

MÜNCHNER ZENTRUM FÜR WISSENSCHAFTS- UND TECHNIKGESCHICHTE  
MUNICH CENTER FOR THE HISTORY OF SCIENCE AND TECHNOLOGY

Arbeitspapier  
Working Paper

Arne Schirmacher

Hilbert and Quantum Physics  
Two Conference Papers

Arne Schirmacher  
Hilbert and Quantum Physics  
Two Conference Papers

The papers of this preprint are based on presentations at two major conferences on history of science. Earlier versions of these talks were delivered at Stuttgart, Göttingen and Munich. Approach, method, and scope have their origin in the work of a research group on the history of quantum physics at the Berlin Max Planck Institute led by Jürgen Renn.

"The Establishment of Quantum Physics in Göttingen 1900-24. Conceptual Preconditions – Resources – Research Politics" was presented at the XXth International Congress of History of Science at Liège in 1997. It has been accepted for the proceedings volume "History of Modern Physics " edited by H. Kragh, P. Marage, and G. Vanpaemel to be published by Edition Brepols (Turnhout).

"Planting in the Neighbor's Garden – Hilbert's Investments in Early Göttingen Quantum Physics" was a contribution to the session "The methods of modern physics" of the 1998 Annual Meeting of the History of Science Society at Kansas City.

## The Establishment of Quantum Physics in Göttingen 1900-24

### Conceptional Preconditions – Resources – Research Politics

The name of Göttingen in the context of 20<sup>th</sup> century science is for most historians of this field closely associated to one of the major scientific revolutions, the establishment of quantum theory and quantum mechanics as a part of it. As self-evident as it may seem that Göttingen is one of the places where quantum physics developed, it is, however, not so straightforward to arrange the various specific Göttingen contributions to quantum theory such that they combine to a satisfactory story. We will hence ask first of all, whether between 1900 and 1924 a line of local developments can be found at all that covers at least a considerable part of this period. (We do not discuss here the final short episode of the construction of a new mechanics, called quantum mechanics, as the emphasis is on the preceding shift of focus establishing the foundations for it.) The perspective of conceptional development, for instance, turns out to be of limited expedience. In presenting a case study (or rather the sketch of it) we will argue that the analysis of research politics is a particularly suitable perspective for exhibiting driving forces for local scientific development.

In the early years of quantum theory, right after the turn of the century there were but few contributions to this field from Göttingen scientists. Walter Ritz' combination principle was of some importance, put forward in his dissertation in 1903 and was more prominently presented in a paper of 1908. Max Abraham, who obtained his doctorate with Planck at Berlin in 1897, where he remained his assistant until 1901 and who had witnessed Planck's discovery of the radiation law, tried himself to contribute to the theory of black-body radiation in 1904 in a *Festschrift* on the occasion of Boltzmann's 60<sup>th</sup> birthday. This, however, was just another solitary publication of a Göttingen scientist in the first decade of the 20<sup>th</sup> century.<sup>1</sup> Paul Ehrenfest was an influential figure in the history of quantum theory and he spent some time in Göttingen, however, without being able to gain position or influence.<sup>2</sup> More prominent work was done by Walter Nernst and Johannes Stark. Nernst developed his heat theorem while preparing to leave for Berlin. Stark arrived at his discovery only after he had left Göttingen, having taken with him the idea from Wolde-  
mar Voigt the leading theoretician there.<sup>3</sup> As a consequence, none of the given scientists

---

<sup>1</sup> Ritz had originally formulated his combination principle in spectroscopy in his thesis in 1903 ("Zur Theorie der Serienspektren," *Annalen der Physik* 12 (1903), 264-310), however, only some years later it appeared more prominently in print as "Über ein neues Gesetz der Serienspektren," *Physikalische Zeitschrift* 9 (1908) 521-529. M. Abraham: *Der Lichtdruck auf einen bewegten Spiegel und das Gesetz der schwarzen Strahlung*, in: *Festschrift für Ludwig Boltzmann*, Leipzig: Teubner, 1904. Abraham did his doctorate with Planck and was his assistant from 189 to 1901 and hence witnessed the work leading to his law.

<sup>2</sup> Cf. M. Klein: Paul Ehrenfest: Vol. 1, The making of a theoretical physicist, Amsterdam: North Holland, 1970.

<sup>3</sup> J. Stark: *Erinnerungen eines deutschen Naturforschers*, A. Kleinert, ed., Mannheim: Bionomica, 1987., p. 22.

or their works can serve as an example making Göttingen a particular fruitful environment open to embark on the field of quantum theory.

A greater continuity can be found in the case of Max Born. In turning to Thomson's atom for his Habilitation lecture and in attending Einstein's Salzburg talk in 1909, he recognized the quantum question and contributed to it constantly; first with Rudolf Ladenburg on black-body radiation, then with Theodore von Kármán in papers on specific heat (that came out at the same time as Peter Debye had formulated his more suggestive theory) and finally with papers alone and one with Richard Courant from 1911 on. Born, however, hardly had the standing to define a research program for the Göttingen physics and from 1915 to 1921 he wasn't there anyway.

Both Born, then at Berlin, and Debye who just had come to Göttingen focused during the war independently on the question of what results would come from Bohr's atom if taken seriously. Born working with Alfred Landé and Erwin Madelung – so to speak at a Berlin outstation of Göttingen physicists in war duty – used Bohr's atoms as building blocks for the constitution of crystals while Debye was working with his Swiss student Paul Scherrer and guided by similar ideas. They finally arrived at a most welcome method for diffraction of X-rays at crystal powder though not at Bohr's electron rings as initially hoped for. Debye also brought in the quantum for the Zeeman effect (in parallel with Arnold Sommerfeld) while at Göttingen. After the war he left for better living conditions to Switzerland. During the war years the quantum as the key to the atomic structure of matter somehow became firmly the Göttingen *credo* which after Born and Franck had filled the physics vacuum in 1921 opened a road towards quantum mechanics.

Having told the story this way, it becomes questionable what a local perspective would lead to. It might just confirm the view that the only sensible way to approach the historical development of quantum theory is a conceptual one that focuses on the spaceless discussions in the journals, possibly enriched by some celebrated conferences and meetings. Everything else becomes part of the ingenuity of the single researchers. The only context were then the cognitive one of the personal profile of knowledge and world views. A wider context of the creation of knowledge referring to particular local circumstances would get out of sight.

To evade this conclusion we suggest to exploit 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 inherent logical structure of scientific theory, a local story of the Göttingen establishment of quantum theory becomes feasible. We will call this scientific entrepreneurship *research politics* as it comprises decisions to direct or re-direct resources in a way that favors or encumbers research in a certain field, irrespectively whether these resources are financial, personal, regarding the public perception or even the relevance for exploitable technology etc. In this sense looking for research politics is at the same time a kind of an economical or resource-oriented history of science, for it asks what resources have been invested to what anticipated end. These expectations and hopes, however, do often not agree with the successes the investments lead to, a fact hard to handle from a viewpoint targeted at a conceptual development. In short, we 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.

## Resources and Research Politics

To characterize the research politics involved in the establishment of quantum theory in Göttingen we analyze the following set of indicators:

1. Changes in physics faculty. – Is there a decisive pattern in the choice of new personnel? Who was instrumental in the decisions? What were the original intentions?
2. Shift of focus of leading researchers. – Was there an obvious change of the way in which researchers spend their time on new topics? How did they use their subordinate personnel? Did groups of researchers change their discussion topics?
3. Spending funds. – Is there a direct monetary support for a research field to be found?

As we cannot discuss these points extensively here, we will rather give three clear examples on the basis of which we will put forward a thesis to be discussed at length elsewhere.<sup>4</sup>

(1) An example for a change in the physics faculty towards the quantum: the Riecke succession

The succession of Eduard Riecke, holding the experimental physics chair until 1915, was a clear instance for turning the interest towards physicists that stood for quantum physical research. Riecke, who had come to Göttingen in 1870, became professor in 1873 and succeeded his teacher Wilhelm Weber as full professor in 1881.<sup>5</sup> He and his long-time colleague Woldemar Voigt did not separate their research fields and equipment but demonstrated great harmony running the institute together and sharing its facilities. As Riecke tended more to the experimental side Voigt taught theory.

Not much can be found about Riecke's attitude to modern physical theory; as to his views on the future of the quantum there is, however, one remark of significance. In a letter to Stark Riecke wrote in October 1911, that

... I do not consider 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.<sup>6</sup>

In Voigt's eyes, Riecke stood for a similar modest and conservative research program of precise measurements as his own:

And where he performs experiments, it is usually never a question of pioneering into undeveloped fields but doing measurements under the guidance of theory.<sup>7</sup>

When Riecke finally asked for retirement due to ill health in fall 1914, he did not fight for a specific successor nor for a specific field, but merely required that a "fresh person in

---

<sup>4</sup> A. Schirmmacher: "Establishing the Quantum in Göttingen. Hilbert's Investments in Physics 1909-1919," to appear.

<sup>5</sup> For literature on Riecke see S. Goldberg. "Riecke, Eduard" in *Dictionary of Scientific Biography* (New York: Scribner, 1970-80) 18 Vols., C. C: Gillispie, ed., in the following cited as *DSB*, vol. XI, pp. 445-447, C. Jungnickel and R. McCormach: *The Intellectual mastery of nature: Theoretical Physics from Ohm to Einstein. Vol. 2, The now mighty theoretical physics 1870-1925*. Chicago: University of Chicago Press, 1986.

<sup>6</sup> Riecke to Stark, Oct. 13, 1911, Sammlung Damstädter, cited after R. Tobies: "Albert Einstein und Felix Klein," *Naturwissenschaftliche Rundschau*, 47 (1994): 345-352, here p. 348.

<sup>7</sup> W. Voigt: "Eduard Riecke," *Chronik der Georg-August-Universität zu Göttingen*, Vol. 1915, Göttingen 1916, p. 6-8, here p. 7.

the middle of his development" should take over his position. Already in December 1914 the faculty presented a list to the ministry that favored Wilhelm Wien. Wien was besides Lorentz the only other renowned supporter of Planck's theory by 1910 and constantly contributing to the discussion on quantum physics.<sup>8</sup> It appears, however, that Riecke's chair was never officially offered to Wien.<sup>9</sup> To the ministry it may have seemed in vein to negotiate with Wien the would-be successor to Röntgen in Munich. As early as one year in advance the simultaneous retirement of Riecke and Voigt had been agreed on and since then it was planned to find a single new director of outstanding quality.<sup>10</sup> The second candidate was Friedrich Paschen of Tübingen, who met in particular this requirement of being able to combine experiment and theory in a way Voigt and Riecke had performed collectively.<sup>11</sup>

Paschen's research in his early career at Hanover stood for competition with the Berlin group working on black-body radiation, viz. Rubens, Lummer, Pringsheim, and 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.<sup>12</sup> 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.<sup>13</sup>

Paschen's relation to Göttingen goes back to a collaboration with Carl Runge on spectroscopy. It was also the influence of Göttingen student Walter Ritz who spent the winter 1907/08 in Tübingen, that made him look for and find the "Paschen series." The Paschen-Back effect of 1912 again was based on Ritz' conceptions, and "was immediately seized upon as potentially one of the most revealing clues to atomic structure and the mechanism of emission of spectral lines."<sup>14</sup> In 1914 Paschen began to work on "Bohr's helium lines," an occupation that should distract him from hurrying to the colors and became further of utmost importance after in November 1915 Sommerfeld wrote to him inquiring about data for his theory thus starting a fruitful collaboration.<sup>15</sup>

---

<sup>8</sup> Cf. T. S. Kuhn, *Black-body theory and the quantum discontinuity*, New York: Oxford University Press, 1978, p. 202ff. Publishing on the laws of black-body radiation almost on a yearly basis from 1893 to 1901 and from 1907 to 1915, Wien clearly represented experimental as well as theoretical expertise on quantum matters.

<sup>9</sup> Ministry to Göttingen Curator, July 2, 1915, writes that appointing Wien was "hopeless." Geheimes Staatsarchiv Preußischer Kulturbesitz Berlin, in the following cited as *SPK*, Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Vol. XXIV, p. 181.

<sup>10</sup> Curator to Minister, Apr. 18, 1914. *Ibid.*, p. 69-70.

<sup>11</sup> For Paschen's view of the relation between experiment and theory cf. Paschen to Sommerfeld Nov. 14, 1904, Deutsches Museum, Munich, archive, HS 1977-28/A, 253.

<sup>12</sup> Cf. H. Kangro: "Das Paschen-Wiensche Strahlungsgesetz und seine Abänderung durch Max Planck," *Physikalische Blätter* 25 (1969) p. 216-220. Compare also Heinrich Kayser's critical view on Wien's work in his *Erinnerungen* edited by M. Dörries and K. Hentschel, Munich: Algorithmus, p. 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."

<sup>13</sup> P. Forman: "Friedrich Paschen," *DSB*, vol. X, pp. 345-353.

<sup>14</sup> *Ibid.*

<sup>15</sup> *Ibid.*; letters Paschen to / from Sommerfeld at Deutsche Museum, archive, *Nachlaß* Sommerfeld; F. Paschen: "Bohr's Heliumlinien," *Annalen der Physik*, 50 (1916): 901-940.

Paschen considered the Göttingen offer seriously and securing working conditions in spectroscopy was a central point to him. Like Wien, he saw resources inappropriately distributed among the experimental and theoretical sides, e. g. the new diffraction grating bought with support of the *Göttinger Vereinigung* in 1911 had come to Voigt's facilities. Under the condition of a new allocation of rooms and that "instruments for spectroscopy" would be transferred to him, Paschen signed an agreement with the Prussian ministry to accept the Göttingen offer. When he tried again to improve the offer Elster, the Berlin official in charge, declared that Paschen "couldn't make up his mind" and asked for a new list of candidates making Hilbert and Debye hurry to Berlin.<sup>16</sup> Having overacted Paschen stayed in Tübingen on a lower income than the one Göttingen had offered.<sup>17</sup> According to the first list, it was then the turn of Stark "first" and Zeeman "second." But it was added: "We can, however, not suppress certain doubts in other respect [than their scientific abilities] against them."<sup>18</sup>

In December 1915 Wien took over the initiative to solve the Göttingen problem of the full professorship in experimental physics as he saw it "in the general interest of our science": The position could become very important for the "future development of the German physics" or it could go down to "entire insignificance," he wrote to the ministry.

[I]f no really productive person [*Kopf*] comes, all the younger workers in particular that gather there like hardly at 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 also the response will be absent that should come from the experimental physics side 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.<sup>19</sup>

This unequivocal assessment points to two distinctive characteristics of the Göttingen physics development in the 1910s. First, the guiding role of mathematics for the physics development as source of inspiration for theory is noticed. Here Wien's term "autocratic" might be read as turning to criteria of inner consistency and notions of simplicity that motivated expectations for physical relations. 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 at Göttingen. Thus Wien's comments both acknowledge the extraordinary 'driving power' of young talent and foreshadow, however depreciatingly, theories like the later Göttingen matrix mechanics. Wien went on picturing an 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 when one of those from some sides particularly recommended gentlemen, viz. Franck, Pohl, Edgar Meyer would be appointed at Göt-

<sup>16</sup> *Vereinbarung* of Paschen with Elster, June 19, 1915. Paschen to Elster and Debye to Elster June 23, 1915. Paschen to Elster June 27, 1915. Elster to Curator and Elster to Voigt July 2, 1915. Telegram Hilbert and Debye to Elster July 6, 1915. *SPK*, Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Vol. XXIV, p. 307-338.

<sup>17</sup> Göttingen offered 8400 M basic salary, Tübingen 6500M. *Vereinbarung*, *ibid.* p. 307-309; P. Forman *DSB*, vol. X, 345-35.

<sup>18</sup> Dekan to Minister, Dec. 24, 1914. *SPK*, Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, Vol. XXIV, p. 124-126v.

<sup>19</sup> Letter of Wilhelm Wien to Otto Naumann, director in the Ministry of Culture, Dec. 4, 1915, *ibid.* p. 341-342.

tingen. (...) [!] ... can only regret that the appointment of Stark, whom I consider despite his personal shortcomings by far the most appropriate candidate, seems for personal reasons impossible. But if the appointment of Stark really is excluded, it will still be better to take a not quite so young physicist who 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.<sup>20</sup>

The Göttingen faculty was finally supported in their decision against Stark by the ministry after they found out that even strong supporters of Stark did not like to have him in their own laboratory.<sup>21</sup>

At this time the ministry explicitly asked for the opinion about two other candidates not mentioned by the Göttingen faculty. This is an interesting case 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 superior choice while proposing the Berlin *Privatdozent* Robert Pohl, who had explicitly been excluded by Wien, indicates that the close interaction of theory and experiment was not seen necessarily worth preserving.<sup>22</sup>

position	list of the Göttingen faculty
no vacancy 1914 (Voigt 1915 anticipated)	Debye
Riecke 1914	1. W. Wien – Würzburg 2. Paschen – Tübingen 3. "zuerst" Stark - Aachen, "zuzweit" Zeeman - Amsterdam <u>new list 1915</u> 1. E. Meyer- Tübingen, 2. <b>Pohl</b> , Berlin, and P.P. Koch- München [Franck, Schweidler, Geiger, Grüneisen for job talks]
Simon 1919 (Institute for applied electricity)	1. M. Wien 2. Gaede <u>new list</u> 1. Rüdénberg, engineer Siemens and PD TH Berlin 2. Barkhausen, Dresden 3. Möller PD Hamburg or <b>Reich</b> , PD Göttingen
Debye 1920	1. <b>Born</b> - Frankfurt, 2. Madelung- Kiel, 3. Lenz- München

Robert Pohl was nominated second on the new list of July 1915, Edgar Meyer first and Peter Paul Koch third. When the dean of the Göttingen faculty reported on the selection procedure and the candidates' talks that could only have been arranged with difficulty, all three physicists were characterized as being interested and competent in the field of atomic physics that became closely related to the quantum after Bohr's theory. As the

<sup>20</sup> Ibid.

<sup>21</sup> Cf. Lummer's attitude in letter from Otto Naumann, director in the Prussian Ministry of Culture, to Wilhem Wien, Jan. 15, 1916, *ibid.* p. 366-367

<sup>22</sup> Elster to Curator, July 2, 1915. *Ibid.* p. 181.



candidates should represent "pure physics in the sense of W. Weber and E. Riecke" and show off "especially the by the latter so successfully pursued and highly topical fields of molecular physics and radioactivity" to its best advantage, Meyer qualified by his work on "radioactive oscillations" that allowed determine the number of atoms per gram atom and won the lead by his work on the photoelectric effect that allowed him to "penetrate even deeper into the atomistic phenomena."<sup>23</sup> Pohl having a "gift for precision physics" in turn was told to open 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"<sup>24</sup> Even Koch who after the judgement of his teacher Sommerfeld was neither a theoretical nor a mathematical physicist "but only a brilliant physical technician"<sup>25</sup> 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; concerning Gaede only doubts of his appropriateness for the position were mentioned.<sup>26</sup>

At this point we can safely state that at least a coincidence of a shift of focus towards quantum physics and the reorganization process of the Göttingen physics staff is unmistakable. It appears that work in this field was the best qualification for a position in Göttingen after 1911. All younger staff was considered only by their prospective abilities in the fields of quantum or atomic physics. A closer look at the lists of candidates for the main physics chairs especially at times of reorganization in 1914/15 and 1920/21 interestingly shows that researchers in the field of quantum physics, in particular in the first list, and atomic physics, in particular in the second, are not only first but also often the only choice. No case was found were advocates of the quantum had to compete with other research fields; classical fields like those Gaede stood for and that had been still represented by Riecke and Voigt had ceased to play a role for the main physics chairs.

The long-standing wish to replace Riecke by Wilhelm Wien can be seen as a sign for a turn towards accepting quantum physics as Wien's name and work is inseparably linked to the quantum. As all candidates of the list from December 1914 were important figures in the development of quantum theory, it is fair to assume that decidedly candidates in this field were chosen. Debye, finally, was clearly a representative of quantum physics and his appointment was meant to strengthen this topic at Göttingen. He engaged in quantum research both theoretically and experimentally. His replacement with Born and Franck in 1921 can thus be seen as a continuation of a research program rather than an outset of a new one. By 1915 work on quantum physics or interest into the quantum structure of the atom was indispensable for candidates of the physics faculty. The shift of interest away from Voigt's and Riecke's research programs and in particular their pessimism about quantum theory was already completed 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 in the middle of the 19th century by Franz Neumann give any indication that they were in any way influential in this shift of focus. Who else could have been? The "personal" full professor Wiechert may have been to some extent, the applied physicist Simon and Prandtl were definitively not. We cannot but consider – as already mentioned at various points – the mathematicians and their leading figure David Hilbert.

---

<sup>23</sup> Dean to ministry Dec. 18, 1915. Ibid. p. 348-354.

<sup>24</sup> Ibid.

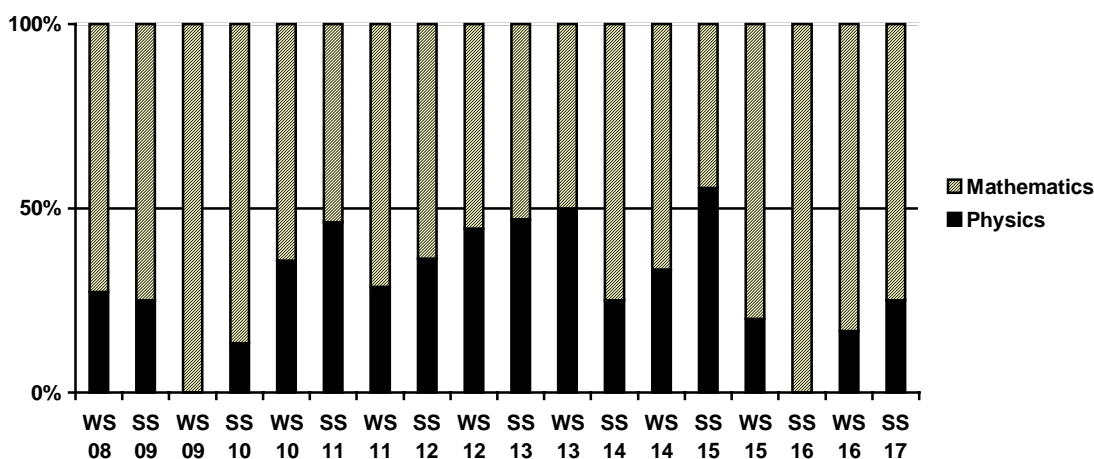
<sup>25</sup> Sommerfeld to Wien, June 1, 1916. Archive Deutsches Museum *Nachlaß* Wien, box 010.

<sup>26</sup> Cf. note 23.

(2) Examples for investments in physics: discussion topics in the *Mathematische Gesellschaft* and Hilbert's assistants

The first indicator for the involvement of mathematicians in physics matters we would like to present are the discussions in the *Mathematische Gesellschaft*. It turns out that for the period under consideration a particular high percentage of talks presented to this congregation of selected mathematicians and members of the philosophical faculty dealt not with mathematics but rather with physics topics.<sup>27</sup>

**Talks in the Mathematische Gesellschaft**



Between winter term 1908/09 and summer term 1916, two terms without discussions on physics in the *Mathematische Gesellschaft* at all, physics topics became a major concern as they roughly occupied half of the time; in summer 1915 they even dominated.

While the spending of time and thought in the *Mathematische Gesellschaft* is a non-monetary indicator, the appointment of assistants already involves funds. Hilbert had a regularly paid assistant from 1905 on, before so-called *Privatassistenten* like Max Born in 1904 were compensated by private contact to the grand scientist. Until 1912 Hellinger, Haar, Courant, Behrends, and Hecke worked for Hilbert by writing down and helping to prepare lectures as well as doing some teaching on their own. When in 1912 Hilbert was granted a second assistant this position was intended for a different purpose: While Hecke remained Hilbert's mathematics assistant until the war, the other position was filled with "physics assistants," first with Paul Ewald and later with Alfred Landé. Similar as for the physics investments of the *Mathematische Gesellschaft*, Hilbert spent roughly half his funds available on physics assistants, however, of a special profile. Both Ewald and Landé had been in Göttingen before and had had contact to mathematics, later they had been educated by Sommerfeld in Munich.

<sup>27</sup> Cf. *Jahresberichte der Deutschen Mathematiker-Vereinigung*, 1908-17. Only talks on research topics were counted, not considered are general reports on literature, conferences etc. like those Klein regularly gave at the beginning of a term.

Landé is a particularly good example of what Hilbert obviously was seeking for: In an interview he recalls: "I went to Göttingen already a convinced quantum theorist,"<sup>28</sup> and describes 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."<sup>29</sup>

The fact that a mathematician spent part of his financial means in the borderland to a neighboring discipline rather than for central mathematical questions may not be too surprising. Hilbert's actions, however, went much further. Not only that he employed researchers of a standing the physics institute should have been striving for; moreover the physics assistant not only equaled the even higher qualified mathematics assistant in financial terms, he also was ranked more important. Hilbert wrote to the Ministry in spring 1913:

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

The mathematical assistant had only a narrow escape from financial degradation, as Hilbert cleverly proceeds:

My past 1. 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, that usually my second assistant gets.<sup>30</sup>

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

Another field of great importance for the understanding of Hilbert's role for the reshaping of the Göttingen physics research is his teaching. It happens that while Voigt needed to give lectures on standard higher mathematics for his physics students, Hilbert likes to lecture about his current interests in courses with standard titles as blackbody radiation is mentioned in his course on mechanics in summer 1911.<sup>31</sup> Between 1911 and 1914 Hilbert lectured about kinetic theory of gases, theory of radiation, molecular theory of matter, electron theory, electromagnetic oscillations and other physics topics.

(3) An example of redirecting funds, or how Fermat's last theorem becomes a matter of quantum physics

A last example for both the mathematicians' investments in physics and Hilbert's leading role is the spending of extra funds like those of the Wolfskehl foundation. When the Jewish amateur mathematician Paul Wolfskehl from Darmstadt died in 1906 he had de-

<sup>28</sup> A. Landé, interview, Archive for the History of Quantum Physics, microfilm, 1962, session I, p.7.

<sup>29</sup> Ibid. p.5.

<sup>30</sup> Letter of Hilbert to Oberregierungsrat Ludwig Elster in the Prussian Ministry of Culture, April 6, 1914. *SPK*, I. HA Rep. 76, Nr. 591, p. 210.

<sup>31</sup> The lectures, notes taken by Hecke, can be found like many others and even seven more on mechanics in the Mathematisches Seminar at Göttingen.

creed by will that 100.000 Marks of his assets, the many years' salary of a professor, should become a prize to be donated to the person who would prove or disprove Fermat's last theorem. This was, as one might recall, on number theory, i.e. far away from any application or relevance for empiric sciences. It was up to the Göttingen Royal Society of Sciences to form a commission for setting up procedures and deciding about the award. It consisted of a physician Ernst Ehlers (one of two secretaries of the Society), Hilbert, Klein, Minkowski (after his death Landau) and Runge. Obviously, Hilbert was the head, as he signed for all reports and was identified as such,<sup>32</sup> all accounts on Wolfskehl support identify Hilbert with decisions, especially while Klein was ill from winter 1911 on and not fit to work for some time.<sup>33</sup> It turns out that only a very small fraction was actually 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 works on the Fermat problem of a Münster mathematician. No further appropriate spending has been found before ten years later when in 1919 a book on the Fermat problem earned 1500 Marks.

The money was instead misappropriated to win leading scientists for lecture series. While in 1909 the first 2500 Marks were spent to have Henri Poincaré come to Göttingen, in 1910 H. A. Lorentz followed lecturing "On the development of our conceptions of the ether" including a discussion of Planck's radiation formula. In 1912 Sommerfeld was richly paid for two talks on quantum theory in Hilbert's class. In 1913 the so-called *Gaswoche*, a conference with talks of Planck, Nernst, Debye (for Einstein who declined to come), Lorentz, Sommerfeld, and von Smoluchowski was organized. The summer term of 1914 brought Debye to Göttingen as a guest professor supported by Wolfskehl money. To keep him 2000 Marks were yearly subsidized to the regular professorial salary until 1916. In 1915 Wolfskehl money was spent to pay young physicists to give job talks in Göttingen for the Riecke succession. Later Mie, Born, Ehrenfest, Planck and others were invited.

The last remark also shows how the different examples of investing time or thought (*Mathematische Gesellschaft*), personnel (positions for professors and assistants), and money (Wolfskehl funds) we presented here are connected.

#### Spending of Wolfskehl funds

Apr. 22–29, 1909	<b>Poincaré</b>	six talks on integral equations and relativity, 2500 M.
Oct. 24–29, 1910	<b>Lorentz</b>	"On the developments of our conceptions of ether", discusses Planck's radiation formula
1911	<b>Zermelo</b>	5000 M. for his works in set theory and as a grant to allow recovering from illness
July 1912	<b>Sommerfeld</b>	1000 M., lectures on quantum theory in Hilbert's class
Apr. 21–26, 1913	<b>Planck, Nernst, Debye, Lorentz, Sommerfeld, von Smoluchowsky (Einstein declined)</b>	Congress, "Gaswoche" on "the kinetic theory of matter and electricity" 4800 M. spent (800 M. each)

<sup>32</sup> According to Günter Frei, Hilbert was president of the prize commission, see G. Frei, ed.: *Der Briefwechsel David Hilbert – Felix Klein (1886-1918)*, Göttingen: Vandenhoeck & Ruprecht 1985, p. 136. Cf. also "Bericht der Wolfskehlstiftung" in: *Jahresberichte der Deutschen Mathematiker-Vereinigung* 1909 ff.; I. Runge: "Carl Runge und sein wissenschaftliches Werk," in: *Abhandlungen der Akademie der Wissenschaften in Göttingen, math.-phys. Kl.*, III Folge, Nr. 23, Göttingen 1949, p. 149.

<sup>33</sup> G. Frei, op. cit. Runge op cit, p. 152.

summer term 1914	<b>Haar</b>	lectures on cosmogony; 2000 M. paid by Wolfskehl funds
summer term 1914	<b>Debye</b>	guest professor for theoretical physics; 1000 M.
winter 1914/15 until summer 1916	<b>Debye</b>	to raise professorial salary 2000 M.
in summer 1915	<b>Pohl, E. Meyer</b>	job talks of experimental physicists for Riecke position
June 1915	<b>Born, Sommerfeld, Einstein</b>	talk in Mathematische Gesellschaft
in winter 1915/16	<b>C. Schäfer, Koch, Franck, Schweidler, C. Müller</b>	job talks of experimental physicists for Riecke position
spring 1916	<b>Smoluchowski</b>	lectures in mathematical physics
June 4–6, 1917	<b>Mie</b>	talks on Einstein's theory of gravitation and matter
June 25–29, 1917	<b>Hecke</b>	lectures on mathematics
Dec. 11, 1917	<b>Born</b>	talk on liquid crystals in Mathematische Gesellschaft
[1918]	<b>[Ehrenfest invited]</b>	refused to come
May 14–17, 1918	<b>Planck</b>	lectures on the current state of quantum physics (in the end without payments from the Wolfskehl fund)
Dec. 16–19, 1918	<b>Driesch</b>	talks on "organic causality"
1919	<b>Bachmann</b>	for book "Das Fermatproblem" 1500 M.

## The Role of Conceptual Preconditions

Judging from his research interest Voigt might appear as one of the more receptive physicist of his age group to quantum ideas at Göttingen since he was interested in spectroscopy, did research on the Zeeman effect and it was also him who suggested to Stark to consider the splitting of spectral lines in the electric field. Also hierarchically he was at the leading position to steer the course of research work.<sup>34</sup> In addition he was well-informed about the quantum physical development. He was among the first who cited Planck's theory in a textbook in 1904 as a "noteworthy combination of probability considerations with the theory of the emission of waves by electric resonators."<sup>35</sup> He presented the formula and acknowledged its experimental adequacy but did not enter any discussion of its conceptual implications. When his disciple Drude went further in the second edition of his textbook on optics<sup>36</sup> this will not have eluded his attention, but again did not evoke second thoughts as neither did Einstein's 1909 Salzburg talk he had witnessed.

Why is it then that we found Hilbert, the mathematician, defining essentially the research program in theoretical physics and not Voigt? An analysis of their writings shows – which we cannot give in more detail here – that it is due to radically different ways of organizing knowledge. Roughly speaking, it is the difference between phenomenology

<sup>34</sup> Cf. S. Goldberg, "Voigt, Woldemar" in *DSB*, vol. XIV, pp. 61-63.

<sup>35</sup> Woldemar Voigt, *Thermodynamik*, 2 vols. Leipzig: Göschen, 1904. Cited after Kuhn 82, 135f. "Most of its closing chapter deals with such standard topics as Kirchhoff's law and the displacement law; ... 'By a most noteworthy combination of probability considerations with the theory of the emission of waves by electric resonators, M. Planck arrives at a formula which satisfies experiment in the entire region that has been investigated.' Planck's law is then presented, and the experimental determination of the two constants discussed. In a thirty-two-page chapter, 'Thermodynamics of Radiation,' nothing more is said about Planck's work." 135 f., citation on p. 355 in original.

<sup>36</sup> Paul Drude: *Optik*, Leipzig: Hirzel, 2. ed., 1906, pp.512-519. Cf. Russell McCormach: *Night thoughts of a classical physicist*, Cambridge, Mass.: Harvard UP, 1982, p. 197.

combined with a firm belief in the fruitlessness of imagining atomistic mechanisms out of reach of experimental verification on the one side and the conviction that a certain axiomatic and reductive thinking is the general method for all science hence negating any *ignorabimus* right away on the other.

Voigt's presentation of the body of knowledge on crystal properties in his 1910 textbook clearly provides most economical and mathematically attractive descriptions of the phenomena but shuns any commitment to explanation. This is, however, done deliberately. He was aware of the reductive and explanatory power of molecular thinking but judged the chances low to achieve progress by this conception. This can be seen as a cognitive restriction. Hilbert in turn told his students in his lecture on radiation theory that it were "above all the atomic theory, the principle of discontinuity, that becomes clearer and clearer these days and is not anymore a hypothesis but, like the teachings of Copernicus, an experimentally proven fact."<sup>37</sup> He convinced the persons in charge in the ministry of culture that mathematics and physics were about to become a single field. He wrote:

And when I should name a such a specific problem [Aufgabe] that can only be solved by mathematicians and physicists combined, then it is the understanding [Ergründung] of the structure of the atom: a great problem, that inaccessible until recently now is the focus of all scientific thought..<sup>38</sup>

It appears here, and many more examples could be given, that Hilbert had a rather clear vision about the fruits that could be harvested in the borderland field between mathematics, physics and chemistry. Having been successful with his axiomatic program in mathematics and in a number of applications in various fields,<sup>39</sup> Hilbert saw a methodological axiomatics as a key for the quantum riddle with the threat by the possibly inconsistently added quantum hypothesis. Possibly he dreamt of an identification of the simple rules governing the atomistic world and underlying all macroscopic phenomena with the axioms of physics that would result in a explanatory power outshining any phenomenological masterpiece.

Hilbert almost saw his vision come true in the beginning of 1915 when Debye met his expectations. In the Hilbert *Nachlaß* a draft for a letter to the ministry is preserved that reads with corrections:

Debye is proves to be the Newton of ~~chemistry~~ molecular physics and we got now in particular due to his latest discoveries from around Christmas [1914] the long in vain sought-after and in far distance believed foundation of a new mathematical chemistry. Thus Debye has become true compensation for Minkowski both in personal and in scientific respect.<sup>40</sup>

In both years 1915 and 1916 in which Hilbert was asked to nominate scientists for the physics Nobel prize he proposed Debye.

<sup>37</sup> Lecture notes by Hecke: "Strahlungstheorie" summer term 1912, Mathematisches Institut Göttingen, p. 2.

<sup>38</sup> Letter of Hilbert to Hugo Andres Krüss in the Prussian Ministry of Culture in Berlin concerning the establishment of a *Gastprofessur*, Oct. 3, 1913, SPK, Rep. 76 V a, Sekt. 6, Tit. IV, Nr. 1, vol. XXVII, p. 158-162, duplicate in *Nachlaß* Hilbert, *Handschriftenabteilung* Staats- und Universitätsbibliothek Göttingen, in the following cited as *SUB*, Folder 494, Nr. 9, p. 23-27

<sup>39</sup> Cf. Hilbert's course "Logische Prinzipien des mathematischen Denkens" summer term 1905 and its discussion in Leo Corry: "David Hilbert and the axiomatization of physics (1894-1905)," *Archive for History of Exact Sciences* 51 (1997), p. 83-198.

<sup>40</sup> From a draft of a letter of Hilbert to Geheimrat Ludwig Elster in the Prussian Ministry of Culture, Jan. 25, 1915, *Nachlaß* Hilbert, *SUB*, Folder 466, Nr. 1.

As physical theory later turned out to be much more complex than Hilbert but to lesser extent also Debye, Born, Sommerfeld and others had envisaged, it was nonetheless Hilbert's vision and forceful investment of his resources to this end that was a decisive factor for the reorientation of the Göttingen physics in the decade between 1909 and 1919.

In closing, two remarks are to be added:

In our discussion we started by somehow dismissing the conceptional view and arrived after a study of the investments of resources at analyzing the role of cognitive preconditions. This should not be seen as circular as the two concepts are quite different. The cognitive preconditions are themselves resources. They are inherited (education) or habits proven useful, they are driving ideas not unveiled truths.

As it may be a common insight at our times that the creation of new knowledge is the result of combining pieces of knowledge considered unrelated before, for instance by exploring disciplinary borderland problems, one might ask whether we can historically identify patterns for it. Can the example of this paper serve to describe a mechanism for scientific change? It might be a helpful vision to think so, but it won't come true. Further study, however, will probably identify a broader development in which mathematical input or investment turns out as a decisive *transforming power* in the development of 20<sup>th</sup> century physics.

#### Acknowledgements

Results communicated in this talk are part of a joint research effort of a working group at the Max-Planck-Institute for the History of Science, Berlin, that discussed the emergence of quantum physics research at its centers Berlin, Munich, Göttingen and Copenhagen comparatively. I would like to thank in particular Jürgen Renn for support, inspiration, and fruitful criticism at the various stages of this research.

# Planting in the Neighbor's Garden

## Hilbert's Investments in Early Göttingen Quantum Physics

### *Introduction*

Main questions

Hilbert's pre-quantum physics agenda: 1900, 1905

### *Neighbor's Garden*

Layout

No impact of the quantum

Background / reasons

### *Introducing new Plants and Fertilizers*

Hilbert: vision and background

Guest professors, Wolfskehl money ...

Peter Debye

### *Harvest*

Göttingen physics research during the war

Short-term results for Hilbert

Long-term impact for Göttingen physics

## Introduction

There are basically two questions that I am concerned with regarding the Göttingen way to quantum theory. First, how did the re-orientation of Göttingen physics from more classical work towards quantum theory take place? And second, when was the basis laid for the glorious and glorified Weimar era of Göttingen physics?

I do not believe that there actually was a big impact of so-called "Weimar spirit" or "culture;" I rather would like to adopt a more long-term and local view. How were the positions created that gave quantum physics a great share in the research programs? When was teaching taking up quantum topics and thus recruiting young researchers sacrificing their energy to the new fields?

As I cannot present such an analysis here within a few minutes, let me just give you the following summarizing thesis.

The establishing of quantum research at a rather provincial place like Göttingen hinged for a great part on the allocation of resources, i. e. positions, teaching time, discussion fora but also equipment like instruments, and the like. For this reason one should look for investments done with specific expectations. This way the perspective becomes a kind of an economical history of science.



All I would like to do today is to try to unveil some driving forces, investment objectives so to speak, and the cognitive preconditions related to them. I will look at to groups the physicists and those of the mathematicians engaging in a disciplinary imperialism toward physics.

The main player I will attribute decisive impact to, is the leader of the mathematics community, David Hilbert. What was his agenda towards physics?

Hilbert's axiomatic program, first applied to Euclidean geometry, gave rise to dwell on physics, most prominently demonstrated by the 6<sup>th</sup> problem of his 1900 Paris speech. It proclaimed the axiomatization of physics as a major aim of 20<sup>th</sup> century mathematics. In his 1905 lecture course on the "logical principles of axiomatic thinking" theories of mechanics, thermodynamics, electrodynamics and even psychophysics were used as examples for successful axiomatization.<sup>41</sup> In collaboration with Hermann Minkowski, who mainly raised his interest in physics, the application of the axiomatic method to rich examples of theories that were essentially considered *well-established and elaborated*<sup>42</sup> from the respective fields was the focus. This, however, and this is one of my more important points, changed. And most interestingly this change is related to the entry of the quantum.

From 1906 on Minkowski turned his interest towards Planck's theory and in the following year he gave a lecture course on heat radiation. He had made plans with Hilbert to begin together a bigger project on statistical mechanics and heat radiation starting from Planck's work. "It is my very intention" he told his audience,

and also professor Hilbert is of the same opinion and has similar aspirations, to win the pure mathematicians for the inspirations that flow into mathematics from the side of physics. It is not unlikely that we will treat in the seminars of the next years mathematical-physical theories in particular of heat radiation.<sup>43</sup>

I will hence argue later that the advent of the quantum made mathematicians, esp. Hilbert, reshape Göttingen physics. But let us first look over the fence onto the riches of the physicists' fields.

## Neighbor's Garden

For the mathematicians the neighbor's garden was physics, theoretical physics. The main representative for it was Woldemar Voigt, though both theory and experiment were shared between Voigt and his colleague Eduard Riecke. Besides there were Wiechert for geophysics, Schwarzschild for astronomy, Prandtl for technical physics and others. Voigt, however, was responsible for the teaching of the theoretical parts and guided the more mathematically minded physicists. Let us focus on him.

---

<sup>41</sup> This part of Hilbert's work has been widely discussed in the literature. A comprehensive recent account was given by Leo Corry: David Hilbert and the axiomatization of physics (1894-1905), *Archive for the History of Exact Sciences* 51 (1997) 83-198. Here the 1905 lectures are discussed in detail.

<sup>42</sup> Leo Corry, op. cit. p. 115, for example concluded that: "In Hilbert's view the definition of systems of abstract axioms and the type of axiomatic analysis described above was meant to be retrospectively conducted for 'concrete' *well-established and elaborated* mathematical entities."

<sup>43</sup> Minkowski, manuscript of summer 1907 lecture course "Wärmestrahlung", Cod. Ms. Hilbert 707, p. 2: "Es ist gerade meine Absicht, und auch der Herr Professor Hilbert denkt da ähnlich und hat ähnliche Bestrebungen vor, die reinen Mathematiker ... für die Anregungen zu gewinnen, die der Mathematik von Seiten der Physik zuströmen. Es ist nicht unwahrscheinlich, daß wir in den nächstjährigen Seminaren mathematisch-physikalische Theorien insbesondere gerade die Wärmestrahlung traktieren werden."

Born in 1850 at Leipzig he was formed by a trader's family, protestant sermons, Leipzig music live and especially a deep appreciation for Bach's cantatas, which he would study and perform with private ensembles for most of his life, both at Königsberg where he studied and at Göttingen where he became full professor in 1883 and never left. For all of his scientific life he had cultivated the seeds of his dissertation under Franz Neumann, that was on crystal physics.

In a constant effort he produced papers on all properties and effects that were related to crystals and condensed his intimate knowledge twice in well-structured compendia. Hence, these were some of the more outstanding trees in the physicists' garden.

It would be too simplistic if one tried to paint Voigt's physics as old-fashioned and distant from current developments, as it is simply not true. He was for example very much interested in the Zeeman effect, he followed the discussions of spectroscopy and was well-informed about the developing quantum theory. He was actually among the firsts who cited Planck's theory in a textbook in 1904.<sup>44</sup> He presented the formula and acknowledged its experimental adequacy but did not enter any discussion of its conceptual implications.<sup>45</sup>

One reason for the absence of an impact of the early ideas on quantum and atomic physics was Voigt's deep doubts about microscopic models and mechanisms. They were just *Arbeitshypothesen*, working hypotheses, dependent on everything else than reality. They reflected personal tastes and, as he put it, the "different nature in the organization of the human mind." Though he welcomed this variety which he found in the sciences as necessary as in the arts, he himself was looking for more general truths that he would find in principles of symmetry and mathematical structures that were independent of the various details of the more or less imaginative models.<sup>46</sup> For example, concerning spectroscopy he conceded that the high expectations that spectroscopic data would suggest the molecular properties did not come true. Even worse, the series laws appeared to hold not for "normal, healthy molecules but only for damaged and ill ones" hence making its study a "pathology of molecules." So it has to be asked whether at all "the light emission phenomena correspond to the material anatomy of the molecules."<sup>47</sup> As "in this respect the success of

---

<sup>44</sup> ... as a "noteworthy combination of probability considerations with the theory of the emission of waves by electric resonators." Woldemar Voigt, *Thermodynamik*, 2 vols. Leipzig 1904, p. 355. Cited after Kuhn 82, 135f.: "Most of its closing chapter deals with such standard topics as Kirchhoff's law and the displacement law; ... 'By a most noteworthy combination of probability considerations with the theory of the emission of waves by electric resonators, M. Planck arrives at a formula which satisfies experiment in the entire region that has been investigated.' Planck's law is then presented, and the experimental determination of the two constants discussed. In a thirty-two-page chapter, 'Thermodynamics of Radiation,' nothing more is said about Planck's work."

<sup>45</sup> When his disciple Drude went further in the second edition of his textbook on optics this will not have eluded Voigt, but again did not evoke second thoughts as did not Einstein's 1909 Salzburg talk that he witnessed. Cf. Paul Drude: *Optics*, Leipzig 2. ed. 1906, pp.512-519, and Russell McCormach: *Night thoughts of a classical physicist*, New York 1982, p. 197.

<sup>46</sup> Voigt, Woldemar: Über Arbeitshypothesen, *Nachr. Ges. Wiss. Göttingen*, 1905 (1905) 102-120: "Diese Verschiedenheit darf nicht Wunder nehmen; sie hängt zusammen mit der Verschiedenheit der Organisation des menschlichen Geistes, und man darf in der Wissenschaft, wie in der Kunst, im Interesse reicher und mannigfaltiger Entwicklung nur begrüßen, daß es für deren Ziele keine starre Formel giebt." S.114

<sup>47</sup> Voigt, Woldemar: *Lehrbuch der Kristallphysik (mit Ausschluß der Kristalloptik)*, Leipzig und Berlin 1910. "Aber diese Hoffnung ist durch die Entwicklung der Spektroskopie in den letzten Jahren beträchtlich getrübt worden. Nach den schönen Beobachtungen von J. Stark scheint es, daß gerade diejenigen leuchtenden Moleküle ..., welche die wichtigsten bisher bekannten Gesetzmäßigkeiten (Serien) der Spektrallinien liefern,

structural theories does not reach very far" he wrote in his 1910 *opus magnum*, "there is no cause to spend much room on structural theories in a textbook on crystal physics."<sup>48</sup> It is thus "more rational" to start from potential functions than from molecular forces.<sup>49</sup> It appears that due to these views an organization of the known facts according to a rather external categorial respect (that of mathematical transformation properties) was the most progressive thing to do.<sup>50</sup>

In Voigt's garden all plants, this is physical effects, were labeled meticulously and ordered not by their color or size but according to a certain mathematical scheme of symmetries. For example look at the headings of his 1910 *Kristallphysik*:

*Woldemar Voigt: Kristallphysik, 1910*

...

*Chapter IV:*

Interrelations between a scalar and a vector.  
(Pyroelectricity and pyromagnetism.)

*Chapter V:*

Interrelations between a scalar and a tensor triple.  
(Thermic dilatation and tensorial pyroelectricity.)

*Chapter VI:*

Interrelations between two vectors.  
(Electricity and heat conduction. Electrical and magnetical influence. Thermoelectricity.)

...

That a re-orientation of Göttingen physics research towards quantum problems would not come from Voigt is the result of a deep conviction that a "genetics" would not be able to simplify the layout of the flourishing variety but rather would lead astray: Any solved question, Voigt explained in a speech in June 1912 only gave birth to ten even more

---

nicht die normalen gesunden Moleküle, sondern beschädigte kranke sind. So wesentlich mit der Zeit auch eine 'Pathologie' der Moleküle werden wird, - bisher wissen wir von dem Aufbau und inneren Leben dieser Gebilde so verzweifelt wenig, daß wir von dem Studium ihrer 'Krankheitserscheinungen' gegenwärtig kaum Früchte erwarten dürfen. Dabei ist von der Frage, inwieweit überhaupt die Leuchterscheinungen mit dem 'materiellen Knochenbau' des Moleküles zusammenhängen und über denselben Auskunft zu geben vermögen, gänzlich abgesehn." p. 5f.

<sup>48</sup> Ibid. "Indessen sind nach dieser letzteren Richtung die Erfolge der sogenannten Strukturtheorien bisher eben nicht weitreichend. ...Wegen dieser Sachlage ist bisher keine Veranlassung, in einer Darstellung der Kristallphysik den Strukturtheorien sehr viel Raum zu gewähren." p. 111.

<sup>49</sup> Ibid. "Es erscheint daher vom physikalischen Standpunkt aus im Grunde rationeller, von Ansätzen für die molekularen Kräfte, d. h. für die Potentialfunktionen der Elementarmassen auszugehen, als von Hypothesen über deren Struktur." p. 120.

<sup>50</sup> Even when atomistic treatment and the use of the quantum hypothesis became widespread in Göttingen physics, Voigt did not change his analysis. In 1915 he still held on to his phenomenological position and gave a careful argument in its favor in the physics volume of a encyclopedia called *Kultur der Gegenwart*. Cf. Woldemar Voigt: *Phänomenologische und atomistische Betrachtungsweise*, in: Physik. Kultur der Gegenwart, Ser. 3, Vol. 3, Teil 1, pp. 714-731.

enigmatic new ones. The results of spectroscopy showed the most unexpected effects and made the constitution of the atom "nothing but more incomprehensible".<sup>51</sup>

### Introducing new Plants and Fertilizers

When Voigt made these statements Hilbert had incorporated modern physics in his curriculum already for a year, either hidden as in his summer 1911 course on mechanics that nonetheless discussed relativity as well as blackbody radiation or openly as in the following winter term in his course on the kinetic theory of gases. The latter course stands in stark contrast to Voigt's views and can be read as a criticism.

Right in the introduction he makes his standpoint clear: The phenomenological point of view, let's call it *A* must be discarded because it has no unifying power, it merely fragments the whole of physics into many single chapters with its own principles.<sup>52</sup> Hilbert argued that on the grounds of atomism and axiomatic formulation a better approach, *B*, is guaranteed for the entire physics.<sup>53</sup> This one he wants to follow in the current lectures. The best approach, however, were a theory of the molecular structure of matter, *C*, which is currently grasped and which he announced to be the topic of forthcoming teaching.<sup>54</sup>

From the introduction of the lecture notes  
Kinetische Gastheorie, winter term 1911/12

	<i>A</i>	<i>B</i>	<i>C</i>
<i>approach</i>	"phenomenological"	"on ground of atomic theory"	"a theory of the molecular structure of matter"
<i>unifying power</i>	"whole physics is fragmented in single chapters: thermodynamics, electrodynamics, optics etc. " "each field builds on specific basic assumptions"	"single point of view for all phenomena"	"goes much beyond B"

<sup>51</sup> Woldemar Voigt: Physikalische Forschung und Lehre in Deutschland während der letzten hundert Jahre (Festrede im Namen der Georg-August-Universität zur Jahresfeier der Universität am 5. Juni 1912), Göttingen 1912. "Aber jede gelöste Frage gebiert zehn neue, und die Rätsel werden immer rätselhafter. Um nur ein einziges zu nennen, so machen all' die in gehäufte Menge gewonnenen neuen Resultate, indem sie uns die unerwartetsten Wirkungen zeigen, die Konstitution des materiellen Atomes nur umso unverständlicher. Was ist das für ein Gebilde von unausdenkbarer Kleinheit, das Tausende verschiedener, dabei völlig definierter und für die Substanz charakteristischer Schwingungen auszuführen und auszusenden vermag?" p. 22.

<sup>52</sup> Hilbert, David [Hecke, Erich]: Mechanik der Kontinua aufgrund der Atomtheorie (WS 1911/12), Born papers Berlin III.2 folder 1816; the Göttingen copy in the Mathematisches Institut of these lecture notes by Hecke carries the title "Kinetische Gastheorie." "A) Man kann die Mechanik der Kontinua rein phänomenologisch behandeln. Die ganze Physik wird dabei in viele einzelne Kapitel zerlegt..."

<sup>53</sup> Ibid. "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]

<sup>54</sup> Ibid. "In 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." (Cf. "Molekulartheorie der Materie" WS 1912/13.)

<i>status</i>	"a first step in understanding" "urgently to be left behind in order to penetrate into the actual sacred objects of theoretical physics"	"attempt to find a system of axioms valid for all physics"	? [deeper foundation, axioms = reality]
<i>type of math used</i>	"partial differential equations"	mathematical method "entirely different", e.g. probability calculus, "not yet fully developed"	? [new mathematics]
<i>exponent</i>	? [Voigt]	"this lecture" winter 1911/12	"one of the next semesters" (winter 1912/13)

One can read off from this transparency a certain parallel of the reduction to few basic axioms in mathematics to the reduction to a few basic properties of atoms in physics. Hilbert's major aim appears to be a theory of molecular structure of matter, a theory that not only refers to atomistic models but allows to deduce all physical properties from something even deeper than the system of axioms for all physics as asked for in the approach *B*. What approach *C* actually should look like, is not completely clear; I have filled in some guesses.<sup>55</sup>

From this, I think, it has already become clear, that Hilbert's understanding of axiomatics has changed considerably since 1905. It appears that at the same time when Hilbert's interest in physics changed its quality around 1911, a reinterpretation of the axiomatic method occurred.

Roughly speaking, Hilbert turns from applying axiomatization of *well-established* physical theories like mechanics, thermodynamics etc. to *topical* research questions. Instead of only laying foundations deeper the axiomatic method becomes a genuine methodology to always and from the beginning on taking care about what is considered as the basic truths and simultaneously checking for consistency. Becoming increasingly aware of the fact, that the modern physical theories evolved by the attempt to add new hypotheses to the established corpus of knowledge, like in the case of the quantum hypothesis or the principle of entropy growth, the consistency question became imperative. Only clarity about the independence and compatibility of the basic assumptions appeared as a successful guide line for further research. In addition, it would be wrong in the case of empiric sciences to wait for maturity before the axiomatic treatment should be attempted; in his private notebooks for example we find the following entry written at some point between 1905 and 1911

<sup>55</sup> Hence the three slot open are the following: There is no example given for the phenomenological approach *A*, which is, of course, Voigt's. 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 "on ground of atomic theory" (*B*) the objective is clearly to give a relation of the basic notions employed in the axiomatization to the actual physics objects (*C*). As partial differential equations are the toolkit for phenomenologists, mathematics like probability calculus is in the respective box of convinced atomists. Thus in the realm *C*, aimed at by people like Hilbert who believed in a pre-established harmony in nature, there was the hope that advanced or novel mathematics would reveal the identity of axiomatic model and physical reality.

I protest against the objection that physics were not developed enough to be axiomatized. Any science is at any time not only ripe enough but requires with necessity axiomatization, understood in the correct sense.<sup>56 57</sup>

To summarize, Hilbert was convinced at that time that by posing the problems correctly in mathematical terms their solution would be guaranteed. Reducing physics, even chemistry and biology to mathematics would eventually lead to a kind of unified science. Needless to say that this was far too optimistic and overlooking all specific complexities of the various fields; this view, however, was the driving force for Hilbert to heavily invest into physics and finally contribute to a marked change of the conditions under which physics after the war could flourish.

Before we begin to describe some of the plants and fertilizers Hilbert and the mathematicians deployed in the physicists' garden in some detail, let me briefly remind you of some context of his personality.

From a family of protestant merchants and judges Hilbert, born in 1862 twelve years after Voigt, was educated to the humanistic standards of his time at the best Königsberg gymnasium, which was neither a happy nor particularly formative influence. He studied mathematics to get rid of memorizing facts, he left the church and had no relation to religious art or music, but was quite interested in popular culture. He disregarded bourgeois conventions and changed his fields of interest quite a number of times.<sup>58</sup>

When Voigt in 1912 had bought a church organ for his gothic style hall at home Hilbert's physics assistant Alfred Landé distasted a special part of his duties: to play the latest music hall hits ("Schlager") on Hilbert's phonograph using the biggest needle, of course, in order to produce the biggest volume.

The biggest scientific volume, Hilbert tried to generate by inviting leading physicists and mathematicians interested in physics. Having Poincaré and Lorentz for special one-week lecture series at Göttingen in 1908 and 1909, in 1913 a Congress was organized with Planck, Nernst, Lorentz, von Smoluchowsky, Sommerfeld and also Debye as speakers.

With such events initiated and realized largely by Hilbert, he undoubtedly influenced if not defined the main physics discussion in Göttingen. Most interestingly, and here the term economic history of science becomes literal, it was money dedicated to a specific mathematical field that Hilbert diverted to physical fields, that in principle were possessions in the physicist's garden. The money was from the Wolfskehl fund, a prize for the solution of Fermat's theorem.

<sup>56</sup> Cod. Ms. Hilbert 600:3, Notebook III, p. 105.

<sup>57</sup> There are many statements of later time one could give. They eventually lead to something one could characterize best as disciplinary imperialism, for example when in his famous 1917 Zurich talk on now "axiomatic thinking" he says: "Ich glaube: Alles was Gegenstand des wissenschaftlichen Denkens überhaupt sein kann, verfällt, sobald es zur Bildung einer Theorie reif ist, der axiomatischen Methode und damit mittelbar der Mathematik. Durch Vordringen zu immer tieferliegenden Schichten von Axiomen im vorhin dargelegten Sinne gewinnen wir auch in das Wesen des wissenschaftlichen Denkens selbst immer tiefere Einblicke und werden uns der Einheit unseres Wissens immer mehr bewußt. In dem Zeichen der axiomatischen Methode erscheint die Mathematik berufen zu einer führenden Rolle in der Wissenschaft." Hilbert, David: Axiomatisches Denken, Zürich 1917, p. 156.

<sup>58</sup> He behaved unconventionally, which was often said to be the drawback of his genius or showing a certain unworldliness. It, however, makes much more sense – to me at least – if interpreted as demonstration of power: he made clear that not he, Hilbert, had to keep to the rules of *Kaiserreich* academic conduct. Unlike Voigt, Hilbert did change his fields of interest quite a number of times: theory of invariant, number theory, foundations of geometry, integral equations, physics etc. and he managed to be the avant-garde that many followed.

There are additional indicators that show how mathematicians became interested in and supported physics research: one could look also at the talks in the *Mathematische Gesellschaft* for the percentage of physics topics, at the salary and rank of Hilbert's mathematics and physics assistants among others.

Let me now come to an often overlooked continuity of Göttingen physics through WW I: the appointment of Peter Debye. Comparing his activities to Hilbert's interests around 1913 – for example his presentation of a paper on the Nernst theorem to the physicists in January or his teaching topics – the parallels strike the eye: Hilbert must have been surprised that Debye has anticipated many of his interests. It must have been for this reason that Debye was actually invited to the Wolfskehl Congress as a replacement of Einstein who was deeply absorbed in a theory of gravitation.<sup>59</sup> After his presentation at the Congress it was clear for Hilbert that he should get hold of him. Plans for guest professorship previously thought for Lorentz, Rutherford and Hadamard were changed to have Debye in Göttingen in the summer term 1914.

As argued in a letter to the ministry Debye was – with Einstein and Laue unavailable – the next "outstanding representative" of the "new direction" in theoretical physics.<sup>60</sup> His research record clearly made him a quantum physicist following the traces of, and at various points also superseding Einstein, whose Zurich successor he was.

In his talk at the congress, Debye assumed that the atomistic picture of solids "agrees in all essential points with reality" and that one should test this physical theory experimentally.<sup>61</sup>

For Hilbert it must have been the best chance to plant the fallow land of quantum physics with the energetic Debye who promised both to bring the quantum into the whole of physics theoretically as well as experimentally and to allow the mathematics influence to stay.

---

<sup>59</sup> Sommerfeld to Hilbert, Nov. 1, 1912. "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..."] Cited after Einstein CP V, p. 506.

<sup>60</sup> Dekan Körte to Minister, May 28, 1914, SPK Rep. 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"

<sup>61</sup> Debye, Peter: Zustandsgleichung und Quantenhypothese mit einem Anhang über Wärmeleitung, in: Vorträge über die kinetische Theorie der Materie und der Elektrizität, Max Planck Hg., Leipzig 1914. "Wir werden im folgenden versuchen, wenigstens andeutungsweise, in das Wesen dieser Gesetze einzufingen 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.

His paper on partition function and quantum hypothesis thus combined quite attractively atomistic reasoning and quantum theory with well-established rather classical fields of research as thermodynamics and heat conduction, making him not only the single available quantum physicist for Göttingen but also the ideal mediator between Riecke and Voigt on the one side and the mathematically trained researchers invading physical homeland like Hilbert, Born, Courant and others on the other. To sum up, we can safely assert that Debye (like Born) was an independent quantum physicist, not put on track by his master but responding to the indisputable failures of classical theory.

To hire Debye, however, was not simple, as he had realized his value and gained a certain self-confidence during his term as *Gastprofessor*. When it was rumored that Zurich would offer a salary of 20.000 Marks Göttingen took pains over a lucrative salary, however, still far away from meeting this height. The regular amount had to be supplemented by a contribution from the Wolfskehl fund and from an unidentified private side, which somehow ironically turned out to be no other than Woldemar Voigt. In August 1914 Debye signed an agreement to take over a newly created professorship, a most unusual event.<sup>62</sup>

## Harvest

Let me come to the last section of my talk. What were the effects of the diversion of resources from the mathematicians fields to the physicists'? Did the investments pay back? How differed short-term result from long-term effects?

The first result was the establishing of quantum theory courses in the physics curriculum provided by Debye. The famous joint seminar with Hilbert "On the Structure of Matter," in addition, became the most influential discussion forum for staff, *Privatdozenten*, and advanced students.

The boldness of the Hilbert view on the accessibility of the molecular realm found its manifestation in a research project Debye worked on together with Paul Scherrer. What is now known as the Debye-Scherrer method of X-ray crystal analysis was the attempt to actually measure the size and arrangement of the electron orbits in the Bohr atom.<sup>63</sup>

For some time at least it appeared to Hilbert that his dreams came true: He wrote to *Geheimrat* Elster, the person in charge in the Prussian Ministry of Culture for the appointment of Debye, in January 1915 that, during the great revolutionary events in the world outside, within the Göttingen mathematical and scientific community a most important development happened. Through Debye's coming the discussions in the mathematical seminar, that were attended by almost all mathematical and physical lecturers, had turned to become "scientific feats". As the draft of this letter reveals, Hilbert first was tempted to write that Debye were "the Newton of chemistry" and than changed it to read, that he "appears as the Newton of molecular physics." Through him the much longed for "mathematical chemistry" had arrived. He concluded that Debye had become a "true replacement" for Minkowski both in personal and in scientific respect.

---

<sup>62</sup> There are more instances of mathematicians effort and money shaping Göttingen physics: For example, when in 1915 Riecke died and a successor was needed, Wolfskehl money again helped to pay for job talks and Hilbert was much involved to get, though without success, Wilhelm Wien or Friedrich Paschen hired, both clear exponents of experimental research on quantum physics.

<sup>63</sup> The main idea was that even when the atoms are in random order there should remain a detectable signal from the atomic constitution. This idea was right but not for the conjectured Bohr atoms but for the tiny crystal pieces used. Concerning this powerful method, in a typical opportunist move, Debye and Scherrer quickly redefined their objective and developed the method for its own sake. This rather marginal anecdote sheds light on an important general point which is of high significance from my point of view: The initial objectives, visions, and driving forces do in most cases not coincide with the scientific results that were then hailed as progress. Cf. Debye, Peter and Scherrer, Paul: Interferenzen an regellos orientierten Teilchen im Röntgenlicht I., *Physikalische Zeitschrift ; Nachr. Akad. Wiss. Gött. math.-nat. Kl. IIa* 1916, 1-26, 17 (1916) 277. 4.12.1915.



Debye hat die Arbeit über die Konstitution des Wasserstoffmoleküls und die Bestimmung der rotatorischen Wärmekapazität  
 gemacht und in der letzten Form gedruckt. Jedoch hat er eine neue  
 math. Arbeit <sup>publiziert</sup> so daß er nicht mehr in Verbindung  
 mit Hilbert sein könnte. Er hat für Hilbert gearbeitet.

Debye's paper on the constitution of the hydrogen molecule finished in the first days of 1915 was the desired attempt to explain molecular constitution from simple principles like Bohr's atom – called the planetary system hypothesis – and deriving by nice calculations properties that met the experimental findings. The success was for the author, as he wrote in the conclusion, "indisputable." Later Sommerfeld would wonder how optimistic many scientists about Debye's concepts in the first years of the war were. Despite all unjustified optimism, Debye had a considerable impact of making the research on atoms and quanta central in Göttingen physics. Naturally Hilbert used his two possibilities to propose candidates for the physics Nobel price in 1915 and 1916 by proposing no different candidate than Debye.

With Debye and the research on rather concrete and thus complex problems Hilbert realized that he would not be able to move on on the quantum path. It is in particular at this point that Hilbert turned to general relativity completely, where again he contributed at a crucial point. The difference to his quantum investments was, however, that it was done with publications, so to speak publicly in contrast to his mostly local teaching activities and research politics in the quantum case.

The long-term harvest for Göttingen physics, however, was clearly the transplantation of a considerable amount of mathematical method, attitude and culture into physics thus defining a specific Göttingen flavor of physical theory. The Structure of Matter seminar begun by Hilbert in 1914 continued through the times of the creation of quantum mechanics and served as an important trading center of ideas and result of research in quantum and atomic physics.

Like Debye, Max Born was during the war actively working on quantum problems together with Alfred Landé, one of Hilbert's physics assistants. Formally in war duty at Berlin, he was as Debye trying to see what results would come from Bohr's theory if taken seriously. In distinction to Debye he found a negative result: the elasticity properties come out completely wrong if one tries to build up crystals from planetary atoms. The positive message was, that the electron must be distributed in the atom somehow three-dimensionally.

In this light Born was a fitting replacement for Debye especially as he managed to get a position for his student time friend and experimental alter ego James Franck.

Born's research program having its root in his wartime research was to press the old theory to the limits and try to see where things go wrong. This way already years before the actual matrix mechanics was found, there was talk about and even a certain understanding, if only consisting of discarding wrong views, of quantum mechanics. The term appeared already in Hilbert's course *Über die Einheit der Naturerkenntnis* "On the unity of

scientific knowledge" in winter 1923/24, and served as title of a publication of Born in 1924.

After 1921, when Born, Franck, and Pohl were taking care for the physics fields, the fence between their garden and that of the mathematicians was in good shape again. In particular the overwhelming teaching load due to the dramatically increased student numbers reduced time for interdisciplinary exchange. Now it was the accumulated method, approach, and attitude of a mathematician turned physicist under Hilbert like Born, that remained effective even when the master was again busy with problems in his own garden: While the tree-tops developed fine and reached to the neighbors, the roots, the axiomatic foundations of logic were in bad shape, a problem even Hilbert would not solve easily.