Born’s view on this matter was more nuanced, however, and it was rooted in his relationship to experiments. As he had always stressed that he could not teach experimental physics the way Debye did—though he might be willing to supervise doctoral students on experimental research as he had done in Frankfurt—the idea was to fill Simon’s position not with a “technician” but rather with a physicist able to teach this field to students of mathematics and physics from a higher perspective. Here Franck would have been an ideal candidate. It was the *Göttinger Vereinigung* and the faculty that foiled this plan and, since he did not want to turn down the Göttingen offer, he had to negotiate again with the ministry. Consequently, in Berlin an agreement was reached that made it possible to hire Franck for an additional professorship anyway: Woldemar Voigt’s former chair, formally degraded to an extraordinary professorship, was to be revived to a full professorship for Franck. In this

³⁰¹Max Born to Einstein, 16 July 1920, and Hedwig Born to Einstein, 31 July 1920, (Born and Einstein 1969, 54–55).

³⁰²Born to Voigt, 10 July 1920, UAF Personal-Hauptakte Born, Bl. 14. Agreement Born and Wende, 10 November 1920, GSPtKB Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXVII, Bl. 76.

³⁰³Stille to Wende, 25 June 1920, UAF Phil. Fak. II Ph. Nr. 36 d, XV. Wende to Stille, 2 July 1920, UAG, Phil. Fak. II Ph. Nr. 36 e, I.

³⁰⁴Stille to Wende, 7 July 1920, GSPtKB. Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXVI, Bl. 418–419

way Born's plan could be realized if the necessary funds for experimental teaching would be provided.

Putting his own interests in heading an independent institute for theoretical physics last, Born suggested that he would be willing to make a double sacrifice: Under the condition that Franck receive a second full professorship for experimental physics, Born would both forgo the directorate in favor of Franck he would take on the full teaching load of theoretical physics alone. "But in this way," he wrote, "I acquire a collaborator with whom I share many common fundamental views and I expect great advantages for teaching and research from this."³⁰⁵

As a consequence, the division of resources with Pohl had now to be renegotiated with Franck, as Pohl was claiming the main lecture theater exclusively,³⁰⁶ while Born tried to sell the new arrangement to Felix Klein, the principal proponent of the applied mathematics and technology fraction, which had at least successfully lobbied for Reich's appointment. Thanking him for supporting the appointment of Franck, Born wrote to Klein that he was fully aware of his responsibility for insisting on this very person, but he was quite sure that "Franck would, in fact, be able to create a school of grand style." Like Faraday, Franck had the rare gift to recognize experimental opportunities, a fact that had been appreciated abroad before it was "discovered" at home as Born had realized when he visited America in 1912. Franck was well aware of the Göttinger Vereinigung and would support it in the same way as Born did in order to fulfill Klein's vision of science and industry stimulating each other. In addition Born even told Klein of own plans for a "calculating institute for theoretical and technical physics," thus sketching common ground in some detail; it was a nicely crafted plan to Klein's liking, however, it was never realized in any form.³⁰⁷

In October and November 1920, agreements with the ministry redefined Göttingen physics into two experimental departments for Pohl and Franck, with two assistants each, and a mathematical department for Born, with one special assistant only. Nevertheless, Born secured 20,000 Marks – a considerable amount for apparatus, thus highlighting his continuing interest in experimental research.³⁰⁸ This appears less surprising given the fact that at this time Born was in the middle of his own experimental work in Frankfurt and the Stern-Gerlach experiment was at a crucial stage. Born and his Frankfurt collaborators had just presented their experimental work prominently at the *Naturforschertagung*, which took place in Bad Nauheim in September 1920 (historically remembered mostly for the dispute between Philipp Lenard and Albert Einstein on relativity) (Born 1920b; Stern 1920b). Later that year Born wrote Klein about his preparations to move to Göttingen for the spring semester: "I am very busy as it means a lot to me to finish an experimental project before my departure."³⁰⁹ Against this background of experimental work in Frankfurt, where he had a number of students performing experimental work but none on theory, it appears consistent that Gerlach was granted the status of *Privatdozent* without having fulfilled any of the

³⁰⁵Born to Stille, 4 July 1920, UAG, Phil. Fak. II Ph. Nr. 36 e, I.

³⁰⁶Minutes "Nachfolge Simon" entry of session on 5 July 1920. = UAG Phil. Fak. II Ph. Nr. 36 e, I.

³⁰⁷Born to Klein, 11 July 1920, SUB Göttingen, Klein Papers, 5 C Bl. 68/69.

³⁰⁸Agreement Wende and Franck, 6 October 1920, GstPKB. Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXVII, Bl. 63–64. Agreement Wende and Born, 10 November 1920, GstPKB. Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXVII, Bl. 76.

³⁰⁹Born to Klein, 21 November 1920, SUB Göttingen, Klein Papers, 5 D, Bl. 79.

standard requirements, and that he was giving his inaugural lecture on “The importance of quantum theory in modern experimental physics.”³¹⁰

In late 1920 and early 1921, a number of shifts in disciplinary terrain and developments in physics accompanied the process of Born’s and Franck’s relocation to Göttingen. In arguing to raise the pay for his “physics assistant,” who had received an offer for an assistant position from Frankfurt (probably from Madelung, who was about to be named Born’s successor there, while Elisabeth Bormann had requested to be released from her contract),³¹¹ Hilbert claimed that it was this assistant, Dr. Kratzer, “on whose shoulders presently rested essentially all activity of theoretical physics here.” Without better funding of his assistant positions, Hilbert feared, his teaching would be at risk, which covered wide areas of physics as has been detailed above, a field Born would quickly reclaim, however.³¹² Before, a lectureship in the field of (experimental) radioactivity had been granted to Rausch von Traubenberg, in full accordance with Born and Franck, as the Göttingen dean wrote to the ministry, thus extending a line of practical classes that Eduard Riecke had established long ago.³¹³

5.5.3 Quantum Theory in the Göttingen Curriculum, 1921–1926

A striking difference to academic conditions in Frankfurt was apparent for Born after the first days of teaching in Göttingen. He reported to Gerlach that together with Hilbert, Franck and Runge he had to supervise 80 students, including some quite capable ones, and in the seminar that he offered together with Franck there were even more. He invited Gerlach to join him in Göttingen “for the purpose of leisure: Monday seminar, Tuesday mathematical seminar, Wednesday mornings proseminar, Wednesday afternoon colloquium.”³¹⁴ Therefore he opted to teach, on the one hand, about topics in which he had firmly established himself, in this case four-hour courses on kinetic theory as he was just completing the second edition of his book on this topic with the help of a private assistant (Emmerich Brody); and, on the other hand, standard fare any physics student had to master: mechanics, optics, heat etc. (see table 5.11). Clearly, Born’s grand plan for teaching physics, developed and negotiated with Pohl and Franck, now had to be met, which restricted Born’s latitude for teaching. In consequence, those students interested in recent quantum and atomic physics in 1921 had to approach this field from the experimental point of view through Franck’s two-semester lecture course on atomic physics, since no lectures on atomic theory were offered. Hilbert and lecturer Paul Hertz at least were offering other fields of modern physics, relativity and radiation theory, Hilbert’s course, however, was advertised as “basic ideas of relativity theory (for students of all fields)” and was thus a popular treatment just one hour per week, rather than an advanced course.³¹⁵ Solid physical theory was then offered by Hilbert in the following term with a weekly four-hour lecture on “statistical methods, particularly in physics” which also included some quantum theory of specific heats (Hilbert 1911, 104).³¹⁶

³¹⁰Dean Lorenz to Kurator, 8 February 1921, UAF Personal-Hauptakte Gerlach, Bl. 9.

³¹¹Bormann to Curator, 2 April 1921, UAF Personal-Hauptakte Born, Bl. 6.

³¹²Hilbert to Ministry, 10 November 1920, GStPKB. I. HA Rep. 76, Nr. 591, Bl. 280.

³¹³Stille to Ministry, 23 August 1920, GStPKB. Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXVII, Bl. 7.

³¹⁴Born to Gerlach, 11 May 1925 and 16 May 1925, DMA, Gerlach papers 83. “Kommen Sie mal auf ein paar Tage zur Erholung nach Göttingen.”

³¹⁵“Grundgedanken der Relativitätstheorie (für Hörer aller Fakultäten),” the lecture notes went under the title “Über Geometrie und Physik” (by Bernays) and “Über Geometrie und Physik” (by Hüchel).

³¹⁶Statistische Mechanik (listed as “Statistische Methoden, insbes. der Physik”), summer term 1922, lecture notes by Lothar Nordheim, Mathematisches Institut Göttingen. [SS 1922], Mathematisches Institut Göttingen.

Coincidentally or not, after summer 1922 when Niels Bohr delivered his famous talks on atomic physics, a total of seven spanning a two week period,³¹⁷ both Hilbert and Born offered courses covering quantum topics. Again, Born was approaching radiation and quantum theory from the point of view of his kinetic theories, thus excluding Bohr's theory and atomic modeling, while Hilbert, in his typical way, used his lecturing to explore new research fields and secured help from two physics assistants, Lothar Nordheim and Gustav Heckmann, both advanced doctoral students of Born (1924).³¹⁸ These lectures presented the Bohr-Sommerfeld theory in a mathematically somewhat streamlined version, including Bohr's quantization rules and the framework of Hamilton-Jacobi theory and action-angle variables, and closed with research results as recent as Born's and Pauli's paper of 1922 (Born and W. Pauli 1922; Majer and Sauer 2009, 503–602). In the following term only Franck offered a course titled “atomic theory,” although his teaching was otherwise always on experimental physics.³¹⁹

Only from winter term 1923/24 on did Born adopt Hilbert's teaching philosophy and use his courses on “higher mechanics” and “atomic mechanics II” to systematically treat the application of astronomical perturbation theory on atomic physics. With the assistance of Hund, again Nordheim and also Heisenberg for some parts the lectures were published in early 1925 as “lectures on atomic mechanics,” and were intended, as indicated in the preface, as an advanced treatment that would significantly differ in particular from Sommerfeld's book, though the student should find this out himself (Born 1925; Sommerfeld 1919b). At this time Born had also monopolized modern theoretical physics, as Hilbert would not return to teach relativity until 1925 and quantum theory until 1926, concentrating instead on central topics from mathematics, and Paul Hertz had also shifted his field to the methodology of natural science. Regarding physics, one finds a curricular pattern and a division of labor between Born and Franck which was repeated in an approximately two-year rhythm, while Hilbert, too, duplicated and extended his 1922/23 course on the mathematical foundations of quantum theory four years later, now including the fully developed quantum mechanics (Majer and Sauer 2009, 605–707).

The strongest connection between teaching and research was made in the seminars, however, on which, unfortunately, no contemporary sources exist aside from some memoirs. What seems obvious from table 5.11 is that Born had established himself as the central actor, who participated actively both in the famous structure of matter seminar with Hilbert and in Franck's seminar, which was meant as a forum where students of experimental and theoretical physics could inform each other about their research work on their dissertations.

³¹⁷Lecture notes of 12, 13, 14, 19, 20, 21 and 23 June 1922, SBB, Born papers 1819.

³¹⁸Nordheim's dissertation was published in two articles: (Nordheim 1923a), and (Nordheim 1923b).

³¹⁹No lecture notes on this course are known to have survived.

| | Hilbert | Born | Franck | <i>other</i> |
|-------------|---|---|------------------------------------|--|
| SS 21 | Einsteinsche Gravitationsth. Relativitätstheorie* | Kin. Th. der festen Körper I: Elektri- zität, Magnetismus | Atomphysik I | <i>Hertz</i> Strahlungstheorie |
| | Struktur der Materie Physikalisches Proseminar | | | |
| WS 21/22 | (Grundlegung der Mathematik) | Kin. Th. der festen Körper II: Elektro- nentheorie, Optik | Atomphysik II | <i>Hertz</i> Statistische Mechanik |
| | Struktur der Materie Physikalisches Proseminar | | | |
| SS 22 | Statistische Metho- den insb. der Physik | Th. der Wärme Elektro- und Magnetooptik | Elektronenleitung durch Gase | |
| | Struktur der Materie Physikalisches Proseminar | | | |
| WS 22/23 | Math. Grundl. der Quantenth. Wissen u. math. Denken* | Kin. Theorie der Materie | Radioaktivität | <i>Courant/ Siegel</i> Differenzengl. |
| | Struktur der Materie Physikalisches Proseminar | | | |
| SS 23 | (Anschauliche Geometrie) | Mechanik | Atomtheorie | <i>Hertz</i> Methodenlehre |
| | Struktur der Materie Physikalisches Proseminar | | | |
| WS 23/24 | (Mengenlehre) | Höh. Mechanik ^a | | |
| | Struktur der Materie Physikalisches Proseminar | | | |
| SS 24 | Mechanik | Atommechanik II | Grenzgebiete d. Phy. u. Che. | <i>Hertz</i> Prinzipien... |
| | Struktur der Materie Physikalisches Proseminar | | | |
| WS 24/25 | Das Unendliche* | Optik | Elektrizitätsleitung durch Gase | |
| | Struktur der Materie Physikalisches Proseminar | | | |
| SS 25 | (Ans. Geometrie) | Th. der Wärme | Exp. Grundl. d. Quantenth. | <i>Hertz</i> Methodenlehre |
| | Struktur der Materie Physikalisches Proseminar | | | |
| WS 25/26 | (Zahlentheorie) Raum und Zeit* | Kin. Th. der Materie | Exp. Grundl. d. kin. Th. | <i>Heisenberg</i> Kristallgitter |
| | Struktur der Materie Physikalisches Proseminar | | | |
| SS 26 | (Algebr. Zahlen) | Atomtheorie | Fluoreszenz u. Phosph. | <i>Hertz</i> Methodenlehre |
| | Struktur der Materie Physikalisches Proseminar | | | |

| | Hilbert | Born | Franck | other |
|----------|---|---|--------|-------------------------------|
| WS 26/27 | Math. Methoden der Quantenth. Struktur der Materie | Elektrizität und Magnetismus Physikalisches Proseminar | — | <i>Hertz</i> Methodenlehre |

Table 5.11: Lectures and joint seminars of Hilbert, Born, Franck and others 1921–1926. Selection with respect to quantum theory and new topics in physics, philosophy and methodology.

*Lecture open to students of all fields.

() Lectures on other topics mentioned only if no other relevant lecture was offered.

^aAdded in brackets on lecture notes: “Störungstheorie mit Anwendungen auf Atomphysik.”

5.5.4 Born’s and Franck’s Research Agendas and Göttingen’s Multiple Avenues Towards Quantum Mechanics

When Alfred Landé received news from Bohr in Copenhagen early 1921, that he was now able to derive the whole periodic table from his quantum theory, Born (who was still in Frankfurt), Gerlach and Stern, wondering how this could be possible, urged Franck to worm the secret out of Bohr, whom he was about to visit.³²⁰ Plans to make Bohr come to Göttingen for Wolfskehl lectures in spring were not realized until the following year, when, at the “Bohr festival” Heisenberg also entered the stage and began working with the new Göttingen quantum physics community spearheaded by Born and Franck. This means that Born and Franck were setting up their research agendas and working groups for at least a year before Bohr was to deliver both of his most influential and still rather puzzling and vague talks. Instead of interpreting this visit as a “dramatic event” that started a “new Göttingen era,”³²¹ in the following I will instead stress a number of continuities, which are related to Born’s collaboration, specific experimental equipment or findings and methods used. At least five such lines of research fields and agendas can be identified: X-ray structural analysis, molecular beams, many-electron quantum theory, the Ramsauer effect and collision theory. My claim is that these five strands of physical science, that were experimental, theoretical or often rather a combination of both, constitute the Göttingen avenues to quantum mechanics. Interestingly, they did not meet exactly in one point—matrix mechanics—but displayed various crossings of different nature. This brings us back to the perspective of resources and research politics and the historical observation that the final trajectory of scientific progress is the result of many decisions to direct or redirect resources in a way that favors or encumbers research in a certain field, and that entails resources of various kinds ranging from financial constraints, to personnel, regarding the investment of time of the single scientist, to the promise of the given research field within science or as an exploitable technology.

³²⁰Born, Gerlach and Stern to Franck, 22 February 1921, SBB Born papers, folder 954, p. 1–2.

³²¹For the interpretation as a “dramatic event” see e.g. (Mehra and Rechenberg 1982b, Chap. III, 262). That Bohr’s talks did not present much spectacular news can be inferred from a comparison of the notes taken of the event (SBB Born papers, folder 1819) and the published Copenhagen lectures, which had been submitted to *Zeitschrift für Physik* back in January 1922 (Bohr 1922).

Relocation of Research and Acquisition of New Resources Including X-rays

Born's move to Göttingen did not mean an end to the experimental research he had initiated in Frankfurt. On the contrary, a remarkable continuity can be found. When he wrote to Gerlach in May 1921 that he really liked Göttingen and that his and Franck's assistants were busy assembling "my apparatus" quickly, with only liquid air still missing, at the same time he was arranging for the work of his old collaborators to proceed. In the case of his student Peter Lertes, who had already completed his dissertation at the university in 1920 but was now improving his experimental work for publication in *Zeitschrift für Physik* Born made clear that he could easily duplicate his work on dielectric fluids in Göttingen (Lertes 1920; 1921a; 1921b): "Please tell Lertes," he wrote,

when he annoys you with his laziness etc. I will have his rotation experiment built up in Göttingen; there are many people here who can handle amplifier tubes and could do the job quickly. One has to talk to the stripling à la entente with short-notice ultimatums and sanctions.³²²

More cordially, Born praised Gerlach's energy to push forward crystal analysis, a project for which Born had provided the theoretical computations. Now he was working with him to figure out how well the atomic theory could be confirmed and where things still had to be changed. Commenting a recent paper by Born's new Göttingen collaborator Franck, he and Gerlach compared various experimental and theoretical accounts for electron affinities, concluding that the force law, which describes the atomic repulsion within a lattice, needed refinement (Born and Gerlach 1921a). A week later, Born besought Gerlach, "Beware of X-ray burn!!!!!!" and threatened to write his wife because any injuries would be irreversible and what he had heard from his assistant about Gerlach's carelessness, really scared him.³²³

But not only did projects in Frankfurt extend to Göttingen, in a certain manner, Debye's research here before Born's arrival, had spread to Frankfurt as well, as precisely these measurements with X-rays and the Debye-Scherrer camera set up were what made Born worry about Gerlach's lab practice and safety. Some of the equipment for this work was provided by the Solvay institute (with von Laue's help a X-ray transformer was borrowed) and some by private funding, which Born had secured (Gerlach and O. Pauli 1921). At the end of 1921 Gerlach was involved in at least three lines of experimental research: besides Debye-Scherrer type X-ray diffraction, he also pursued molecular beams, which became the Stern-Gerlach experiment, and optical activity, all fields that offered stronger or weaker cooperation with Born. In October Born even suggested to investigate a new effect when Gerlach had started looking into an experimental method to determine the impact of atomic dipoles on (mechanical) friction coefficients. He conjectured that rotating dipoles should produce measurable magnetization.³²⁴

Born was aiming at much more upscale X-ray equipment for Göttingen, and his closeness to Einstein was instrumental in gaining access to funding from the Kaiser Wilhelm

³²²Born to Gerlach, 16 May 1921, DMA Gerlach Papers 83. "Sagen Sie dem Lertes, wenn er Sie durch Faulheit etc. ärgert, so lasse ich den Drehversuch in Göttingen aufbauen; hier sind mehrere Leute, die mit Verstärker-Röhren umgehen können und die Sache schnell machen könnten. – Man muß mit dem Jüngling à la Entente reden, in kurzfristigen Ultimativen mit Sanktionen."

³²³Born to Gerlach, 23 May 1921, DMA Gerlach Papers 83.

³²⁴Born to Gerlach, 10 Oct 1921, DMA Gerlach Papers 83.

Institute for Physics, which was still a rather virtual institution. As the respective details have been discussed in the contribution to this volume by Goenner and Castagnetti, it suffices here to mention that Born, on the one hand, was strengthening his case by siding with Franck and Pohl – eventually they made the request jointly, and, on the other hand, was able to forgo expounding definite research needs in the application, in this way winning the allocation of funds of no less than 100,000 Marks (although inflation had started to accelerate).³²⁵ Thanking Einstein for the great gift, he explained that such equipment “is now part of a reputable institute and there are quite often questions that can only be answered with help of X-rays. [...] This grand donation means that all of you in Berlin are confident that we might figure out something respectable with the apparatus, which makes us happy.”³²⁶

Molecular Beams

One line of experimental research that actually extended from 1920 up to 1925 and came from Frankfurt to Göttingen (and with Otto Stern to Hamburg) were atomic or molecular beams. When Born referred to “my apparatus” to be reassembled in Göttingen he meant the experimental setup he had used for the determination of the mean free length of silver atoms with some help from Bormann (Born 1920b; 1921). Although not much more is known about the use of this equipment in the first years in Göttingen, a doctoral student of Born, Fritz Bielz, explained at some length in *Physikalische Zeitschrift* in 1925 how the “experimental setup had been improved by Prof. Born in cooperation with Dr. E. Hauser and Dr. R. Minkowski,” two assistants from Göttingen and Hamburg, respectively, so that he was able to realize useful quantitative measurements including the measurement of the diameter of an uncharged silver atom. Thanking Born both for suggesting and for supporting the execution of the project, and also the two mechanics of the institute for constructing the somewhat complicated brass apparatus, Bielz’ dissertation is a good example for Born’s continued interest in experimental atomic physics (see Fritz 1925 and table 5.12).

³²⁵Verzeichnis der im Rechnungsjahre 1921/1922 bewilligten Zuwendungen, AMPG, I. Abt. Rep. 34, Nr. 13.

³²⁶Born to Einstein, 29 November 1921, Born and Einstein 1961, p. 91ff.

| Name | Topic | Year | Published in |
|------------------------------|---|------|--------------|
| Hund, Friedrich | Versuch einer Deutung der großen Durchlässigkeit einiger Edelgase für sehr langsame Elektronen | 1923 | Z. Phys. |
| Hermann, Carl | Über die natürliche optische Aktivität von einigen regulären Kristallen (NaClO ₃ und NaBrO ₃) : eine Prüfung der Bornschen Theorie der Kristalloptik | 1923 | Z. Phys. |
| Peter, Fritz | Über Brechungsindizes und Absorptionskonstanten des Diamanten zwischen λ 644 und 266 | 1923 | Z. Phys. |
| Heckmann, Gustav | Über die Elastizitätskonstanten der Kristalle | 1923 | |
| Schmick, Hans | Zur Theorie der Dipolflüssigkeiten | 1923 | Z. Phys. |
| Nordheim, Lothar | Zur Behandlung entarteter Systeme in der Störungsrechnung. [1.] (Auszug aus d. 1. T. d. vollst. Diss.); [2.] Zur Quantentheorie des Wasserstoffmoleküls | 1924 | Z. Phys. |
| Kornfeld, Heinz | Über die Bindung der Partikel in den Gittern verschiedener Dipol- und Quadrupolgase | 1923 | |
| Wessel, Walter | Über das Massenwirkungsgesetz in ionisierten Systemen und die numerische Berechnung chemischer Gleichgewichte | 1924 | |
| Jordan, Pascual | Zur Theorie der Quantenstrahlung | 1925 | Z. Phys. |
| Bielz, Fritz | Versuche zur direkten Messung der ‘mittleren freien Weglänge’ von ungeladenen Silberatomen in Stickstoff | 1925 | Z. Phys. |
| Bollnow, Otto | Zur Gittertheorie der Kristalle des Titanoxids, Rutil und Anatas | 1925 | Z. Phys. |
| Bubenzer-Rolan, Karl Theodor | Die Eigenschwingungen tetraederförmiger Molekeln | 1925 | Z. Phys. |

Table 5.12: Dissertations concluded under Born’s supervision in Göttingen 1921–1925.

Many-Electron Quantum Theory

Contrary to standard historiography, Born's program to systematically push forward Bohr's quantum theory for systems with few or many electrons was not a result of the 1922 "Bohr festival," nor did it coincide with his remark on competition towards Sommerfeld. Rather, it can be traced back to Born's late Frankfurt days. Born wrote Einstein in February 1921 that he had just hired a private assistant—like Hilbert used to acquire his physics assistants—to help him complete his book on atomic theory of the solid state (Born 1923a, 594–595, 712, 754).³²⁷ Emmerich Brody was a Hungarian Jew with "Eastern manners" and weak hearing and thus appeared in Born's judgment untenable for the German university system, so he was paid from private funds.³²⁸ It was within this collaboration that in the context of a theory of crystal lattices both the problem of anharmonic oscillations and the use of perturbation theory, adapted from celestial mechanics, were brought to bear on quantum theory, both crucial resources on which Born later drew in work with Pauli and Heisenberg, while Schrödinger found the application of quantum theory unnecessary here.³²⁹

Born, who had waived claims for one or more regular assistants in order to reach a deal with Franck and Pohl, had only one special assistant, Hans Weigt, who had done his dissertation with Debye on the electrical moments of molecules and whose skills were greater in the experimental fields (Weigt 1921). In July 1921—again, long before what is usually suggested to be the starting point of Born's program toward a quantum mechanics—Born urged the university to get Pauli hired as his assistant, since "Mister Pauli is regarded to be the greatest talent that has emerged in the field of physics in recent years" and the wish to collaborate with him had also been expressed by Hilbert, Runge, Franck and other colleagues.³³⁰ His highly ingenious, though eventually unsatisfactory treatment of the simplest atom with two electrons, the hydrogen molecule ion, complemented efforts by Born and Hilbert (W. Pauli 1922). Both supervised Lothar Nordheim's dissertation on degenerate systems in quantum theory and the very same problem of the hydrogen molecule ion, treated in a different fashion (Nordheim 1923a; 1923b).

This line of research included Brody, Pauli, and after his departure, also Heisenberg. It led to some preparatory efforts and results which Born understood as the attempt to push the mechanical picture, the quantization and the approximation methods to the very limit, in order to recognize those elements of the classical theory that ultimately fail to provide appropriate models consistent with the experimental properties of the real systems studied (Born 1924; 1925).

³²⁷"A critical discussion of the applicability and the limits of Bohr's theory can be found already in this book, including the untenability of flat orbital models, the unobservability of electron rings by X-ray analysis and that atomic dynamics may relate to infinite-dimensional quadratic forms (matrices) which may have both a discrete and an continuous spectrum, [...]"

³²⁸Born to Einstein, 12 February 1921, (Born and Einstein 1969, 81, 100). On Brody see (Born 1975, 214).

³²⁹(Born and Brody 1921; 1922; 1922; 1923b; Brody 1921; Schrödinger 1922.)

³³⁰Born to Curator, 4 July 1921, UAG 4 V h 35, Bl. 185.

Ramsauer Effect

As Gyeong Soon Im discussed in his pioneering study on the experimental constraints on formal quantum mechanics, another experimental input played a huge role in the discussions in Göttingen about solving the quantum riddle in the early 1920s: the Ramsauer effect (Im 1996). Originating from a rather different culture of physics, Philipp Lenard's Heidelberg institute with its long tradition on cathode ray research, the effect that slow electrons could, contrary to classical theory, penetrate noble gases almost freely, despite strong inter-atomic force fields, invited response from quantum theory (Ramsauer 1921).³³¹ Immediately after having heard Ramsauer's talk at the Jena *Physikertag*, Franck wrote to Bohr that he was surprised by "a paper by Ramsauer that I am not able to believe, though I cannot show any mistake in the experiment. Ramsauer obtained the result that in argon the free path lengths are tremendously large at very low velocities of electrons. [...] If this result is right, it seems to me fundamental."³³²

Born, in turn, told Einstein about his reaction to Ramsauer's presentation with respect to the theoretical consideration of the mean free length on electrons in gases: "[T]his, however, is of interest because of the simply crazy claim by Ramsauer (at Jena) that in argon the free length becomes infinite with decreasing velocity (the atoms are penetrated by *slow* electrons!). This, however, we would like to disprove."³³³ In this way the Ramsauer effect contributed to bringing the problems of quantum theory to center stage in Göttingen.

Here this problem was tackled from both sides, experiment and theory. Franck's assistant Herta Sponer, a 1920 doctoral graduate of Debye in theoretical physics, had turned to experiments on atomic collisions after Franck came to the university. When she published results from her experiments, first together with Rudolf Minkowski from Hamburg and then alone, Friedrich Hund had just finished his dissertation with Born on a theory of the Ramsauer effect.³³⁴ Hund, however, was only able to present a qualitative derivation of the effect using quantum theory; therefore, Sponer and Minkowski concluded: "A possible interpretation of the effect could be given in relation with Hund's theory. Due to its uncertainty, however, we would rather not address this interpretation for the time being (R. Minkowski and Sponer 1923)."

It was at this stage in particular that Born started an attempt to solve the Ramsauer puzzle that slow electrons can penetrate atoms with almost no effect. With the help of Pascual Jordan, who had replaced Pauli as Born's assistant after he went to Hamburg, Hund's early qualitative results should be replaced by a sound and detailed treatment, which hinged on a quantum theory of aperiodic processes (Born and Jordan 1925).

The extent to which the problem of the Ramsauer effect motivated research and the introduction of new concepts into the description of collision processes may also illustrate the proposal by Walter Elsasser to perform diffraction experiments with free electrons according to de Broglie's dissertation. Initially a student of Franck's who then turned to theory and Born, Elsasser learned from Born and Franck that such experiments had just been reported and that by and large the magnitudes would agree so that he should work things out

³³¹For a discussion from the experimental perspective see (Im 1995).

³³²Franck to Bohr, 25 September 1921, BCW_8, 689.

³³³Born to Einstein, 29 November 1921, (Born and Einstein 1969, 92f).

³³⁴(Hund 1923; R. Minkowski and Sponer 1923; Sponer 1923; cf. also the summary R. Minkowski and Sponer 1924).

theoretically (Jammer 1966, 249; Born and Einstein 1969, 121; Im 1996; Elsasser 1925, 711).

Collision Processes

For those like Born and Franck who were less interested in spectroscopy, for which the quantum theory of periodic processes was quite effective, but rather immersed in experiments of particle rays and molecular beams—a field that would eventually grow into the huge industry of particle physics—it was imperative to find a quantum theory that treated not only periodically orbiting electrons within the atom, but also collisions. In their still pre-quantum mechanics paper, Born and Jordan started from the classical theory for aperiodic electrical fields acting on a restricted periodic system. They discussed induced probabilities of quantum jumps and employed a “correspondence principle of movements” in order to explain the Ramsauer effect with quantum theory. This meant the use of a technique, which can be traced back to Hilbert’s lectures on quantum theory in winter 1922/23, and which was also discussed by various Göttingen physicist concurrently: the use of averaging methods and the replacement of differential by difference equations.³³⁵

As Born explained to Einstein, the motivation for all this work was to understand collision processes, which Franck had mastered in Göttingen:

Chiefly, however, I am interested in the also quite mysterious difference calculus that is behind the quantum theory of atomic structure. With Jordan I am systematically examining—though with limited mental effort—every imaginable correspondence relationship between classical, multiply periodic systems and quantum atoms. [...] This is a preliminary work for an investigation into the processes at atomic collisions (quenching of fluorescence, sensitized fluorescence à la Franck, etc.); [...] (Born and Einstein 1969, 118f.)

Born and Franck were in ongoing discussions about collision processes from the very beginning of their Göttingen collaboration and in the crucial years of the formulation of matrix mechanics of Born, Heisenberg and Jordan, which, however, was not able to cover these processes before wave mechanics came to its aid. Born was alternately publishing with Jordan and Heisenberg on a new mechanics of matrices and with Franck on the formation of molecules from collision processes (Born and Jordan 1925).³³⁶

With Franck he explored the question of molecule formation (and dissociation) and asked what role excitation and collision processes may play. They concluded that one cannot understand molecule formation processes as collisions of just two atoms or ions, but rather, as energy considerations suggested, collisions of three involved entities. Born’s theoretical analysis showed that quantization had to distinguish rather slow nuclear rotation and oscillation from the motion of the faster electrons, an idea Franck had already developed rather intuitively and qualitatively.³³⁷ What later became known as the Franck-Condon principle,

³³⁵There is a priority debate in the secondary literature about this point, typically attributing invention of this technique to the respective person treated, i.e. Heisenberg by (Jammer 1966; Mehra and Rechenberg 1982a; Cassidy 1992; 1987; Duncan and Janssen 2007b; 2007a). All their datings are preceded by the treatment in Hilbert’s lectures, which have widely been heard and studied as the lecture notes were available. (Majer and Sauer 2009, 503–602). This may be a typical example of the problems with dating a key idea which was, however, essentially developed collectively.

³³⁶Received 11 June 1925. For a detailed description of Franck’s research and his collaboration with Born cf. (Lemmerich 2007, 82ff.).

³³⁷Franck followed a lead from (O. Klein and Rosseland 1921; Franck 1922; 1924; Born and Franck 1925).

and was then fully derived from quantum mechanics,³³⁸ described the possible molecule formations and processes in which energy differences of configurations would be balanced not by radiation energy but by translation, rotation or vibration, i.e. mechanical energy. These radiation-free processes where the released quantum energy changed into kinetic or translational energy of the atom without radiation were also called collisions of the second kind, and they were at the heart of many projects by Franck's doctoral students.³³⁹

³³⁸Edward U. Condon: A Theory of Intensity Distribution in Band Systems. In: *Phys. Rev.* Nr. 28, 1926, pp. 1182–1201.

³³⁹Cf. for details (Im 1996, 86f.) and (Lemmerich 2007, 125ff.).

| Authors | Title | Date |
|----------------------------|--|-----------|
| Born + Brody | Über die Schwingungen eines mechanischen Systems mit endlicher Amplitude und ihre Quantelung | 09/07/21 |
| Born + Pauli | Über die Quantelung gestörter mechanischer Systeme | 29/05/22 |
| Born | Über das Modell der Wasserstoffmolekül | 27/06/22 |
| Born + Brody | Zur Thermodynamik der Kristallgitter II | 26/08/22 |
| Born + Hückel | Zur Quantentheorie mehratomiger Molekülen | 01/11/22 |
| Born + Heisenberg | Über Phasenbeziehungen bei den Bohrschen Modellen von Atomen und Molekülen | 16/01/23 |
| Born + Heisenberg | Die Elektronenbahnen im angeregten Heliumatom | 11/05/23 |
| Born | Quantentheorie und Störungsrechnung | *06/07/23 |
| Born + Heisenberg | Zur Quantentheorie der Molekeln | 21/12/23 |
| Born + Heisenberg | Über den Einfluß der Deformierbarkeit der Ionen auf optische und chemische Konstanten | 10/02/24 |
| Born + Franck | Bemerkungen über die Dissipation der Reaktionswärme | 07/10/24 |
| Born | Die chemische Bindung als dynamisches Problem | *26/12/24 |
| Born + Franck | Quantentheorie der Molekelbildung | 15/12/24 |
| Born + Jordan | Zur Quantentheorie aperiodischer Vorgänge | 11/06/25 |
| Heisenberg | Über quantentheoretische <i>Umdeutung</i> kinematischer und mechanischer Beziehungen | 29/07/25 |
| Born + Jordan | Zur Quantenmechanik | 27/09/25 |
| Born + Heisenberg + Jordan | Zur Quantenmechanik II | 16/11/25 |
| Born + Jordan + Nordheim | Zur Theorie der Stoßanregung von Atomen und Molekülen | *27/11/25 |
| Born + Wiener | Eine neue Formulierung der Quantengesetze für periodische und nichtperiodische Vorgänge | 05/01/26 |
| Nordheim | Zur Theorie der Anregung von Atomen durch Stöße | 14/02/26 |
| Born | Zur Quantenmechanik der Stoßvorgänge (Vorläufige Mitteilung) | 25/06/26 |

Table 5.13: Born's collaboration in various fields of quantization, atomic structure, molecule formation and quantum collision theory, 1921–1926 (selection).

* publication date, when date received not available.

5.5.5 Conclusion

This sketch of the collaboration between Born and Franck is just one more example of the many strong experimental constraints that Born was fully aware of when he was formulat-

ing matrix mechanics with Heisenberg and Jordan. They suffice to illuminate that in the context of great efforts to experimentally capture all aspects of atomic collisions, molecular formation and dissociation it was self-evident for Born that any theory of quantum mechanics must address the question of collision and scattering experiments. While Heisenberg, who flunked the practical classes with Franck, Born suggested he attend after his weak performance at his Munich doctoral examination in July 1923,³⁴⁰ celebrated the break down of a space-time description of single objects in the atomic realm governed by matrix mechanics as a revelation, and even proclaimed that the notion of electron or particle trajectories had to be abandoned completely, Born immediately started to seek a framework that would allow the new impossibility of accounting for all mechanical quantities on the atomic scale to be married with the experimental evidence that was constantly gained from collision and scattering experiments performed in Franck's laboratory. For this reason Born, Jordan and Nordheim still employed difference calculus to explain excitations of atoms in collisions even with matrix mechanics at hand, which, however, was not yet capable to deal with this case, only after Schrödinger's wave mechanics did this become possible (Born, Jordan, and Nordheim 1925; Nordheim 1926). This example illustrates how the different lines of development did not all meet exactly at a single point.

It therefore appears rather straight forward that it was Born who proposed his statistical interpretation of quantum mechanics in June 1926 and stressed that this step would hardly have been feasible without his collaboration with Franck. By formulating the scattering problem in terms of Schrödinger's wave mechanics, but interpreting the \square -function as a new entity that allows to determine the probability of a quantum state (rather than that of a transition), solving the equations with appropriate approximations and expanding the result in terms of plane waves that represent free particles, Born was able to account for the probabilities of the respective scattering outcomes. As a tribute to Franck he included inelastic scattering, thus providing a quantum mechanical description of the very Franck-Hertz experiment that is regarded as the first corroboration of quantum atomic structure as introduced by Bohr (Born 1926b).

When Born traveled to the US in late 1925 just after the formulation of Göttingen matrix mechanics and before Schrödinger published his technically more traditional wave mechanics—though begging the question as to what these waves would actually represent—he fully remained on this track of mastering collision processes. From this perspective it is also clear that the often mentioned near miss of wave mechanics, when he and Norbert Wiener, who had just worked out a general operator calculus, introduced operator techniques for quantum theory, does not do justice to his approach (Wiener 1926). Since the relation between energy and time appeared most important for collision processes, while direct observation of the position seemed to be unfeasible, Born and Wiener merely replaced the energy with a differential operator $\frac{\hbar}{i} \frac{d}{dt}$. However, there was no motivation to relate linear momentum p to the coordinate derivative $\frac{d}{dx}$ (Born and Wiener 1926). Born's self-understanding as a physicist with strong attention to experimentation, which had developed to some extent after the war, probably also overshadowed his identity as a theoretician and mathematician trained by Hilbert and Minkowski. In any case, the collaboration between mathematicians and physicists in Göttingen, which was so close when Hilbert turned into an ersatz physicist

³⁴⁰AHQ Interview with James Franck, 9–14 July 1962.

in the years before World War I, had somewhat loosened after war when the experiment claimed some priority over theory.

Consequently, it comes as no surprise that Hilbert complained about a lack of interest on the part of mathematicians when so few from this field showed up to Heisenberg's presentation of matrix mechanics in Göttingen in September 1925, admonishing them not to ignore "great things that come into the world in Göttingen."³⁴¹ Due to Hilbert's illness around that time, Born had not yet heard his view on matrix mechanics, which had become thoroughly established by the three-man paper of November 1925 before he left for America (Born, Heisenberg, and Jordan 1926).³⁴² When Hilbert finally had a closer look at quantum mechanics with his lecture course on its mathematical foundations a year later, wave mechanics and Born's statistical interpretation had arrived, so that now everything fell into its (mathematical) place (Sauer and Majer 2009a, 605–707; Hilbert, Neumann, and Nordheim 1927).

³⁴¹Undated notes of a speech c. September 1925, UAG Hilbert papers 657, 33.

³⁴²Born asked in a letter to Hilbert, 28 November 1915, UAG 40A, Nr. 21, what he would say "regarding our attempt at a quantum mechanics."

5.6 Appendix: Selected Documents

Document 1

From a letter from Hilbert, on vacation in Alassio, to Sommerfeld in Munich, 5 April 1912.³⁴³

Wie Sie wissen, haben wir bisher aus den Zinsen der Fermatstiftung die Kosten für die Einladungen der Herrn Poincaré und Lorentz bestritten. Wir möchten aber dieses Jahr, wo zu allem Sonstigen auch noch der internationale Kongress für Mathematik in England kommt und da diesmal auch Klein nicht dabei sein kann, nicht wieder in derselben Weise zu einer wissenschaftlichen Fermat-Woche einladen. Ich habe mir daher folgenden Ersatz in bescheideneren Dimensionen gedacht: Ich selbst lese dieses Semester Mittwoch u. Sonnabend 9–11 Uhr über Principien und Axiome der Physik. Wie wäre es, wenn für die beiden letzten Doppelstunden des Semesters also etwa 29. Juli u. 2. Aug. 9–11 Uhr statt meiner Sie eintreten würden? Diese Zeit würde den Göttinger Dozenten u. jüngeren Mathematikern u. Physikern wohl am besten passen, so dass ich für ein gut besuchtes Auditorium bürgen könnte. Der Gegenstand bliebe Ihnen ganz überlassen: Am besten wohl Strahlungstheorie und Quantentheorie. Auswärtige würden wir nicht besonders einladen, wengleich solche, wenn sie kommen, uns natürlich sehr willkommen sein würden. Ein gutes Honorar (ich denke etwa 1000 M.) könnte ich Ihnen aus der Fermatstiftung in Aussicht stellen. Wie denken Sie darüber? Es wäre auch eine gute Gelegenheit uns einmal wiederzusehen und ausführlich zu sprechen. Alle Göttinger Kollegen würden sich ausserordentlich freuen, am meisten aber ich.

Document 2

From a letter from Debye after taking his position in Utrecht to Sommerfeld in Munich, 3 November 1912.³⁴⁴

Mit meinem Colleg geht alles schon seinen gewohnten Gang, ich lese Mechanik, weil es nötig ist und Kinetische Theorie von Magnetismus und Dielectrica, weil es mir Spaß macht. Obwohl das hier nicht gebräuchlich ist, habe ich doch die Semestereinteilung beibehalten, damit nicht zu viele Dinge nebeneinander her laufen, wie das bei einer Jahreseinteilung nötig wäre. Das zweite Semester kann ich anfangen lassen, wann ich will und so die Zeit vernünftig ausnutzen. Für das zweite Semester habe ich angekündigt 1) Thermodynamik 2) Nernst'sches Theorem und Quantentheorie. (...)

Ich muß noch eine Bemerkung machen, damit Du ganz genau weißt, wie ich über die Berufung denke. Du hättest gerne gehabt, daß ich Utrecht ausgeschlagen hätte mit Rücksicht auf Leiden. Das schien mir nicht so sehr wünschenswert, denn der einzige Grund dafür wäre das Erreichen des ideellen Wertes gewesen: Nachfolger von Lorentz heißen zu können. Das ist nun aber

³⁴³DMA, HS 1977–28/A, 141.

³⁴⁴DMA, HS 1977–28/A, 61.

doch schließlich nicht anders als rein äußerliche Sache zu betrachten, etwa wie ein Orden. Das was ich mache oder machen werde, würde dadurch in keiner Weise beeinflußt und das ist die Hauptsache, denn meine Stellung hier ist tatsächlich genau dieselbe, wie sie in Leiden gewesen wäre. Also kurz und gut ich bin zufrieden, wie es ist. Wenn Du nun fragst, wie es kommt, daß gerade Ehrenfest genommen wurde: nun das habe ich gut merken können, dafür hat Einstein gesorgt ... (...) In Deinem vorigen Briefe sprichst Du weiter davon, daß ich nach Göttingen zu der Physikerzusammenkunft kommen soll, von der ich aus Deinem Briefe zuerst höre und von der ich absolut nicht weiß, was sie sein soll oder wann sie abgehalten wird. Daß Du versucht hast mich mit dorthin zu kriegen, hat mich sehr gefreut und ich danke Dir wirklich sehr und gerne dafür. Du mußt es mir aber nicht übelnehmen, wenn ich nun so das Gefühl habe nicht als ungeladener Gast dort plötzlich auftauchen zu können. Du sagst zwar, Du könntest mich offiziell einladen, aber in Wirklichkeit ändert das doch nichts daran, daß die Herren denken werden, ich hätte bei etwas mehr Taktgefühl empfinden müssen, daß ich besser wegbleiben könne. Ich möchte nicht den Eindringer spielen, der in die Küche gehört, aber nun 'mal mit in den Salon hineingekommen ist und dann dort allerdings recht freundlich geduldet wird. Sei mir deshalb nicht böse, ich möchte Dir gerne, ohne das fürchten zu müssen, offenherzig sagen können, was ich denke. (...)

Document 3

Application from the Göttingen Königl. Gesellschaft der Wissenschaften to the Prussian Ministry of Culture regarding annual funds of 5000 Marks for a guest professorship in the mathematical sciences, 7 July 1913.³⁴⁵

Die persönliche Zusammenarbeit in der soeben charakterisierten Weise erscheint uns im gegenwärtigen Zeitpunkte besonders wichtig und fruchtbar für die Disziplinen der mathematischen Erkenntnistheorie einerseits und der theoretischen Physik und Astronomie andererseits. Die beantragte Institution soll daher außer der reinen Mathematik besonders der Pflege dieser Disziplinen zugute kommen, indem wir alljährlich für das Sommersemester einen Gelehrten als Gast hierher nach Göttingen berufen, der entweder in hervorragender Weise einen Spezialzweig der reinen Mathematik vertritt oder auf einem der genannten Nachbargebiete eine führende Rolle einnimmt. Die Aufgabe der Berufenen besteht darin, an den gelehrten Vereinigungen insbesondere der mathematischen und physikalischen Gesellschaft, wie sie an unserer Universität bestehen, tätig teilzunehmen, mit den hiesigen Dozenten in persönliche Beziehung und wissenschaftlichen Ideenaustausch zu treten und endlich einen Cyclus von Vorlesungen aus seinem Spezialgebiet abzuhalten, welche für die Dozenten, Assistenten, Doktoren, und evtl. einer in bestimmter Masse auszuwählenden Studenten in höheren Semestern berechnet sind. Demnach würde die in Rede stehende Institution nur akademisch wissenschaftlichen

³⁴⁵GStPKB. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXVII, Bl. 153–155, duplicate in UAG Hilbert Papers 494, Nr. 7, Bl. 13–15.

Interessen dienen und keinen unmittelbaren Einfluss auf den allgemeinen Unterrichtsbetrieb der Universität ausüben. Die von uns beantragte Institution wäre eine Förderung der theoretischen Forschung auf rein akademischen Boden wie eine solche den experimentellen Wissenschaften in der letzten Zeit so mannigfach zuteil geworden ist, so insbesondere neuerdings durch die Forschungsinstitute der Kaiser-Wilhelm-Stiftung. Im Vergleich mit den für die experimentellen Wissenschaften aufgewandten Mittel erscheinen die hier beantragten Mittel als ausserordentlich bescheiden. Wir erwähnen noch, daß die genannten, der Mathematik benachbarten Gebiete – mathematische Erkenntnistheorie, theoretische Physik und mathematische Astronomie – sämtlich auf den deutschen Universitäten nicht so vielseitig vertreten sind ... als notwendig erscheint. Dieser Übelstand ist im Augenblick vor allem für die theoretische Physik fühlbar; denn da das Ausland in dieser Hinsicht durchweg besser gestellt ist, so haben wir häufig unsere tüchtigsten, jungen theoretischen Physiker an das Ausland abgeben müssen... Es muß betont werden, daß die Pflege der theoretischen Seite der Naturwissenschaften und die Kultur der in ihr wirksamen reinen Geistesarbeit ebenso dringende Notwendigkeit und Pflicht wie die Erforschung der experimentellen Tatsachen in der Naturwissenschaft ist.

Document 4

Addition to the memorandum of Hilbert to the Prussian Ministry of Culture regarding the application for a guest professorship, circa 7 July 1913.³⁴⁶

Zur eingehenden Motivierung der für die Göttinger Gastprofessur beantragten 5000 M. gestatte ich mir noch Folgendes zu bemerken: 1. Für die Berufung von Männern ersten Ranges aus dem Auslande wie H. A. Lorentz (Leiden), den wir vor allem wünschen, Rutherford (Cambridge), Hadamard (Paris) etc. glauben wir aus Gehalt für das Semester die volle Summe von 5000 M. nötig zu haben. 2. Wenn Inländer, die für das Sommersemester hierher nach Göttingen beurlaubt werden, oder jüngere Gelehrte berufen werden, so wird allerdings eine geringere Summe als Gehalt genügen. Es ist aber zu bedenken, dass für die Gastprofessur – abgesehen davon, dass wir uns doch vorbehalten müssen gelegentlich auch einen reinen Mathematiker zu berufen – nicht weniger als drei grosse und gleich wichtige Nachbardisziplinen nämlich: mathematische Logik und Philosophie, theoretische Physik, und mathematische Astronomie in Frage kommen. Wenn daher die beabsichtigte neue Institution der Gastprofessur eine nachhaltige Wirkung erzielen soll, so wäre die gleichzeitige Berufung von mehreren Gelehrten nötig. Dies ließe sich nur erreichen, wenn unserem Antrage auf Bewilligung von 5000 M. entsprochen wird. Da nach dem Weggang von Brendel und Schwarzschild die mathematische Astronomie hier garnicht vertreten ist, so sind wir mit Professor Haar aus Clausenburg in Verbindung getreten und es ist Aussicht

³⁴⁶GStPKB. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXVII, Bl. 156–157, duplicate in UAG Hilbert Papers 494 , Nr. 8, Bl. 19–20.

vorhanden, dass Haar für das Sommersemester 1914 Urlaub bekommt, uns hier in Göttingen mathematisch-astronomische Vorlesungen zu halten. Haar soll aus dem Fermatfonds honoriert werden und wenn dieser auch schon für verschiedene andere Zwecke in Anspruch genommen ist, so könnten wir ihn doch gelegentlich mit heranziehen um im Verein mit den beantragten 5000 M. die gleichzeitige Berufung mehrerer Gastprofessuren zu ermöglichen.

3. Überhaupt betrachte ich die von der Gesellschaft der Wissenschaften beantragten 5000 M. nur als einen Grundstock zur Förderung der theoretischen Forschungsgebiete; ich hoffe, dass, wenn wir erst die genannte Summe im Etat der Gesellschaft haben, es möglich sein wird, Stiftungen von privater Seite zu gewinnen – hat doch die Verwendung der Fermatgelder sich so gut bewährt, und den weitesten Anklang gefunden, so insbesondere der im Frühjahr 1913 einberufene Kongress über die kinetische Theorie der Materie mit den Vorträgen von Planck, Nernst, Debye, H. A. Lorentz, Sommerfeld, von Smoluchowsky, die je 800 Mark aus dem Fermatfont erhielten.

Document 5

From a letter from Hilbert to Hugo Andres Krüss in the Prussian Ministry of Culture in Berlin concerning the establishment of a guest professorship, 3 October 1913.³⁴⁷

Nach der allgemeinen Motivierung die die hiesige Kgl. Gesellschaft auf einstimmigen Beschluß in ihrer Eingabe an das hohe Ministerium gegeben hat und nach den speziellen Ausführungen, die ich in dem neulich an Sie gerichteten Briefe zur Begründung der Höhe gemacht habe, und die wesentlich die für die Berufung in Frage kommenden Persönlichkeiten betraf, glaube auch ich, wie Sie, die Sache am besten zu stützen, wenn ich noch in sachlicher Hinsicht eingehender ausführe, warum gerade die sich gegenwärtig auf mathematischem Fundamente vollziehende Umwandlung der physikalischen Grundbegriffe die innere Notwendigkeit der Schaffung einer solchen Institution, wie wir sie wünschen, in sich birgt. Wenn ich in einem fahrenden Eisenbahnzuge in der Fahrtrichtung vorwärts gehe, so bewege ich mich gegenüber dem Erdboden... (...). Der Einsteinsche Gedanke ist seit David Humes Kritik des Causalitätsbegriffes die kühnste und gewaltigste wissenschaftliche Idee geworden. (...) In ihr [der neuen vierdimensionalen Weltmechanik] erscheinen schon früher entdeckte Tatsachen in neuem und einfacherem Zusammenhange so z.B. der von Kaufmann experimentell und von Abraham mathematisch erbrachte Beweis, dafür dass die Trägheit, diese auch dem Laien bekannte alltägliche Fundamenteigenschaft der Materie ein rein elektromagnetisches Phänomen ist. (...) Sie erkennen aus diesen kurzen Darlegungen, wie eng Mathematik und Physik gegenwärtig miteinander verknüpft und wie sehr dies beiden Wissenschaften aufeinander angewiesen sind. Während bisher die Mathematik wohl gelegentlich einzelne Probleme aus der Physik aufgriff und dann für sich allein in abstrakt mathematischem Sinne weiter behandelte und andererseits die

³⁴⁷GStPKB. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXVII, Bl. 158–162, duplicate in UAG Hilbert Papers 494, Nr. 9, Bl. 23–27.

Physik meist von der Mathematik nur technische Rechenregeln oder formale Rechenmethoden verlangte, verschmelzen hier beide Wissenschaften zu einem einzigen Wissensgebiete: es folgt daraus, dass der Mathematiker weit tiefer als bisher in das innerste Wesen der physikalischen Wissenschaft eindringen muss und ebenso der Physiker nicht mehr blo[s] mathematischer Laie bleiben darf. Die Schwierigkeiten, diesen Anforderungen zu genügen, sind für die Vertreter beider Wissenschaften an sich sehr bedeutende durch blosses von Abhandlungen lassen sie sich gegenwärtig überhaupt nicht überwinden: denn der Physiker überspringt in der schriftlichen Darstellung leicht wichtige logische Schlußreihen, die er wegen der ihm geläufigen und anschaulich gegenwärtigen Experimente als selbstverständlich hinnimmt, wo für den Mathematiker oft gerade der Schlüssel zum Verständnis der physikalischen Vorgänge ruht. Dem Physiker andererseits ist es meist ganz unmöglich, dem abstrakten Gange einer modernen mathematischen Abhandlung zu folgen, selbst wenn diese ein ihm geläufiges Wissensgebiet betrifft. Die populär gehaltenen vielgelesenen Schriften von H. Poincaré und E. Picard liefern fortlaufende Illustrationen dafür, dass die wichtigsten Fortschritte in den Naturwissenschaften durch die Verbindung der Grenzgebiete erreicht werden. “Gewisse Arbeiten” so sagt E. Picard in der Einleitung zu seiner Schrift über das Wissen der Gegenwart, “können nur durch das Zusammenwirken eines Mathematikers mit einem Physiker zur Durchführung kommen.” Und wenn ich Ihnen eine solche spezielle nur gemeinsam von Mathematikern und Physikern zu lösende Aufgabe nennen soll, so ist es die Ergründung der Struktur des Atoms: ein großzügiges Problem, bis vor kurzem noch fast unzugänglich, in das gegenwärtig alle wissenschaftlichen Gedankenfäden zu münden scheinen, wenn auch seine definitive Lösung wohl noch in weiter Ferne steht! Die bedeutsamen Arbeiten, über die Struktur der Materie, die gegenwärtig besonders in England und Holland erscheinen, durch persönliche Einwirkung und wechselweiser Aussprache unserem Ideenkreis vertraut zu machen, wär eine der Aufgaben, die dem theoretisch physikalischen Gastprofessor für die nächste Zukunft zufiele und es wäre damit eine klaffende Lücke in unserem gegenwärtigen Wissenschaftsbetriebe ausgefüllt. Ähnlich, wenn auch nicht so augenfällig für die Fernstehenden – steht es mit den Beziehungen zwischen Mathematik und Erkenntnistheorie: ...

Document 6

From the protocol of the Tagung der Göttinger Vereinigung zur Förderung der angewandten Physik und Mathematik, held November 21–22, 1913 in Göttingen.³⁴⁸ (Voigt requests support for spectroscopic equipment for 1200 Marks. Subsequently he speaks about the second Solvay Congress which he had attended.)

Der Herr Kollege Riecke und ich haben einen Bericht über die allgemeine Tätigkeit unserer Institute nicht eingereicht, weil letztere nicht eigentlich in das Arbeitsgebiet der G[öttinger] V[ereinigung] fallen. Dagegen habe ich mir erlaubt, ganz unkommentmässig in elfster Stunde noch ein Gesuch um

³⁴⁸GStPKB. 76 V a, Sekt. 6, Tit. X, Nr. 4 Adbih. Heft VII, pp. 45–47

eine Beihilfe für das von mir geleitete Institut einzusenden. Ich bin dazu gekommen einmal durch den Eindruck der in diesem Jahr ganz bedrohlichen an das Institut gestellten Anforderungen, die auf dessen starker Frequenz beruhen, sodann durch die Erwägung, dass die G.V. durch das Nichtzustandekommen des Juristenkurses in diesem Jahre einige Mittel besitzt, auf die sie nicht gerechnet hatte. Ich war der Meinung, dass man der letzteren an sich unerfreulichen Tatsache vielleicht eine für mein Institut erfreuliche Seite abgewinnen könnte. Das von mir geleitete Institut ist ein theoretisch physikalisches in dem Sinne, dass die Theorie darin herrschen ist; aber es ist nichtsdestoweniger ein Institut experimenteller Arbeit. Nur beschäftigen wir uns nicht mit der Pionierarbeit in neuen, unaufgeklärten Gebieten, sondern verfolgen Aufgaben, die mit der Prüfung und Ausgestaltung der Theorien in bereits erschlossenen Gebieten zusammenhängen, und Bestimmten speziell die Zahlenwerte physikalischer Konstanten, die in den Theorien auftreten. Das ist eine nach aussen einigermassen unscheinbare Arbeit, die aber nichtsdestoweniger notwendig und nützlich ist. Diese Tätigkeit steht auch, wie ich bei einer früheren Gelegenheit ausgeführt habe, in engem Zusammenhang mit dem höheren wissenschaftlichen Unterricht in der Physik. Die Göttinger Vereinigung hat meinem Institut, trotzdem dasselbe nicht zu ihrem eigentlichen Unterstützungsgebiet gehört, wiederholt ihre willkommene Hilfe angedeihen lassen. Zum letzten Male vor zwei Jahren, wo sie zusammen mit der Unterrichtsverwaltung die Mittel zur Anschaffung eines modernen erstklassigen Gitterspektroskopes gespendet hat.

Document 7

From a letter from Hilbert to *Oberregierungsrat* Ludwig Elster in the Prussian Ministry of Culture, 6 April 1914.³⁴⁹

Für das bevorstehende Etatsjahr habe ich meinen bisherigen 2. Assistenten Dr. Landé (theor. Physik) als ersten Assistenten engagiert, der mit für die Vorbereitung meiner theoretisch physikalischen Vorlesungen unentbehrlich geworden ist und da mir die Erhöhung seiner Renumeration von 800 auf 1200 Mark notwendig erschien. Mein bisheriger erster Assistent Dr. Hecke, Privatdozent für Mathematik wird mir seine Assistentendienste noch ein weiteres Jahr widmen und soll daher mein zweiter Assistent werden. Da nun Dr. Hecke bisher 1200 M. erhalten hat und mir unschätzbare Dienste leistet, so beantrage ich, ihm ausnahmsweise für das kommende Etatsjahr statt 800 M, die sonst mein zweiter Assistent bekommt, 1200 , bewilligen zu wollen. Ich gestatte mir noch zu bemerken, dass ich im kommenden Sommersemester meine Assisten[ten] in besonderem Grade in Anspruch nehmen muss: es ist nämlich teils aus der Wolfskehlstiftung, teils aus dem vom Ministerium und der Kgl. Gesellschaft der Wissenschaften bewilligten Mitteln zwei "Gastprofessoren" nämlich Debey-Utrecht für theoretische Physik und Haar-Klausenburg für theoretische Astronomie zu engagieren gelungen.

³⁴⁹GSIPKB. 76, Nr. 591, Bl. 210.

Document 8

From a letter from Voigt to Otto Naumann, director in the Prussian Ministry of Culture, 20 May 1914.³⁵⁰

In dieser Eingabe ist auf meine Veranlassung ausgeführt, daß es angemessen erscheint, dem in die betreffende Stelle zu Berufenden (neben dem persönlichen Ordinariat) die Leitung der bisher mir unterstehenden Abteilung B des physikalischen Instituts zu übergeben. Es ist dies nicht nur deshalb geschehen, weil dem der Fakultät in erster Linie erwünschten Kandidaten Debye von Zürich und nun Frankfurt (neben dem Ordinariat) ein nicht unbedeutendes selbständiges Institut angeboten wird und Göttingen ihn unter anderen Bedingungen nicht gewinnen könnte, – wir halten diese Anordnung auch für an und für sich zweckmäßig. Eine starke Schaffenskraft, wie sie bei Debye unzweifelhaft vorliegt braucht für ihre Betätigung freien Raum, und so sehr ich erwarte mit dem neuen Kollegen in wissenschaftlicher Arbeit auch im Institut zu kooperieren, so würde er doch mein verbleiben an der Spitze des Instituts naturgemäß als Einengung empfinden. Bezüglich der Wünsche, die der neue Institutsdirektor geltend machen wird, beehre ich mich folgendes auszuführen: der Universität ein theoretisch-physikalisches Institut zu schaffen, das im Sinne des Ideales meines Lehrers Franz Neumann die breiteste Berührung zwischen Theorie und Beobachtung vermittelt. Ein solches Institut kann naturgemäß nicht an jeder Universität bestehen ... (...) Es folgt hieraus, daß man in dem mit Mühe und mit Opfern auf eine gewisse Höhe gebrachten Institut der Betrieb ferner in der bisherigen Weise aufrecht erhalten werden soll, ähnliche Geldmittel auch weiterhin zur Verfügung stellen müssen. Kaum wird ein neuer Direktor in der Lage und bereit sein, ähnliche Zuschüsse zu leisten, wie ich aus Interesse an der Begründung und Hebung des Institutes geleistet habe. Um das Institut nicht Not leiden zu lassen, wird also der Etat für sachliche Geldmittel ... einer nachdrücklichen Aufbesserung bedürfen.

Document 9

From a letter from Dean Körte of the philosophical faculty at Göttingen to the Prussian Ministry of Culture, 28 May 1914.³⁵¹

Eine Hochschule, die in theoretischer Physik vollwertig vertreten sein will, muss diese wichtige neue Richtung in Lehre und Forschung ausgiebig berücksichtigen, eventuell durch Heranziehung neuer Mitarbeiter. Zur Illustration der hierdurch entstandenen Bewegung sei darauf hingewiesen, dass das ganz allgemein und auch gerade bezüglich jüngerer Kräfte so reich ausgestattete Berlin in allerletzter Zeit zwei der bedeutendsten Vertreter der neuen Richtung gewonnen

³⁵⁰GStPKB. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 71–73

³⁵¹GStPKB. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 74–76; also Göttingen UA 4 I 105, Bl. 149–151. This memorandum was presented in the session of the philosophical faculty on 28 May 1914 after Voigt had reported on the prehistory of the motion for a replacement professorship in theoretical physics. The proposed text was unanimously authorized. Cf. Protokollbuch

hat, resp. zu gewinnen sucht: Einstein in eine akademische, Laue in eine Universitätsstellung (...) Der gegenwärtige Inhaber der Professur für theoretische Physik, der Kollege Voigt, hat uns nun erklärt, dass er in seinem Alter den neuen bedeutungsvollen Entwicklungen wirklich gerecht zu werden nicht imstande ist, zumal seine gepflegten Arbeitsgebiete nach anderer Seite hin liegen. (...) Derjenige, dessen Gewinnung uns in allererster Linie erwünscht sein würde, der gegenwärtig in Göttingen als Gastprofessor wirkende Herr Debye aus Utrecht, bewährt z.B. diese Eigenschaft [eines Ordinariats würdig zu sein] dadurch, dass er nicht nur in Utrecht eine ordentliche Professur inne hat, sondern auch augenblicklich von Frankfurt und von Zürich in Ordinate begehrt wird. Wir haben Ursache zu hoffen, dass Herr Debye das Extraordinariat in Göttingen, verbunden mit einem persönlichen Ordinariat cum iure succedendi sowohl der gegenwärtig innegehaltenen, als auch den beiden angebotenen Stellen vorziehen würde. Könnten wir ihm aber nicht eben jetzt eine Berufung nach Göttingen in Aussicht stellen, so würde er unserer Hochschule, unzweifelhaft in bedauerlicher Weise, definitiv verloren gehen.

Document 10

From a draft of a letter from Hilbert to Geheimrat Ludwig Elster in the Prussian Ministry of Culture, 25 January 1915.³⁵²

Während der weltumwälzenden ~~großen~~ Geschehnisse draußen spielt sich hier eine Entwicklung ab, die für den engen Kreis der math.-naturwissenschaftlichen Kreis in Verhältnissen von nicht geringer Bedeutung sind und da Sie, hochver. Herr Geheimrat, indem Sie die Herberuf. Debyes ~~ermöglichten~~ herbeiführten, der Urheber der Entwicklungen sind, so kann ich nicht umhin, ~~in aller Kürze mitzuteilen~~ Ihnen das[?] mitzuteilen. Durch den Beitritt[?] Debye hat das math. Seminar hier ein solche[?] Höhe err[ungen?], daß fast alle math.-phys.ischen Lehrkräfte daran teilnehmen und die Dispute [?] dann gestalten sich zu wissens[chaftlichen] Taten. Debye ist erweist sich [als] der Newton der ~~Chemie~~ Molekül Physik und wir haben jetzt insbes. durch seine neuesten Entdeckungen um Weihnachten [1914] herum die solange vergeblich gesuchte in weiterer Ferne geglaubte Grundlage einer neuen math. Chemie. So ist zugleich für mich persönlich in wissenschaftlicher Hinsicht Debye wirklicher Ersatz für Minkowski geworden. (...) Übrigens hat Debye für morgen die Vorladung zur militär-ärztlichen Untersuchung erhalten... Nun fehlt mir nur noch Willy Wien, dessen Herberufung wie ich glaube nicht sehr sicher wäre evtl. nach Friedenschluß!

Document 11

From a letter from Wilhelm Wien to Otto Naumann, director in the Prussian Ministry of Culture, 4 December 1915.³⁵³

³⁵²UAG Hilbert Papers, folder 466, Nr. 1.

³⁵³GStPKB. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 341–342

Ich nehme mir die Freiheit an Ew. Excellenz ein Schreiben in einer Frage zu richten, welche für die ganze Physik von erheblichem Interesse ist nämlich die Besetzung des Ordinariats für experimentelle Physik in Göttingen. So ungern ich mich ohne unmittelbare Aufforderung in Berufungsfragen einmische, ... so scheint es mir doch diesmal im allgemeinen Interesse unserer Wissenschaft geboten zu sein. ... Es ist eine Stelle, die für die künftige Entwicklung der deutschen Physik sehr bedeutsam werden kann... (...)

Wenn nach Göttingen nicht ein wirklich produktiver Kopf kommt, so werden vor allem die vielen jüngeren Arbeitskräfte die sich dort wie kaum an einer anderen Universität sammeln, nicht in richtiger Weise gelenkt werden und es werden besonders die vielen Anregungen, die dort wie nir[gends] wo anders von mathematischer und theoretisch-physikalischer Seite ... ungenutzt bleiben. Naturgemäß werden auch die Rückwirkungen fehlen, welche von Seiten der experimentellen Physik auf die Theorie und die Mathematik stattfinden sollen und die in Göttingen besonders wünschenswert sind. Denn bei der dortigen Entwicklung überwiegen gegenwärtig die mathematischen die experimentellen so erheblich, daß sie, man kann sagen, gegen ihren Willen fast alleinherrschend geworden sind. Ich glaube, daß die Mathematiker selbst darunter leiden und es gern sehen würden, wenn die experimentelle Physik einen größeren Einfluß auf das wissenschaftliche Leben Göttingens gewinnen könnte. Nach meiner Meinung das Schlimmste wäre die Berufung einer jungen physikalischen Mittelmäßigkeit, wie sie nach dem von mir gewonnenen Eindruck einzutreten droht. Dann wäre die Göttinger Physik auf mehr als ein Menschenalter lahmgelegt... Nach meiner Überzeugung würde dies eintreten, wenn einer der von manchen Seiten jetzt besonders empfohlenen Herren Franck, Pohl, Edgar Meyer nach Göttingen berufen werden. (...) ... ich kann nur bedauern, daß die Berufung Starks, den ich trotz seiner persönlichen Mängel für die bei weitem geeignetste Persönlichkeit halte, aus persönlichen Gründen unmöglich erscheint. Aber wenn die Berufung Starks wirklich ausgeschlossen ist, wäre es immer noch besser, einen nicht mehr ganz jungen Physiker zu nehmen, der jüngeren Forschern Arbeitsgelegenheit verschafft, als einen in organisatorischen Fragen ganz unerfahrenen zu berufen, der schließlich für vier Jahrzehnte dem Göttinger Institut voraussichtlich den Stempel der Mittelmäßigkeit aufdrücken würde.

Document 12

From a letter from Hilbert to Otto Naumann, director in the Prussian Ministry of Culture, 24 December 1915.³⁵⁴

Vom Briefe W. Wiens an Eure Excellenz in Angelegenheit der Wiederbesetzung des hiesigen Ordinariats für Experimentalphysik hat mir Herr Kollege Voigt Mitteilung gemacht. Meine Einwendungen gegen Stark-Aachen beziehen sich nur auf die Göttinger Stelle, für die ich allerdings nach wie vor die Berufung jedes anderen Physikers außer Stark, auch eines solchen, der nicht auf

³⁵⁴UAG Hilbert Papers, folder 466, Nr. 2.

der Liste steht, erheblich vorziehe. Doch sehe ich die Gefahr jetzt nach unserem Vorschlag und der neuerlichen Erklärung Eurer Excellenz als beseitigt an. Dem sehr gefälligen Wunsch Euer Excellenz über die neuen Untersuchungen von Debye näheres zu erfahren, entspreche ich außerordentlich gerne. Das Ziel desselben ist die experimentelle Erforschung der Materie bis in das Innere des Einzelatoms hinein. Die zur Erreichung dieses Ziels verwandte Versuchsanordnung habe ich in meinem neulichen Briefe als ein höchst verfeinertes Mikroskop bezeichnet. Damit hat es folgende Bewandnis: ... (...) ... es wird die von chemischer Seite schon als Hypothese angenommene Ringanordnung der sechs Kohlenstoffatome festgestellt und der Abstand zweier Atome voneinander zu 0,000 000 62 m.m. gemessen. Es liegt schon nach diesen ersten Proben auf der Hand, daß damit ein unabsehbares neues Feld der Experimentalforschung eröffnet ist, und ich möchte – mit etwas Kühnheit – sagen, daß mir eigentlich von nun an keine auf die Struktur der Materie bezügliche Frage mehr unbeantwortet bleiben kann, so groß auch noch die bis zu dem Ziel notwendig zu leistende Arbeit sein mag.

Document 13

From a letter from Otto Naumann, director in the Prussian Ministry of Culture, to Wilhelm Wien, 15 January 1916.³⁵⁵

Obgleich manche persönliche Eigenschaften an ihm seine Berufung an eine Hochschule nicht leicht machten, habe ich ihn seinerzeit dem Herren Minister für die zweite Physikerstelle in Hannover und später für die erste Physikerstelle in Aachen empfohlen. ... immer wieder durch Herrn Professor Rubens veranlaßt worden ... Freilich auf der anderen Seite erklärte er stets, an sein Institut würde er Professor Stark keinesfalls nehmen. Gegen die Berufung nach Göttingen sprachen sich dann sehr energisch die nächstbeteiligten Göttinger Professoren aus, die zu diesem Zwecke hier persönlich vorstellig wurden. (...) Sieht man von diesen jüngeren Vertretern ab [Meyer, Pohl, Koch], so frage ich: wer ist denn sonst der geeignete Experimentalphysiker für Göttingen? (...) ... auch folgende Lösung in Frage gekommen: Professor Debye in Göttingen, der zugleich anerkannter tüchtiger Experimentalphysiker ist, wird als Ordinarius für Experimentalphysik an die Spitze der physikalischen Institute in Göttingen gestellt, und die von ihm bekleidete Professur – ein Extraordinariat – wird einem zweiten jüngeren Physiker, z.B. Dr. Pohl, übertragen, etwa in der Weise, daß beide Herren abwechselnd Experimentalphysik lesen. Die theoretische Physik könnte dann noch auf Jahre hinaus durch Professor Voigt in Vorlesungen vertreten werden.

Document 14

From a letter from Wilhelm Wien to Otto Naumann, director in the Prussian Ministry of Culture, 17 January 1916.³⁵⁶

³⁵⁵GStPKB. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 366–367.

³⁵⁶GStPKB. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 368–369.

Zur ersten Liste möchte ich, obwohl sie als erledigt anzusehen ist, das folgende äußern. Falls eine Berufung ergangen wäre, würde ich sie ernstlich in Erwägung gezogen haben und zwar ohne Rücksicht auf die Möglichkeit einer Berufung nach München. Eine solche ist nicht aktuell. Niemand weiß, wann Herr Röntgen zurücktritt und wir haben Beispiele genug, daß die Universitätslehrer von 80 Jahren u. mehr im Amte bleiben. Allerdings kann ich nicht sagen, ob Verhandlungen mit mir zu Ziele geführt hätten, weil in der That die Verhältnisse des Göttinger Institutes nicht günstig sind. Die der Experimental-Physik zugewiesenen Räume sind verhältnismäßig klein, da die theoretische Physik sehr viel belegt hat. Wenn die Göttinger Kollegen sich so sehr gegen Stark verwahrt haben, so findet das auch in den eigentümlichen Institutsverhältnissen eine gewisse Berechtigung. Ein Zusammenarbeiten zwischen Debye u. Stark unter diesen Umständen würde so gut wie ausgeschlossen ..[?]. Die Sache wäre nur gegangen, wenn man Stark das ganze Institut überlassen und Debye anderswo untergebracht hätte. (...) Jeder der drei vorgeschlagenen würde die Experimentalphysik neben dem unvergleichlich bedeutenderen Debye kaum ausreichend vertreten können. Wenn ich von älteren Physikern gesprochen habe, die meiner Meinung nach vorzuziehen wären, so hatte ich dabei Zenneck ..[?] Dißelhorst (Braunschweig) im Auge... Beide sind keine großen Bahnbrecher... Zenneck ist ein vorzüglicher Organisator... (...) Falls Debye geneigt ist, die Professur für Experimentalphysik zu übernehmen, müßte er es meiner Meinung nach ganz thun ... empfehlen einen anderen Theoretiker zu berufen... Mie in Greifswald oder Laue in Frankfurt. Mie ist ein ganz hervorragender Theoretiker, was die Göttinger selbst am besten wissen. Debye und Mie würden jedenfalls die Physik so glänzend vertreten, daß ich ... diese Lösung ... für die bei weitem beste halte.“

Document 15

From a letter from Hilbert and Klein to the Prussian Ministry of Culture, 29 January 1916.³⁵⁷

Von dem gleichen Wunsche aus erlauben wir uns hier auch noch über Debye's augenblickliche finanzielle Stellung einiges anzugeben. Als Debye im Sommer 1914 nach Zürich in die gleiche Stellung berufen wurde, in die jetzt Edgar Meyer eintritt, wurde ihm dort eine Jahreseinnahme von 25.000 Frs. = 20.000 Mark in sichere Aussicht gestellt. Aber Herr Geheimrat Elster war nicht in der Lage, ihm in Göttingen eine höhere Summe als 12.000 M. zu garantieren. Daher haben die Göttinger Fachkollegen Debyes von sich aus damals Maßregeln getroffen, um Prof. Debye, dessen Verbleiben allseitig als eines der wichtigsten Interessen der Universität erachtet wurde, wenigstens die Hälfte der Spanne zwischen 12.000 M. und 20.000 M. zu ersetzen. Es ist dies, wie Ew. Exzellenz damals vielleicht nicht ausdrücklich mitgeteilt wurde, in der Weise gelungen, dass a) Prof. Voigt zugunsten von Debye auf die Hälfte seines Gehalts verzichtet (was 2000 M. ergab), und b) die Fermatkommission der Gött. Ges. d. Wissenschaften einwilligte, aus den Zinsen des Wolfskehl'schen Legats bis auf weiteres

³⁵⁷GSStPKB. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXIV, Bl. 372–373.

2000 M. zuzuschiessen. Diese 4000 M. jährlich sollten nun wohl bei jetziger Gelegenheit seitens des Staates irgendwie abgelöst werden. In der Tat kann doch das Provisorium nicht dauernd aufrecht erhalten werden. ... Denn Prof. Debye erreicht jetzt schon, in Kriegszeiten, wo er nur eine theoretische Vorlesung hält, ungefähr die Höhe meiner Kolleggeldgarantie und seine Einnahme aus Kolleggeldern muss doch selbstverständlich bedeutend wachsen, wenn er seinen Teil der Experimentalphysik mitübernimmt.

Document 16

From a letter from the Minister of Culture (Haenisch) to Minister of Finance (Erzberger), 9 January 1920, discussing the national dimension of Debye's imminent departure from Germany as well as the danger that Hilbert also might leave Göttingen.³⁵⁸

... die Gefahr immer drohender, dass wir Deutschland, nachdem wir es in immer steigendem Masse von materiellen Gütern entblösst worden sind ist, nun auch an geistigen Werten verarmen. Wie auf wirtschaftlichem Gebiet benutzt das Ausland den Tiefstand der Valuta, um besonders ausgezeichnete Vertreter deutscher Wissenschaft für sich zu Bedingungen zu gewinnen, denen die preussische Unterrichtsverwaltung auch nicht annähernd folgen könnte, wenn sie sich an die für normale Verhältnisse gegebenen ... Richtlinien halten müsste. ... Pflicht ..., das Aeusserste zu tun, um die drohende geistige Verarmung und die daraus folgende Ausschaltung Deutschland auch aus dem geistigen Wettbewerb der Nationen zu verhindern. Vielmehr muss sie bestrebt sein, im Rahmen des Möglichen über die heimatlichen Verhältnisse im engeren Sinne hinaus nationale Kulturpolitik zu treiben, um das Ansehen der deutschen Wissenschaft in der Welt zu erhalten ... Von dem Polytechnikum in Zürich hat der Göttinger Physiker Debye einen materiell wie ideell sehr vorteilhaften Ruf erhalten. ... dessen Zugehörigkeit zur deutschen Wissenschaft immer ein besonderer Gewinn für Deutschland gewesen ist, ... Der Versuch, ihn abzuhalten, muss schon aus Gründen des nationalen Ansehens gemacht werden. Hierfür ist vor einiger Zeit auch die Vertretung der gesamten deutschen Studentenschaft entscheiden eingetreten, und auch die Preussische Landesversammlung hat in einer förmlichen Anfrage den Wunsch, Debye für Deutschland zu erhalten, ausdrücklich kundgegeben. Zudem ist zu befürchten, dass, wenn Debye nach Zürich geht, ihm der berühmte Göttinger Mathematiker Hilbert folgt und dass so die in ihrer Bedeutung weltbekannte naturwissenschaftliche Fakultät der Universität Göttingen ihres Gehalts im wesentlichen beraubt wird.

Document 17

From an autobiographical sketch "Über meine Tätigkeit in Göttingen," typescript written after January 1932.³⁵⁹

Meine Heimat ist Königsberg, wo ich als Sohn einer alten ostpreußischen Juristenfamilie im Jahre 1862 geboren wurde. (...) Im Jahre 1895, 33 Jahre

³⁵⁸GStPKB. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Bd. XXVI, Bl. 166–168

³⁵⁹UAG Hilbert Papers, folder 741, Nr. 7, Bl. 24–30. Printed in (Reidemeister 1971, 78–82).

alt, wurde ich von Felix Klein nach Göttingen berufen, wohl wesentlich auf Grund meiner Arbeiten zur Invariantentheorie, deren zentrale Probleme ich von neuartigen Gesichtspunkten aus in Angriff genommen und gelöst hatte. Die Annahme des Rufes an die Stätte der alten mathematischen Tradition war selbstverständlich. Aber es fiel meiner rau[h] und mir keineswegs leicht, uns sofort in der damals etwas kühlen Göttinger Atmosphäre heimisch zu fühlen. Nicht selten wurden wir mit Kopfschütteln betrachtet, wenn wir uns voller Verständnislosigkeit über die strengen Rangunterschiede hinwegsetzten und zwanglos mit Privatdozenten und gar Studenten verkehrten. Aber bald hatten wir festen Fuß gefasst. Wir fanden uns mit einer Reihe jüngerer Freunde – unter denen auch Walter Nernst war – zu einer Gruppe Gleichgesinnter zusammen. Mit Felix Klein und seiner Frau verbanden uns trotz der Verschiedenheit der Temperamente bald ein völliges Vertrauen und die gemeinsamen Interessen. Von Anfang an habe ich mich freudig in den Dienst des Kleinschen Ziels gestellt, Göttingen zu einem Zentrum der mathematischen und physikalischen Wissenschaften zu machen. In allen Fragen der Organisation hatte Klein die unbestrittene und unbedingte Führung; um Dinge der Verwaltung habe ich mich nie gekümmert. Aber wenn es sich um wesentliche Entscheidungen handelte, insbesondere bei Berufungen, bei Schaffung neuer Stellen und dergl., habe ich stets aktiven Anteil genommen. Ich glaube, daß die absolute Einigkeit zwischen Klein und mir sowie den anderen Kollegen, die in allen wichtigeren Fragen bestand, für den Erfolg der Kleinschen Bestrebungen vielfach von großer Bedeutung gewesen ist. Einer dieser Erfolge war die Errichtung einer weiteren mathematischen Professur, in welche mein nächster Freund, Hermann Minkowski, berufen wurde. Es war mir so trotz manchen Verlockungen (mehrfache Rufe nach Berlin, Leipzig, Heidelberg, Bern) nicht schwer, dem Göttinger Wirkungskreise treu zu bleiben und mein wissenschaftliches Lebenswerk hier stetig auszubauen. Entscheidend für meine Tätigkeit ist die denkbar engste Verbindung zwischen Forschung und Lehre gewesen. (...) Es war mein Grundsatz, in den Vorlesungen und erst recht in den Seminaren nicht einen eingefahrenen und so glatt wie möglich polierten Wissensstoff, der den Studenten das Führen sauberer Kolleghefte erleichtert, vorzutragen. Ich habe vielmehr immer versucht, die Probleme und Schwierigkeiten zu beleuchten und die Brücke zu den aktuellen Fragen zu schlagen. Nicht selten kam es vor, daß im Verlauf eines Semesters das stoffliche Programm einer höheren Vorlesung wesentlich abgeändert wurde, weil ich Dinge behandeln wollte, die mich gerade als Forscher beschäftigten und die noch keineswegs eine endgültige Gestalt gewonnen hatten. Höhere Vorlesungen dieser Art führten oft zu einer engen Wechselwirkung mit den Zuhörern, welche ihrerseits mit Kritik oder eigenen Gedanken hervortraten. In den Unterhaltungen nach der Vorlesung, bei Spaziergängen und Radfahrten, im Garten, bei Gesellschaften und überhaupt bei jeder sich bietenden Gelegenheit wurden oft solche Diskussionen mit Schülern oder Kollegen fortgesetzt. Das gemeinsame Seminar mit Minkowski und die von Klein gegründete mathematische Gesellschaft mit ihren Nachsitzungen gaben jede Woche neue Anregungen; dieser wissenschaftliche Betrieb wurde in seiner Intensität gesteigert durch die Atmo-

sphäre von Kameradschaft und menschlicher Zusammengehörigkeit, durch die Verbundenheit mit der schönen Natur, durch die Geselligkeit, die den Göttinger mathematischen Kreis unter Führung meiner Frau zu einer großen Familie vereinigte. (...) Im Jahre 1909 unterbrach ein jäher Schlag den gleich mäßigen Fluß der Dinge. Mein Freund Hermann Minkowski starb ganz plötzlich, und es blieb menschlich und wissenschaftlich für mich eine tiefe Lücke zurück. (...) Kleins umfassende Pläne zur Stabilisierung der Göttinger Einrichtungen schritten fort. ... In dem Moment, wo alles vorbereitet war, das Grundstück und die Mittel bereit standen, brach der Krieg aus. Unser Kreis zerstob in alle Winde. (...) Ich selbst habe mich damals vorzugsweise mit Physik und Relativitätstheorie beschäftigt und nur wenige, zum Teil ausländische, Schüler gehabt. Bei Kriegsende schienen mit dem Zusammenbruch alle Hoffnungen auf die Vollendung der Kleinschen Pläne begraben zu sein. Die Göttinger Vereinigung löste sich auf. Die mathematischen Einrichtungen, insbesondere das Lesezimmer, befanden sich einem Zustande des Verfalls. Mittel zum Wiederaufbau schienen nicht vorhanden zu sein. Und diese Lage war nicht auf Göttingen beschränkt. So schien z.B. das Drucken von mathematischen und wissenschaftlichen Büchern unmöglich zu sein. In dieser Situation habe ich dann aktiver eingegriffen als ich das früher getan hatte. (...) Klein und ich waren uns darüber einig, daß alles versucht werden mußte, um einen Wiederaufbau anzubahnen, und daß hierfür die Frage der Neubesetzungen der freigewordenen Plätze von ausschlaggebender Bedeutung sei. In der Physik fiel die Wahl auf Franck und Born. In der Mathematik haben Klein – welcher sich wegen seiner Krankheit zurückzog – und ich die Berufung von Courant bewirkt, welcher die Verpflichtung übernahm, die Kleinschen Pläne aufrechtzuerhalten und fortzusetzen. Zunächst wurde mit Hilfe von Freunden aus der Industrie und zum Teil auch aus Amerika die Inflationszeit überwunden. Schon hierbei bewährten sich die alten Verbindungen, die durch Kleins und meine frühere Lehrtätigkeit in der ganzen Welt angeknüpft worden waren. Dann im Jahre 1927 schien die Zeit reif zu sein, um die alten Aufbaupläne von Klein zu erneuern. Leider hat Klein das Wiederaufblühen seiner Pläne nicht mehr miterlebt. – Unterstützt von Niels und Harald Bohr in Kopenhagen nahm Courant damals Verhandlungen mit der Rockefeller Foundation auf. Das große Ansehen, das sich die Göttinger Mathematik und Physik nach dem Kriege rasch wieder erobert hatte, bereitete den Weg. Nach langwierigen Unterhandlungen bewilligte die Rockefeller Foundation eine sehr erhebliche Summe für die Errichtung des mathematischen Institutes; das preußische Staatsministerium sicherte die Aufrechterhaltung der Mathematik und Physik auf dem entsprechenden Standard zu. Ende 1929 konnte das Göttinger mathematische Institut eingeweiht werden. Meinen 70. Geburtstag habe ich im Januar 1932 im neuen Institut unter dem Eindruck dieses Erfolges feiern können.

Abbreviations and Archives

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| AHQP | Archive for History of Quantum Physics. American Philosophical Society, Philadelphia |
| BCW | Niels Bohr: Collected Works. 12 Vols., Amsterdam 1972–2007 |
| DMA | Deutsches Museum, Archive |
| ECP | Albert Einstein: Collected Papers, Princeton 1987ff. |
| GStA PK | Geheimes Staatsarchiv Preußischer Kulturbesitz |
| Jahresberichte DMV | Jahresberichte der Deutschen Mathematikervereinigung |
| Nachrichten | Nachrichten von der Gesellschaft der Wissenschaften zu Göttingen |
| UAG | Universitätsarchiv Göttingen |
| UAF | Universitätsarchiv Frankfurt |

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