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Experimenting theory: The proofs of Kirchhoff's radiation law before and after Planck

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David Hilbert told the German physicists at one of their main professional meetings in the morning of September 18, 1912 that they had failed for more than fifty years to provide a proof for one of their most precious laws: Kirchhoff's law on heat radiation that turned involved experimental results into a relation as simple and persuasive like Ohm's law 33 years before, had not even in the simplest special cases been made plausible. The physicists remained surprisingly unstirred and one of their main spokesmen, the chairman Arnold Sommerfeld, applauded in saying that everybody had gotten the impression that in Hilbert's approach everything would fit together very beautifully. Only Planck's theory of quanta his approach could not produce, Sommerfeld added, and so the physicists were ensured that at least the new field of quantum theory they were still in command of.[1]

In the fifty years after Gustav Kirchhoff's presentation of this law in 1859 such authorities like Kirchhoff himself, Hermann von Helmholtz and Max Planck have given a number of different proofs. But, were they really all wrong as Hilbert implied? Or, did rather the validity of the derivations and in particular the presupposed concepts expire as new understanding of the phenomena developed? Did, possibly, the standards of proof change this much that one had to disqualify earlier attempts? Or, was it only a matter of gaps within the arguments that were always only much later identified? Naturally, we may expect that a mixture of various such factors must have been at work, including that also Hilbert may have overstated the situation. The analysis of these questions will lead us

[*] This paper has grown out of a presentation to the Quantum Theory Centenary at Berlin in December 2000 and has benefited from comments by Olivier Darrigol, Klaus Hentschel, Dieter Hoffmann and Erhard Scholz. I would like to thank Michael Eckert and Ulrich Majer for reading of earlier drafts of the paper and for stimulating discussions.

[1] David Hilbert: Begründung der elementaren Strahlungstheorie, in: Nachrichten von der königlichen Gesellschaft der Wissenschaften zu Göttingen 1912 (math.-phys. Klasse) 773-789, and with a summary of the discussion after the talk in: Physikalische Zeitschrift, 13 (1912) 1056-1064. The original reading manuscript is Cod. Ms. D. Hilbert 586 at the manuscript department of the Niedersächsische Staats- und Universitätsbibliothek Göttingen. Quotations are from the manuscript, p. 5, and Physikalische Zeitschrift, p. 1064.

to the identification of different styles of reasoning employed in the proofs of the law that are in particular distinguished by their relation to experiment.

From a rigorous mathematical point of view many derivations and proofs of 19th century physics may appear "experimental," i.e. as if the physicists were experimenting with different and often incomplete sets of hypotheses and threads of different types of argument. (This criticism of the physicists' lack of logic and rigor has been raised repeatedly since then.[2]) Independently of this point, however, there is a much more direct role in which experimental approaches entered the theoretical proofs and which we take to justify the term *experimenting theory*: It is the role of experimental set-ups, the framing of the line of argument in experimental terms, the step-by-step procedures of varied situations, and particularly the postulating of idealized objects being used in thought experiments. This paper tries to give an example of experimenting theory from the field of classical radiation theory. At the same time it may contribute to open up a certain new vista of the interplay of theory, experiment, and the use of instruments or tools (both material and conceptual) that has attracted much attention in recent years; a new vista of the experimental remains in theoretical reasoning, or short, the experiment-ladenness of theory.[3]

CLASSICAL RADIATION THEORY AND THE RISE OF MODERN PHYSICS

That Kirchhoff's law was a necessary prerequisite for Max Planck's finding of the proper radiation formula is widely accepted although not much attention was paid to the relation of their respective histories in particular of justification. Simply speaking, it is the relation between the proof of the existence of a solution and the specification of the correct formula. This paper will focus on the specific question of the ways of founding

[2] Cp. for a recent manifestation e. g. Arthur Jaffe, Frank M. Quinn: "Theoretical mathematics": Towards a cultural synthesis of mathematics and theoretical physics, Bulletin (New Series) Am. Math. Soc., 29 (1993) 1-13. The term "experimental mathematics" was used here to describe the physicists' way to deal with mathematical problems in a non-rigorous way.

[3] Cp. John Ackermann: Review article: The new experimentalism, British Journal of Philosophy of Science, 40 (1989) 185-190, and references therein. See also the two proceedings volumes: Michael Heidelberger, Friedrich Steinle: Experimental essays - Versuche zum Experiment, Baden-Baden 1998, and Christoph Meinel: Instrument - Experiment: Historische Studien, Bassum 1999, with Klaus Hentschel's review article: Historiographische Anmerkungen zum Verhältnis von Experiment, Instrumentation und Theorie, pp. 13-51.

Kirchhoff's law and is hence rather a history of justifications than a history of discoveries.[4] Looking at various proofs of a general physical law over a period of more than 50 years, a period in which theoretical physics in Germany developed into a new discipline, the problem of a history of proof is addressed and it is asked what different ways of reasoning can be found and whether these can be distinguished as styles of reasoning or thinking.[5]

The case chosen is Kirchhoff's formulation of a general law that relates the emission of radiant bodies to their absorptive properties. It stands at an intersection of a number of developments and fields of physics in the 19th century.

First, there is the development of spectroscopy and the beginning of astrophysics: Fraunhofer's spectral lines and his observation that specific absorption lines of the solar spectrum coincide with the main emission lines of sodium brought into a flame made the beginning of many considerations of relations between absorption and emission of luminous bodies early in the 19th century. Kirchhoff, whose characteristic research style was rather to complete a line of research than to begin a new one (according to Woldemar Voigt), was thus on the on hand trying to complete the "drawing" of the spectrum of the sun, on the other hand he was, however, also trying to condense all the qualitative knowledge on the coincidence of emission and absorption spectra into a quantitative law that would govern spectral analysis.[6] Making explicit and promoting the insight that from comparing the spectra of the sun and the chemical elements, information on the physical composition of the stars can be inferred, he together with Bunsen became the fathers of spectral analysis. While the qualitative rule that a body can emit all those wavelengths that it is able to absorb applied generally to all phenomena of radiating substances, the exact law, however, Kirchhoff arrived at in December 1859 did no longer apply to systems like salt in a flame but required clear situations of equilibrium in order to apply the mechanical theory of heat with Carnot's theorem, i.e. thermodynamics and its second law. Much confusion about the validity and

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4 For the latter cf. Edmund T. Whittaker: A history of the theories of aether and electricity. Vol. 1: The classical theories , New York 1951, ch. XII; Hans Kangro: Vorgeschichte des Planckschen Strahlungsgesetzes, Wiesbaden 1970; by the same author: Kirchhoff und die spektralanalytische Forschung, in: Gustav Kirchhoff: Untersuchungen über das Sonnenspektrum und das Spektrum der chemischen Elemente und weitere ergänzende Arbeiten aus den Jahren 1859-1862, Osnabrück 1972, pp. 1-54; Hans-Georg Schöpf: Von Kirchhoff bis Planck. Theorie der Wärmestrahlung in historisch-kritischer Darstellung, Berlin 1978; Eisui Uematsu: The role of Kirchhoff's 1859 work in the history of radiation theory [in Japanese], Kagakusi Kenkyu: Journal of History of Science, Japan, 25 (1986) 14-19.

5 Throughout this paper no specific approach towards styles of scientific thinking or styles of scientific reasoning will be presupposed, though the views taken here may appear near to Ian Hacking's notion of "styles of reasoning" as different from Alistair Crombie's "styles of thinking." Cf. for a survey the essay review of Marga Vicedo: Scientific styles. Towards some common ground in the history, philosophy, and sociology of science, Perspectives on Science, 3 (1995) 231-254. We will discuss the issue of style at the of this paper in some more detail.

6 Christa Jungnickel et. al.: Intellectual mastery of nature. Theoretical Physics from Ohm to Einstein. Vol. 1: The Torch of Mathematics, 1800-1870, Chicago 1986, p. 299 and quote of Woldemar Voigt on p. 301.

applicability of Kirchhoff's exact law developed in the following decades. Specialist of the field like Aimé Cotton and Heinrich Kayser wrote extensive reviews on the status of Kirchhoff's law around the turn of the century either to determine the field of application of the law or to review the theoretical implications.[7]

Second, as we have seen in part from the above, there is a line from observations to a general law that corresponds to the work of researchers of different fields and abilities: It started with instrument makers like Joseph von Fraunhofer, that make the triggering observations and proceeded with experimentalists like William Ritchie, Frederic de la Provostaye and Paul Desains, that have accumulated hosts of measurements and tried, without too much success, to formulate general results.[8] Using this, theoretically inclined physicists like Kirchhoff and later Helmholtz, succeeded to make the appropriate abstractions and arrived at general mathematical descriptions and at least claimed to be able to derive the laws from theoretical principles. This line we will follow further in this paper up to the point were pure mathematicians enter the discussion with rather surprising claims as we saw in the beginning. The way to Kirchhoff's law and the changing way of providing a theoretical foundation for it is hence a discussion that takes place in a process of a disciplinary formation and transformation that finally leads to the emergence of a theoretical physics for which Max Planck generally serves as the model scientist.

Third, the history of Kirchhoff's law is the prehistory of quantum theory as it emerged from Planck's determination of Kirchhoff's universal radiation function. With equal right is also the history of the instrumentation used to confirm Kirchhoff's law and in particular his introduction and proposals for construction of black-bodies was the material basis of the experimental history of early quantum theory. As we are here primarily concerned with theoretical justifications, this side of the history of radiation and quantum theory is only touched occasionally.[9]

7 Aimé Cotton: The present status of Kirchhoff's law, Astrophysical Journal, 9 (1899) 237-268, here p. 267 (Translated from Revue Générale des Sciences, February 15, 1899.) Aimé Auguste Cotton (1869-1951) completed his thesis on "Recherches sur l'absoption et la dispersion de la lumiére par les milieux doués du pouvoir rotaire" in 1896 at Ecole Normale in Paris. Heinrich Kayser: Handbuch der Spectroscopie, Vol. II, Leipzig 1902.

8 Wiliam Ritchie: Experimental illustration of the equality between the radiating and absorbing powers of the same surface, Journal of the Royal Institution 2 (1831) 305-307. (The radiating or emissive power was defined with respect to soot of the same temperature and hence leads to an equality in distinction to Kirchhoff's different definition: $E/e=A$.) For the publications of Frederic de la Provostaye and Paul Desains see Kayser, *Handbuch* (ref. 7) p. 28 f.

9 Cp. Dieter Hoffmann: Schwarze Körper im Labor. Experimentelle Vorleistungen für Plancks Quantenhypothese, Physikalische Blätter, 56 (2000) 43-47, and by the same author: On the experimental context of Planck's foundation of quantum theory, in: Revisiting the quantum discontinuity, preprint 150 of the Berlin Max Planck Institute (2000) pp. 45-68. Already Kirchhoff's turn to spectral research finds its simple explanation in the fact that although his interest in this matter had been since long, only in 1857 he was able to get one of the few existent flint glass prisms manufactured by Fraunhofer. Cf. Ludwig Boltzmann: Gustav

Before we turn to the relation of the history of Kirchhoff's law and its relation to the history of Planck's quantum theory in some detail, few remarks on the historiography of the former are expedient.

Kirchhoff himself felt obliged to write a history of his researches as early as 1862, just after his definite publication on the proof of his law.[10] The reason was that since he had used much research of others, priority claims came promptly. In particular an exchange with the British physicist Balfour Stewart originated who had given a version of Kirchhoff's law already a year before the first paper of the Heidelberg professor. This controversy then grew to a major one of national import.[11] Stewart will, however, play no role in the following, as his formulation of the problem does not lead to a universal function.[12]

Cotton's and in particular Kayser's review from around the turn of the century are very much historical in style and show the moderate French and German views on the by then cooled down controversy. Both retell mainly the arguments that have been put forward to justify Kirchhoff's law and to determine its field of application and abstained from formulating a true theory of the results established that had to refer to a mechanism of emission and absorption of radiation. In later works on radiation theory and Kirchhoff's law a historical perspective disappeared; Planck, for instance, does not comment on this any more in his lectures on heat radiation published in 1906.

In the historical research on early quantum theory the role of Kirchhoff's law is deliberately left out of consideration and the status of the law at the time of Planck's use is not discussed.[13] In biographical studies, the point is occasionally raised in what sense Kirchhoff's work was crucial to the development of quantum theory. Either was Kirchhoff's law seen as "the key to the whole thermodynamics of radiation... the key to the new world

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Robert Kirchhoff. Festrede zur Feier des 301. Gründungstages der Karl-Franzens-Universität zu Graz, Leipzig 1888 (reprinted in Ludwig Boltzmann: Populäre Schriften, Leipzig 1905, 53-75.), p. 8.

10 Gustav Kirchhoff: Zur Geschichte der Spektral-Analyse und der Analyse der Sonnenatmosphäre, Annalen der Physik 194 (1862) 94-111.

11 Stewart replied promptly on Kirchhoff's *Geschichte*. Cp. the definite study on this controversy by Daniel M. Siegel: Balfour Stewart and Gustav Robert Kirchhoff: Two independent approaches to "Kirchhoff's radiation law", ISIS, 67 (1976) 565-600. Even in the historical research of the 1970s a certain national difference in the appreciation of the respective achievements remained, cp. Siegel's study to Kangro, *Kirchhoff* (ref. 4) pp. 24-26.

12 As Siegel concludes (ref. 11) p. 584: "Stewart's approach here in contradistinction to Kirchhoff's, focused upon the conditions in the interior of a single material, rather than on the equilibrium between two materials. Stewart's conclusion was correspondingly restricted and did not embrace the sort of connection between the emissive and absorptive powers of different materials, through a universal function of wavelength and temperature, which Kirchhoff established."

13 Hans Kangro: Vorgeschichte des Planckschen Strahlungsgesetzes, Wiesbaden 1970, p. 3. Cp. also Armin Hermann: Frühgeschichte der Quantentheorie (1899-1913), Moosbach/Baden 1969 (engl. translation 1971).

of the quanta,"[14] or its methodologically approach, his "clever way" of deriving his law, was identified as the "methodological example for Planck's and Einstein's investigations."[15]

Two further authors have tried to draw a line of development from Kirchhoff to Planck: Hans-Georg Schöpf did this by translating Kirchhoff's argument of his 1860 publication in modern physical terminology but followed most of the crucial assumptions without commenting on the criticism raised by a number of physicist after Kirchhoff.[16] Joseph Agassi's "Radiation Theory and the Quantum Revolution" is a rather unique discussion of Kirchhoff's law and its proof.[17] It is repeatedly emphasized that Kirchhoff's law were false and his proofs wrong, without, however, going into specific detail on the erroneous assumptions. Instead of a historical analysis of the claimed deficiency of Kirchhoff's proof the author tried to settle the issue himself on two pages, he did, however, not address that as a consequence of the falseness of the law in the end also Planck's theory would be at stake.[18] Arguing from the results of modern quantum theory the historians duty to elucidate the historical framework and rationality, in which scientific theories developed is de-emphasized in favor of certain philosophical positions.[19]

In order to make clear in what precise sense Kirchhoff's law is a prerequisite for quantum theory, let us first recall briefly Kirchhoff's original line of thought and spell out his law. As thermal equilibrium of any radiating body requires that the emitted energy must equal the absorbed one, for suitably defined notions of emissive and absorptive power a relation must be fulfilled. For Kirchhoff it appeared in 1860 established since long that this means the ratio of emissive power to absorptive power must be the same for all bodies. That this relation applies to radiation of each wavelength separately was for him the

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14 Leon Rosenfeld: Gustav Robert Kirchhoff, in: Dictionary of scientific biography, edited by Charles Coulston Gillispie, vol. VII, New York 1973, pp. 379-383, here p. 382.

15 Klaus Hentschel: Gustav Robert Kirchhoff (1824-1887) und Robert Wilhelm Bunsen, in: Die großen Physiker. Bd. 1: Von Aristoteles bis Kelvin, edited by Karl von Meyenn, München 1997, pp. 416-430 and 532-534, here p. 430.

16 Hans-Georg Schöpf: Von Kirchhoff bis Planck. Theorie der Wärmestrahlung in historisch-kritischer Darstellung, Berlin 1978. Kirchhoff's 1860 publication is discussed in chapter 1 of the introduction, pp. 11-29, the paper is reprinted on pp. 131-151. The crucial physical argumentation in Kirchhoff's § 3 is not discussed in its original form, cp. p. 22.

17 Joseph Agassi: Radiation theory and the quantum revolution, Basel 1993, in particular chapter 5 and appendix A, which is a reprint of the authors earlier publication: The Kirchhoff-Planck radiation law, Science, 156 (1967) 30-37.

18 He, however, did his simplified derivation in a rather questionable manner, as e. g. the restriction to one wavelength is not conclusive, cp. pp. 82 f.

19 Agassi's rather eclectical and personal style and his often wholesale criticism on historians of science gave rise to harsh reviews. Cf. e. g. Helge Kragh, Centaurus, 37 (1994) 91-92 and Agassi's letter to the editor, in the same volume, pp. 349-352 with Kragh's reply, pp. 352-354

new law to be established.[20] Clearly, the consideration of single wavelengths came from his researches of emission lines of colored flames and absorption lines in the solar spectrum as spelled out in his 1859 paper.[21] His conclusion "that for rays of the same wavelength at the same temperature the ratio of emissive power and absorptive power is the same for all bodies," which is one way to formulate his law, was already in his first publication reformulated into the formalized statement that the considered ratio of emissive and absorptive powers "is a function of the wavelength and the temperature."[22] In modern notation:

$$\frac{e}{a} = f(T, l)$$

This function *f* describes in particular the unique emission spectrum for all bodies that are black, i.e. that absorb all incident radiation ($a = 1$). The search of the correct formula for the black-body radiation was the search for the function of Kirchhoff's law. In this sense Kirchhoff's law was a logical prerequisite for Planck's radiation formula. When this law could be considered as theoretically and experimentally established, Planck naturally will have assumed it without further discussion. One might think in particular that Kirchhoff's arguments in the proof of the existence of this universal function will have served as starting point for the determination of it, but this was not the case. As Kirchhoff's considerations clearly do not anticipate any aspect of quantum theory, the classical problem of the determination of the universal function was still a problem that gave rise for the quantum and should hence be understood as a *proto quantum problem* like the problems of specific heat etc.[23]

The birth of quantum theory came with Planck's formula and in particular with Einstein's and Ehrenfest's interpretation of the role of the energy elements and the establishment of a quantum discontinuity. Clearly, it was not a matter simply of accurate description of experimental results.[24] Hence, concerning the relation between theoretical proof and experimental confirmation we find that it was rather a matter of presenting and interpreting a derivation or proof for the radiation law that provided a surplus of understanding besides the reproduction of empiric data. This is obvious from the fact, that in the first years after Planck's derivation textbooks would still write that for example the Lummer and Jahnke formula would account equally well for the measurements, only it

[20] Gustav Kirchhoff: Ueber das Verhältnis zwischen dem Emissionsvermögen und dem Absorptionsvermögen der Körper für Wärme und Licht, Annalen der Physik, 109 (1860) 275-301, on p. 276.

[21] Gustav Kirchhoff: Ueber den Zusammenhang von Emission und Absorption von Licht und Wärme, Monatsberichte der Akademie der Wissenschaften zu Berlin, sessions of Dec. 1859 (1860) 783-787, on p. 783. Reprinted in Kirchhoff / Kangro *Untersuchungen* (ref. 4) with original pagination.

[22] Kirchhoff, *Zusammenhang* (ref. 21), pp. 784 and 786.

[23] I owe this term to discussions in a working group on the history of quantum physics at the Berlin Max Planck Institute led by Jürgen Renn.

[24] Cp. also Thomas S. Kuhn: Black-body theory and the quantum discontinuity, New York 1978.

contained empirical coefficients in a more arbitrary way.[25] And when Max Born first learned about Planck's formula in a lecture on astrophysics by Karl Schwarzschild in 1905, he was told that Wien's formula would fit a bit more accurately than Planck's.[26] The same situation applies for Kirchhoff's law. The results that measurements to high accuracy suggested the independence of the energy distribution of radiation from the material properties of the experimentally approximated black-bodies in the laboratory could in no way dispense with a proof. And hence we find the peculiar situation that we do not have a valid proof of Kirchhoff's law, which we identified as an essential prerequisite for Planck's theory at the time Planck set up his formula. Interestingly enough (or perhaps rather as a consequence of this situation), the ways to derive the existence of the universal radiation function and the ways to derive its actual expression were rather unrelated. When Planck wrote his book on the theory of heat radiation in 1906 he gave an entirely new proof of Kirchhoff's law that had nothing in common with Kirchhoff's argumentation. We shall hence try in the following to re-contextualize Planck's work within the then ongoing debate on the proof of Kirchhoff's law. We restrict ourselves to the German line of research in which Planck was situated: his teachers, Kirchhoff himself and Helmholtz, his colleague Ernst Pringsheim, (who together with Otto Lummer provided the data that led Planck to the right interpolation formula), and an eminent contrasting figure, the mathematician David Hilbert.

In doing this a threefold aim is pursued. First, we try to tell that part of the history of Kirchhoff's law, which is of interest for the later, views on radiation and in particular Planck's work. This includes the evolution of what was understood to be the content of the law as well as what was assumed to be its foundational roots. Second, Planck's long search for the correct justification of his radiation formula will be placed in the context of the still ongoing debate on the derivation of Kirchhoff's law. The variety of approaches, arguments, and ontological claims that can be found in radiation theory determine to great extent available conceptual frames for Planck's researches. And, third, the different styles of thinking and reasoning applied in proving a physical law are exemplified, ranging from procedures that are closely abstracted from experimental action like those found with Kirchhoff or Helmholtz up to a purely mathematical style of Hilbert. As a result we will locate the rather special approach of Planck as one lying in some particularly powerful middle ground and elucidate the great difficulties the establishment of a truly mathematical physics met.

[25] Orest D. Chwolson: Lehrbuch der Physik. Zweiter Band: Lehre vom Schall (Akustik) - Lehre von der strahlenden Energie, Braunschweig 1904, p. 230

[26] Karl Schwarzschild: Astrophysik, lecture notes by Max Born for the winter term 1904/05, Cod. Ms. K. Schwarzschild 13, item 1, pp. 115-119.

ON EXPERIMENTAL THINKING I: THE PROOFS OF KIRCHHOFF'S LAW BEFORE PLANCK'S FORMULA

Kirchhoff's first paper that communicated the new law had the title "On the relation between emission and absorption of light and heat" and was presented to the Berlin Academy of Sciences on December 15, 1859.[27] Originating from his observations concerning the relation between Fraunhofer's lines and the solar spectrum, that he had presented seven weeks earlier to the same audience,[28] he now reported that he had arrived at a general law "by a very simple theoretical consideration." The simple proof he presented employed one specific ingredient, which was clearly an idealization of the sodium colored flames he used in his experimental investigations:

> ... but it appears as safe to conceive as *possible* a body that emits of all heat radiations, the luminous as well as the dark ones, only rays of *one* wavelength and only absorbs rays of *the same* wavelength.[29]

This is without further elaboration nothing than a hypothesis. For the proof he asked his readers to consider two facing infinitely extended plates, one of them with the specified properties. The analysis of light of this wavelength being exchanged to-and-fro the plates, getting partly absorbed, partly reflected, showed that Kirchhoff's law must hold no matter what properties the second plate had as long as thermal equilibrium was maintained. The argument thus started with conceiving an experimental set-up and performing a though experiment with rays of a certain wavelength. Keeping track of the absorbed and reflected energies, a short calculation yielded the desired relation. Finally, by asking his readers to imagine the second plate replaced by a different one of the same temperature, he concluded that the relation must be the same for all bodies. He adds that in consequence the way he had proposed for the chemical analysis of the solar atmosphere had thus gotten its theoretical foundation.[30] While the assumption of infinitely extended plates is a mathematical simplification in the argument, the one-wavelength plate, which appears as a physical idealization, is only postulated as long as its existence is not further justified.

Only few weeks later Kirchhoff apparently changed his view that the general proof would be feasible by such simple theoretical considerations and submitted a second much more involved proof in January 1860 without commenting on the fate of the first one at this time. Two years later Kirchhoff published a better-structured second version of this derivation which appeared as appendix of the second edition of his "Investigations on

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[27] Kirchhoff, Zusammenhang (ref. 21).

[28] Gustav Kirchhoff: Ueber die Fraunhoferschen Linien, Monatsberichte der Akademie der Wissenschaften zu Berlin, sessions of Oct. 1859 (1860) 662-665.

[29] Kirchhoff, *Zusammenhang* (ref. 21), p. 784.

[30] Kirchhoff, *Zusammenhang* (ref. 21), p. 786 f.

the solar spectrum and the spectra of the chemical elements" and that found their way into the collected works.[31] In this revision he now commented on the issue as follows:

> The necessary complement of the proof could easily be given, when a plate could be assumed as possible, that had the property for rays of wavelength between l and $l+dl$ and polarization plane parallel to a, to let them penetrate unweakened, for rays of other wavelength or opposite polarization direction, however, to fully reflect them.

Contrary to his earlier convictions he dismisses the existence of such a body, however, only to make a new claim for an even more intricate object:

> The assumption, that such a plate is possible, is, however, justified by nothing. On the other hand, a plate is possible that lets through and reflects different amounts of rays that arrive from the same direction according to their wavelength and polarization direction. A plate that is this thin, that it shows the colors of thin plates for the visible radiation and which is put into the way to the rays at an angel, provides this.[32]

Interestingly, the reason to drop the first hypothesis is not a specific objection against it but the wholesale rejection due to a lack of justification. But is the case for the new "possible" object in a better state, does it exist? In his 1860 presentation of the second proof Kirchhoff stressed in the introductory section that his proof would rest on the assumption of the existence of completely black bodies; the crucial plate, however, was rather silently introduced later in the course of the experiment-like argument only in the third section:

> In the setup of fig. 2, tab. III one imagines a small plate P (fig. 3) be put in between the holes 1 and 2, that shows the colors of thin plate for visible radiation, and that partly due to its small thickness, partly due to its material composition neither emits nor absorbs a recognizable amount of radiation.[33]

The new crucial assumption is that of a so-called perfectly diathermanous plate, which is an object that both should consist of material fully transparent for heat waves and should show at the same time the colors of thin plates (i.e. in the case that the plate is appropriately thin, i.e. order of wavelength, it reflects more or less of the radiation according to the wavelength). This object, however, could have been called with equal right as justified by nothing. Two years later Kirchhoff now introduced all three assumptions, the conceivability of black-bodies (being still the "essential aid" in the proof), of completely diathermaneous bodies, and of perfect mirrors right in the first section of his work.[34]

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[31] Gustav Kirchhoff: Untersuchungen über das Sonnenspektrum und die Spektren der chemischen Elemente, 2nd ed., Berlin 1862, pp. 22-39; also in: Gesammelte Abhandlungen, pp. 571-598. The 1862 version also omits among others the final paragraph on the claimed validity of the law for fluorescent bodies.

[32] Kirchhoff, *Untersuchungen* (ref. 31), p. 26.

[33] Kirchhoff, *Verhältnis* (ref. 20), on p. 279.

[34] Kirchhoff, *Untersuchungen* (ref. 31), on p. 23.

It should have become already clear, that the proof Kirchhoff had presented in 1860 and 1862 bears nothing from the simplicity of the initial argument. And it is little wonder that later commentators found rather harsh criticism for both proofs. Frederic de la Provostaye already in 1863 and along similar lines much later Ernst Pringsheim in 1903 (who still emphasized that "Kirchhoff's derivation is without any flaw"), doubted all three assumptions of the existence of completely black, completely reflecting, and completely diathermanous substances.[35] Wilhelm Wien took up the issue of the postulated plate in the first proof (without direct reference to Kirchhoff's first proof) in a publication in 1894 and demonstrated that such an object would actually violate the second law of thermodynamics, the very starting point of Kirchhoff's considerations.[36] While the French radiation experimentalist Aimé Cotton in his review on "The present status of Kirchhoff's law" in 1899 concluded that the first proof "that is too frequently reproduced in the classic works of the present day, does not establish the law rigorously" (due to the improper assumption of the one-wavelength plate), he nevertheless believed that in particular for the second proof "these imaginary bodies may be realized of a higher and higher degree of approximation, and this renders their use legitimate."[37] The spectroscopist Heinrich Kayser shared this view for some of the "imaginary bodies" Kirchhoff had employed, but not for all. Hence he could not approve of the validity of the second proof and wished in 1902 "to see the proof replaced by another that does not present such logical difficulties."[38] Finally, Wilhelm Wien who himself gave a prominent formula for the function Kirchhoff tried to establish, judged in his encyclopedia contribution in 1909 Kirchhoff's second proof being "extremely artificial and onerous."[39] Only Woldemar Voigt called the proof "admirable."[40] Balfour Steward's dictum, "that the proof of the Heidelberg Professor is so very elaborate that I fear it has found few readers" hence turned out wrong in the long run.[41]

Let us take a closer look at the second proof. Like in his first proof Kirchhoff thought in terms of an experiment. This time employing an intricate set-up of holes or diaphragms,

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[35] Cf. Kayser: *Handbuch* (ref. 7), p. 26; Ernst Pringsheim: Herleitung des Kirchhoffschen Gesetzes, Zeitschrift für wissenschaftliche Photographie, 1 (1903) p. 360f.

[36] Wilhelm Wien: Temperatur und Entropie der Strahlung, Annalen der Physik und Chemie, 52 (1894) 132-165, here p. 163. Due to the Doppler effect a plate that would completely reflect, absorb, and let penetrate, resp., certain wavelength ranges, would lead to contradictory results when moved. Apparently referring to Kirchhoff's second proof, Wien concludes that one can only require that certain wavelengths are reflected etc. to a higher degree but not fully.

[37] Cotton, *Status*, p. 267.

[38] Kayser, *Handbuch* (ref. 7), p. 27.

[39] Wilhelm Wien: Theorie der Strahlung, Encyklopädie der mathematische Wissenschaften, Leipzig 1909, p. 285.

[40] Woldemar Voigt: Über die Proportionalität von Emissions- und Absorptionsvermögen, Annalen der Physik, 67 (1899) 366-387, on p. 366.

[41] Balfour Stewart: Reply to some remarks by G. Kirchhoff in his paper "On the history of spectrum analysis," Philosophical magazine 25 (1863) 354-360, on p. 359, cited after Siegel, *Stewart* (ref. 11), p. 589.

completely black walls, ideal mirrors and the perfectly diathermanous plate. The argument of the proof is given on the basis of three drawings:

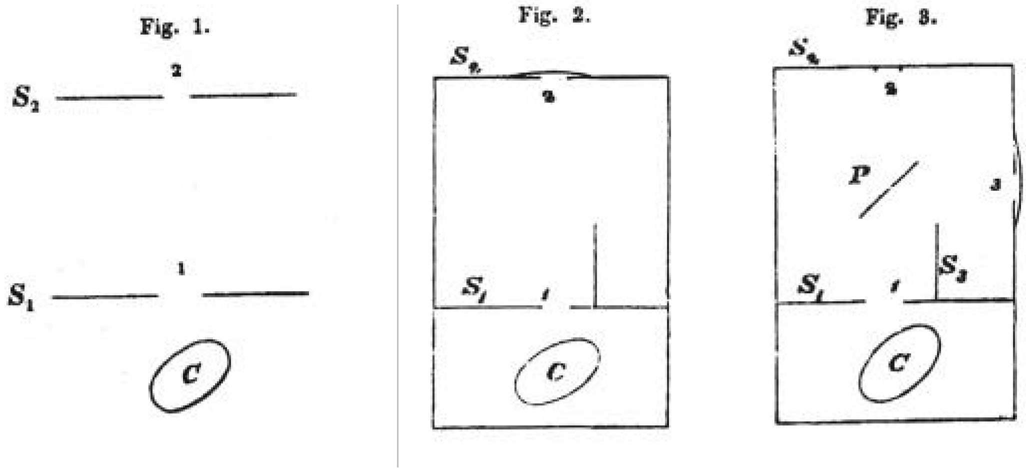

FIG. 1 Drawings used in Kirchhoff's second proof of his law.[42]

What should interest us here is the way the proof is given: It is a combination of imagined situations with imaginary objects and the argument is given step-by-step as if a real experiment would be performed, in short, it is a certain type of thought experiment. Consider e.g. the following citations typical for the argumentation:

> Now imagine that the surface 2 is removed and the uncovered hole closed by a piece of a completely reflecting spherical mirror put directly behind...
>
> For the set-up of fig. 2 imagine a plate of the characterized kind be put between the two holes 1 and 2 ...
>
> The hole 2 be covered by a black surface ... the hole 3 in the one instance by just such a surface... in the other instance by a perfect spherical mirror...
>
> If one conceives now the body *C* replaced by another black-body of the same temperature...[43]

The independence from the material properties was derived by replacing the kind of black-body *C* and by variation of the thickness of the diathermanous plate *P*. This allowed the desired conclusion that the relation between emission and absorption, that clearly holds for the total amounts can be decomposed spectrally and with respect to polarization such that there is a unique ratio for each wavelength. This is done by applying Fourier theory to the relations that follow from the theory of colors of thin plates.

42 Kirchhoff, *Untersuchungen* (ref. 31), pp. 24-26.

43 Kirchhoff, *Untersuchungen* (ref. 31), p. 25ff.

In distinction to the first proof, Kirchhoff now also considered polarized radiation and the case that the space in which the heat radiation propagates is not necessarily empty but filled by a diffracting medium. With help of a law by Helmholtz, he further generalized the range of applicability of his law for cases with absorptive and reflecting media.[44] The much greater length of the second proof hence has two reasons: first, the more intricate experimental argument and, second, the extension of the general theory to a wider range of applicability on radiation within material media of various kind.

While we do not know exactly, for what reason Kirchhoff had withdrawn his first proof so quickly,[45] be it criticism of colleagues or second thoughts of his own, the second one was criticized in print as early as 1863 by the French physicist Frederic de la Provostaye.[46] He did not accept that one could presuppose the existence of perfect mirrors and in particular of fully diathermanous bodies. Heinrich Kayser systematically discussed these objections among others in his very detailed study much later in 1902. Here he identified four questionable presuppositions: First, he pointed out that the assumption of black-bodies were "indeed a bit questionable," a closer look, however, revealed that "strictly speaking, a black-body cannot exist, under given circumstances, however, any body can play this role approximately."[47] The assumption of the existence of completely diathermanous bodies, next, presented much greater difficulties. Either one had to conclude that only the vacuum can satisfy the required properties, which was clearly useless, or possibly a diluted gas could fit, that, however, would emit nothing and hence would not reach thermal equilibrium. As a consequence, it appeared far from justified to Kayser to employ these entities in a rigorous proof.[48] The third presupposition Kayser mentioned is the existence of perfect mirrors that had already been used in the literature since Fourier and which was obviously the least problematic point. The last point is the more of less implicit assumption of a kind of locality. Emission and absorption should only depend on the local conditions and not on the conditions in neighboring regions.[49] In summary, a thorough analysis of Kirchhoff's second proof like Kayser's, arrived at the insight that though ideal mirrors and black-bodies should be of no harm as idealizations in general since they can be well approximated, the completely diathermanous plate was as much a phantom as the one-wave-length-plate before. A critical reader could have gotten the impression that Kirchhoff was exposed as a recidivist of postulating non-existent ideal objects.

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44 Kirchhoff, *Untersuchungen* (rep. 31), p. 32. Hermann von Helmholtz: Handbuch der physiologischen Optik, Vol. 1, Leipzig 1856, p.169.

45 There is no indication that Kirchhoff considered the Doppler effect, cp. ref. 36.

46 Frederic de la Provostaye: Considération théorique sur la chaleur rayonnante, Annales de chimie et physique 67 (1863) 5-51.

47 Kayser, *Handbuch* (ref. 7), p. 27.

48 Kayser, *Handbuch* (ref. 7), p. 26 f.

49 Kayser, *Handbuch* (ref. 7), p. 30 f. Kayser believed that there actually were such dependencies but estimated them to be negligible.

Kayser's analysis also mentioned a different but related treatment in Paul Drude's book on optics of 1900.[50] This new proof, however, is actually that one given by Hermann von Helmholtz in his famous lectures of the 1890s that influenced many physicists among them Max Planck. That these lectures were published only in 1903 in the case of theory of heat, explains why Kayser refers to Drude only.[51]

Helmholtz started his proof with the requirement of thermal equilibrium and the consideration of black-bodies.[52] As for all black-bodies the total intensity of the radiation on a unit surface must be the same, it, however, could still be the case that the partial intensity for a certain color were higher than that for all other black-bodies, if only it were less for any other color. Hence Helmholtz concludes that by spectral decomposition the issue could be attacked:

> Let us imagine, for instance, a completely transparent prism in the interior of an absolutely black cover. Then one can achieve by appropriately arranged completely reflecting diaphragms d that only that radiation from the one side of the cover that originates at a certain surface element F enters a prism as a straight pencil and becomes refracted in such a way that a point g_1 on the other side of the cover receives only that radiation of the pencil with the color f_1, a different point g_2 only that of color f_2.[53]

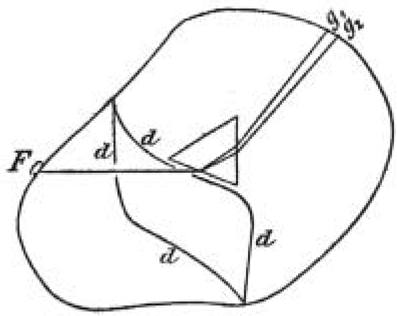

Fig. 23.

FIG. 2 Drawing used by Helmholtz in his proof.[54]

Following the addressed scenario Helmholtz demonstrated that when the emission for the first color f_1 at the point g_1 were greater than for all other black-bodies and accordingly less for the second color, the temperature for the first point would decrease

[50] Paul Drude: Lehrbuch der Optik, Leipzig 1900, pp. 454-457.

[51] Hermann von Helmholtz: Vorlesungen ueber theoretische Physik. Band: 6. Vorlesungen ueber Theorie der Waerme, edited by Friedrich Richarz, Leipzig 1903.

[52] Also Helmholtz presented two different proofs in his *Vorlesungen* (ref. 51). The first one on pp. 162-164 is according to Kayser rather close to Stewart's argument. As it only applies for radiation absorbed and emitted perpendicular to the surface, the statement is of limited generality. Cp. Kayser *Handbuch* vol. 2, pp. 8-12.

[53] Helmholtz, *Vorlesungen* (ref. 51), p. 165f.

[54] Helmholtz, *Vorlesungen* (ref. 51), p. 165.

at the expense of the second. Central ingredient for this argument is Helmholtz reciprocity theorem, which basically states that for each light ray traveling a certain path, a light ray that traveled the same path but in reversed direction would undergo the same rate of absorption, reflection, diffusion etc. as the original one. In order to generalize his result for arbitrary bodies Helmholtz adds few lines in a rather cursory if not misleading manner.[55]

Helmholtz' line of argument as different as it is from Kirchhoff's still employed a style of reasoning which is very similar: On basis of an ingenious thought experiment the conclusions are drawn. Like Kirchhoff he presupposes the existence of perfect mirror and completely black-bodies; in a sense, only the completely diathermanous plate was replaced by the completely transparent prism.

All proofs employ at one stage or other Helmholtz' "general reciprocity law" that he had postulated three years before Kirchhoff's.[56] The question of its proof was raised by none of the authors; Planck even presented a generalized reciprocity theorem in his 1906 book without proof.[57] At this time the problem had actually already been addressed in the physics literature and the author found it strange that both Kirchhoff and Clausius had overlooked this law or at least had refrained from formulating it.[58] For mathematicians the issue was different. David Hilbert would only point out to his students that the theorem can be easily transformed into a problem in theory of surfaces, which had been proven by Pierre-Ossian Bonnet and Gaston Darboux long ago.[59]

Kayser, however, made short work of the Helmholtz-Drude proof by noting that complete transparency and dispersion of light simply exclude each other. Hence again, the conceived object and with it the proposed thought experiment did not exist even in principle.

This concludes the first group of 19th century proofs of Kirchhoff's law and we can identify telling similarities in the strategies of proof. If we put aside the probably rather simplistic explanation that Kirchhoff as well as Helmholtz and Drude coming from a physics tradition in which experimental physics was not separated and still dominated theoretical

[55] He, e. g. claims "that the same ratio" of emissive and absorptive power "that applies to the total radiation must also apply for each single kind of radiation separately" which would imply constancy with respect to the wavelength. Helmholtz, *Vorlesungen* (ref. 51), p. 166.

[56] Helmholtz, *Handbuch*, (ref. 44) p. 169. Cf. also § 42. in his *Vorlesungen* (ref. 51).

[57] Max Planck: Vorlesungen über die Theorie der Wärmestrahlung, Leipzig 1906, § 46.

[58] Rudolf Straubel: Über einen allgemeinen Satz der geometrischen Optik und einige Anwendungen, Physikalische Zeitschrift, 4 (1903) 114-117, here p. 115.

[59] David Hilbert: Strahlungstheorie, lecture course summer 1912, notes taken by Erich Hecke, typescript at Mathematisches Institut Göttingen, p. 59. (To be published in vol. 4 of David Hilbert: Writings on the foundations of mathematics and natural science, edited by Ulrich Majer et. al.)

research even in the modes of foundational and explanatory thinking,[60] a possible alternative interpretation may be that for some reason radiation theory defied simple derivations from fundamental principles and necessitated a quasi-experimental style of reasoning. Or is it the case that it were well possible to give a straightforward derivation from first principles, only some conceptual tools or mathematical technology were missing? We will come back to this point.

Nineteenth century proofs of Kirchhoff's law and its refutations

author	*existence claim*	*author*	*refutation*
Kirchhoff Dec. 1859	bodies that emit and absorb only radiation of one specific wavelength (and fully reflect all other)	**Kirchhoff 1862**	justified by nothing
		Wien 1894	would violate 2nd law when moved
Kirchhoff Jan. 1860	bodies showing colors of thin plates without emitting of absorbing any radiation itself	**Provostaye 1863** ...	"hypothèses gratuites"
Kirchhoff 1862	completely diathermanous bodies	**Kayser 1902**	"The limit of such a body would be the vacuum..."
Helmholtz c. 1890 Drude 1900	completely transparent prism	**Kayser 1902**	no dispersion with complete transparency
Richarz 1903	a diffraction grating instead	**Pringsheim 1903**	ray optics inappropriate for radiating ether

What about Kirchhoff's general style of theoretical work? Without doubt he influenced a whole generation of German and European physicists that subscribed to the phenomenological view. This approach goes back to Franz Neumann, Kirchhoff's teacher in Königsberg. Woldemar Voigt, another disciple of Neumann described with reference to the "famous" introduction to Kirchhoff's book on mechanics the general character of the phenomenological approach as the task of the theory, "that it should describe the motions that occur in nature completely and in the simplest way" such that "rigorous conclusions on the basis of a minimum of assumptions" could be drawn. "Such a view is called phenomenological. This shall indicate that the foundations of the theoretical treatment are exclusively taken from the direct observation..." In distinction to "Kirchhoff's 'description' of the effects" Voigt observed in the atomistic view the urge to get in addition an *explanation* of the effects at the expense of the ambiguousness of the assumptions.[61]

[60] Kirchhoff took over the chair for theoretical physics only in 1875 "where as professor of theoretical physics a new career opened up and where he concluded his life far away from experiment." Boltzmann, *Kirchhoff* (ref. 9)., p. vii.

[61] Woldemar Voigt,: Phänomenologische und atomistische Betrachtungsweise, in: Physik. Kultur der Gegenwart edited by Emil Warburg, Ser. 3, Vol. 3, Teil 1, Leipzig 1915, 714-731, here p. 715 f. On

According to the phenomenological view one should also not conceive of possible molecular mechanisms that would explain the effects and laws in a reductionistic way but rather describe the effects quantitatively and accurately by straightforward equations. Unfounded hypotheses and in particular molecular ones were unacceptable. Considered "today's greatest physicist" by Woldemar Voigt or praised for his "furthermost caution and conscientiousness" by Helmholtz, Kirchhoff was widely seen as a model scientist who in Boltzmann's words like Euler, Gauss and Neumann before defined the "prototype of the German way to treat mathematical physical problems."[62] Radiation theory as it turns out put phenomenology to the test since the basic phenomenological doctrine of banning the use of special hypothetical models would naturally extend to the ban of conceived objects with idealized properties never observed in reality. Hence, we may ask, was Kirchhoff's proof of his law that met so much criticism just the exception to the rule?

It may strike that Boltzmann in his Graz rectorial address on Kirchhoff did not comment on this issue but praised the beauty of Kirchhoff's work on mathematical methods from which he takes as characteristic points of his approach: highest precision in the hypotheses, careful analysis ("feine Durchfeilung"), and consequential amplification of the insights without concealing any difficulties but illuminating the slightest discrepancies ("leisesten Schattens").[63] He, however, juxtaposes Kirchhoff's approach with Maxwell's work on gas theory where first a "chaos of formulae" is generated, then by means of a miraculous substitution, "which to explain no time appears to be available, the formula now spews out result after result until as a surprising final effect the heat equilibrium of a heavy gas is gained..."[64] Having seen the reception of Kirchhoff's second proof in particular by Stewart and Wien, one might, however, conclude that Kirchhoff's style was not always this distant form Maxwell's as Boltzmann suggested. Interestingly too, Kirchhoff himself did apparently not discuss his own law in his lectures on mathematical physics as the fourth volume of the published lectures on the theory of heat, that was edited by Planck, indicates.[65]

There was at least one physicist who saw the necessity to justify the model phenomenologist's inclination to deal with conceived bodies. He interpreted an element of the phenomenological approach to extend to the standards of proof: the element of simplicity and economy. Friedrich Pockels, Kirchhoff's former Heidelberg colleague, commented in 1903 directly on the specific type of Kirchhoff's argumentation and accentuated the use of conceived bodies as follows:

Neumann's phenomenology cp. its characterization in: Olivier Darrigol: Electrodynamics from Ampére to Einstein, Oxford 2000, esp. pp. 43-49.[62] Quotations of Voigt, Helmholtz, and Boltzmann after Hentschel, *Kirchhoff* (ref. 15), p. 416.

63 Boltzmann, *Kirchhoff* (ref. 9), p.30.

64 Boltzmann, *Kirchhoff* (ref. 9), p. 30.

65 Gustav Kirchhoff: Vorlesungen über mathematische Physik. Vierter Band: Theorie der Wärme, Leipzig 1894.

> This operating with bodies or processes in thought, that are in reality only approximately realizable, may appear at first sight strange; it is, however, absolutely admissible as a means of simplification of the argumentation, for the truth of the facts to be proven cannot depend on the degree of perfection of our artificial instruments.[66]

Therefore Pockels turned the unjustified assumptions, which appeared external to the chosen approach ("strange"), into admissible means or tools of argumentation within the phenomenological doctrine.

This interpretation that clearly served as a defense of Kirchhoff against criticism, is illustrated further by pointing to the present production of these "artificial" black-bodies in the *Physical-Technical Imperial Institute*. In stark contrast to the other views on the proof that complained about its lengthiness and complication, Pockels' implied that without the assumption of these conceived bodies the proof still were feasible but even more complicated. When Kirchhoff was a model theoretical physicist and his style, e.g. in Boltzmann's characterization, exemplary, how does his proof of his law fit into the picture? And in what sense can it be true that Kirchhoff's "subtle way" of deriving his law independently of any material properties "was later the methodological model for the investigations of Planck and Einstein"?[67]

Planck who edited the republication of Kirchhoff's works on emission and absorption in 1898 for *Ostwald's Klassiker* added a handful remarks, some of them commenting on more recent developments and some clarifying some subtle steps in the argumentation. He did, however, not mention any of the above-discussed general objections.[68] And again in 1906 when he presented his proof of the law, he remained conspicuously silent on the issue how much, if at all, his treatment was influenced by Kirchhoff's. Three years later Wien justified the disregard of Kirchhoff's argumentation:

> Today, it is hardly necessary to follow the original proof of Kirchhoff when one chooses the definitions and the starting point slightly differently.[69]

It appears that this tendency of partial reinterpretation and alleged slight modifications is characteristic for the strong identification of the early generations of theoretical physicists, most of them closely related as colleagues or teachers and disciples, within a tradition. As much as Max Planck stands for a turning point in the conceptual frame of theoretical physics and its professionalization, we will see in the following how much he

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66 Friedrich Pockels: Gustav Robert Kirchhoff, in: Heidelberger Professoren aus dem 19. Jahrhundert, edited by Fritz Schöll, Heidelberg 1903, Vol. 2, pp. 243-263, here p. 256 f.

67 Hentschel, *Kirchhoff* (ref. 15), here p. 430.

68 Gustav Kirchhoff: Abhandlungen über Emission und Absorption (Ostwald's Klassiker der Exacten Wissenschaften Nr. 100) edited by Max Planck, Leipzig 1898. Annotation on pp. 37-41

69 Wien, *Theorie* (ref. 39), p. 285.

70 Heinrich Kayser: Ziele der Zeitschrift, Zeitschrift für wissenschaftliche Photographie, 1 (1903) 1-4, here p. 1.

still tried to link his work and style of research and reasoning to his 19th century colleagues and in particular to Kirchhoff.

ON EXPERIMENTAL THINKING II: THE IDEAS AROUND 1900

As we have mentioned before, the two major accounts on the status of the foundation and applicability of Kirchhoff's law around 1900 were given by Cotton and Kayser. These papers will not have taken Planck by surprise, who had so closely followed the works by Kirchhoff and Helmholtz. So we have reason to assume that they basically reflect the situation concerning the status of Kirchhoff's law rather well and it was this situation of shaky foundation and dubious assumptions in his prerequisite in which Planck did his work on the radiation formula.

Kayser, as we have mentioned, concluded his analysis of the proofs from the 19th century with the statement that though he did not want to imply that he would doubt the correctness of Kirchhoff's law, which would have implied the deathblow to Planck's theory that, too, but he wished the proof replaced by a logically sound one. This wish he saw fulfilled by a brand-new and rather simple proof published in 1901.

Let us approach the new conception underlying this proof by first looking at the rearguard action of the Kirchhoff-Helmholtz position. Friedrich Richarz, who edited Helmholtz' lectures on the theory of heat, thought he could save his master's reputation in this matter by a brief article. This article was actually the first one that appeared in a new journal, whose chief editor was no other then Kayser. This *Journal for scientific photography, photophysics and photochemistry* should nonetheless mainly deal with the physics of the ether as Kayser mentioned in the introduction.[70] When Richarz pointed out that he could save Helmholtz' proof by a simple modification and hence would remain in the typical optical framing of the thought experiment, this already stands out from the concept of radiating ether now more popular. Richarz reminded the readers that Helmholtz had also mentioned the possibility of using a line grating instead of the prism in passing in his lectures and in this way one could evade the devastating contradiction of in the rather ill-defined assumptions. For this reason, Richarz concluded, Helmholtz' "simplification" of Kirchhoff's proof were still "thoroughly flawless."[71]

It was Ernst Pringsheim who had provided the simple new proof in 1901 and who had published a version of it in the same journal. This followed a retraction by Richarz, who wrote on his proposed modification of Helmholtz' argument:

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71 Franz Richarz: Bemerkungen zur Theorie des Kirchhoffschen Gesetzes, Zeitschrift für wissenschaftliche Photographie, Photophysik und Photochemie 1 (1903) 5-8, here p. 8. The second article in the journal, interestingly, dealt with the experimental test to the law.

> Also in this way the proof can still not be given in a flawless manner, a fact to which my friend E. Pringsheim drew my attention; unless one takes the standpoint to consider always only the mutual irradiations of parts of the *surface of the body*, as it is done by Helmholtz in his lectures.[72]

Since it had been the essential progress in recent radiation theory to consider the radiation that is present in the ether itself, Helmholtz' set-up and with this also Kirchhoff's had no longer any value.

Pringsheim had excelled both as experimental and with his new proof also to some extent as theoretical champion of heat radiation coming from the *Physical-Technical Imperial Institute* at Berlin and now professor in Breslau. From his experimental work he drew the conclusion that Kirchhoff's law would "not be valid for all kinds of light but only for those phenomena for which the light emission is a function of the temperature only." As a consequence there existed no gaseous light sources that obey the law.[73] Now he made Richarz concede that Helmholtz' proof could not be saved and that the one and only flawless proof were his new one. Pringsheim did this, however, by bringing in an argument which lay outside the theoretical frame in which from Kirchhoff over Helmholtz up to Kayser the discussion took place. For them radiation was emitted by the surfaces of bodies only and otherwise obeyed the optics of rays. So, Pringsheim simply pointed to the new understanding of radiating ether or pure radiation. Light paths being restricted by diaphragms in such a way that radiation would come from one well-defined surface area and go to a distinct other one could not longer be maintained. Furthermore, radiation seen like a substance itself implied a loss of control of the radiation paths and caused a first attack of the visualization of the proof in terms of a step-by-step experiment.

How did Pringsheim choose his argument in the proof and in what manner did he replace the nicely arranged set-ups of Kirchhoff and Helmholtz? First of all, he made clear that his approach would do without all three of Kirchhoff's questionable assumptions but, clearly, he needed something else:[74]

> The derivation that is given in the following does not make assumptions of this kind, but starts with the empirical fact that arbitrarily many bodies are existing or producible, resp., whose absorptive power varies in completely different ways from wavelength to wavelength.[75]

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72 Franz Richarz: Nochmalige Bemerkung zur Theorie des Kirchhoffschen Gesetzes, Zeitschrift für wissenschaftliche Photographie, 1 (1903) 359-360.

73 Ernst Pringsheim: Kirchhoff's Gesetz und die Strahlung der Gase, Annalen der Physik, 45 (1892) 428-459 on p. 428 f.

74 We follow here Ernst Pringsheim: Herleitung des Kirchhoffschen Gesetzes, Zeitschrift für wissenschaftliche Photographie, 1 (1903) 360-364, here right at the beginning on p. 360. Pringheim presented his proof first in Berlin under the eyes of Planck as Einfache Herleitung des Kirchhoff'schen Gesetzes, Verhandlungen der Deutschen Physikalischen Gesellschaft, 3 (1901) 77 and 81-84. In a certain parallel with Kirchhoff's papers from 1860 and 1862, also Pringsheim's first presentation introduced his assumptions in the course of the argument while the 1903 one presented them at the outset.

75 Pringsheim, *Herleitung 1903* (ref. 74), p. 361.

Combined with Carnot's principle, which is the indispensable basis of all arguments, this would suffice as assumptions.

Pringsheim seems to have observed from the prior derivations that after the implementation of thermal equilibrium the next most important step lay not so much the in radiation geometry but rather in the replacement of one body by another of different physical properties. While Kirchhoff and Helmholtz treated the generalization of the law over all wavelength on the one hand and for all bodies on the other hand in two rather separate steps, Pringsheim had realized that both can be done together: The replacement of the radiating substances does not only change their emissive and absorptive properties generally, i.e. being more or less black, but also the spectral variation of these properties change. This fact then can be exploited to deduce from the constancy of the ratio of total emission and absorption the according for each wavelength separately. This, in a way, replaced diathermaneous plates, prisms, gratings and the like used before.

Pringsheim proceeded in two steps that also Kirchhoff took (establishing the formula and exhibiting the unique radiation distribution for a black-body), but in opposite order: First, he considered a body with absorptive power A_l and the radiation within a cavity that contains this body and establishes that in equilibrium a unique radiation distribution must be reached that is "quantitatively and qualitatively the same a completely black-body would emit, if it existed."[76] But to show this, not the body is replaced but the cavities or rather its walls, which are exchanged by ones of different material properties, however, of the same temperature in order not to disturb thermal equilibrium. Assuming that in principle it could happen that the distribution of the emitted radiation energy over the wavelength l were different for different cavity materials $e_{1l}, e_{2l}, ..., e_{nl}$, the total absorbed energy of the body must not change, which is

$$\int_0^\infty A_l e_{1l}\, dl = \int_0^\infty A_l e_{2l}\, dl = \quad ... \quad = \int_0^\infty A_l e_{nl}\, dl \ .$$

Now the assumption on existent or producible substances is used by stating that the absorptive powers A_l of the body within the cavities can take arbitrary functions of the wavelength unrelated to the emission of the different wall materials $e_{1l}, e_{2l}, ..., e_{nl}$, which in consequence all must equal to a universal function for an ideal blackbody e_l, since otherwise the integrals could not all coincide.

Only as a second step Kirchhoff's formula is then derived, which is interestingly done by consideration of the mutual irradiation of two surface elements, one of the body and one of the cavity, and with help or the (unproven) reciprocity theorem of Helmholtz. Hence, the new view of radiating ether was put aside for a moment. A closer look actually

[76] Pringsheim, *Herleitung 1903* (ref. 74), p. 363.

reveals that the new view on the radiation used by Pringsheim to invalidate the Helmholtz-Richarz proof, is not made explicit in Pringsheim's publications on his proof.

Let us now turn to the presentation and the style of the argument of Pringsheim's proof and let us relate it with Kirchhoff's. This proof although rather different when compared with the preceding ones, still employed an experimental thinking style. To exhibit this just look at the style of the argument:

> Bringing the same body K into an arbitrary number of cavities one after the other that all have the same temperature but are completely different in shape and composition of the bodies they contain, the emission of the body K remains unchanged as does the absorptive power A_l for each determinate kind of radiation.[77]

In distinction to the earlier thought experiments Pringsheim presents an experiment that actually can be performed in the laboratory and the citation given can equally well be read as an experimental observation. Only in the following it becomes clear that the statement is a result of a theoretical consideration. Interestingly the variation of the absorptive power A_l of the body K is not presented in terms of replacing the actual body but by pointing to the arbitrariness of the function A_l.

The crucial step in the mathematical parts of the argument in the proofs of Pringsheim and Kirchhoff are rather similar: The integrals of certain emission functions for black-bodies of different material properties are multiplied with a set of auxiliary functions and equate always to the same value. The conclusion is then, that this can only be the case when the emission functions themselves are identical. In Kirchhoff's case the set of auxiliary functions are generated by changing the thickness of the (non-existent) diathermanous plate, in Pringsheim's case by supposing that there exist infinitely many substances with different functions of their absorptive power. While Kirchhoff had mathematical conclusiveness up his sleeve with Fourier's well-defined theory but problems with his ontology, Pringsheim had problems with both: It is not clear to what mathematical theory he could for example claim orthogonality of his function set, and how should he could show that the respective functions of existing or producible materials actually form a complete set. These subtle questions of functional analysis, however, were clearly external of the physicists' considerations of his time, but they would surface in a rigorous mathematical analysis of the proofs.

As we have indicated at some points before, apart from the structure of the proofs one has also to compare the scope of the claims. Pringsheim considered only radiation in otherwise empty space, whereas Kirchhoff and Helmholtz as well as later Planck had employed considerable effort to extend the conclusion for radiation in transparent, diffusing and absorbing media. The determination of the generality of the law must be seen as part of its foundation. That Pringsheim does not even mention this necessity can also be seen as an indicator that the role and application of the law had undergone a

77 Pringsheim, *Herleitung 1901* (ref. 74), p. 82.

transformation. The black-body radiation that could be produced in cavities had become the more important object of research, the relation of emission and absorption of radiation in arbitrary media got less attention. This development is also reflected by the fact we have learned at the end of Pringsheim's 1901 paper, that he inverted Kirchhoff's reasoning in a certain sense. While the latter concluded from his law that the radiation in a cavity in thermal equilibrium must be black, the former first established this fact in order to derive Kirchhoff's law.

IN THE MIDDLE GROUND: PLANCK'S LATE PROOF OF HIS PREREQUISITE

Max Planck, although he did not raise any direct criticism to previous work, felt obliged to redo the complete proof in his 1906 book. He mentioned only in the preface that his treatment frequently deviated from the "customary methods of treatment" where "factual or didactic reasons" suggested this "especially in deriving Kirchhoff's laws" among others, but he did not indicate in what cases it actually were factual reasons for dismissing older account.[78]

The derivation of Kirchhoff's law is discussed on 25 pages in the first section of Planck's book with the heading "fundamental facts and definitions." In a step-by-step manner a number of different configurations are discussed that finally shall insure general validity of Kirchhoff's law. Planck considered the radiation from the beginning on within a medium and allowed absorption, reflection, refraction and diffusion but excluded diffraction "on account of their rather complicated nature" by requiring that surfaced should not have sharp edges, he took the most general case by then.[79]

Let us have a cursory look at his main steps. As in equilibrium the absorbed and emitted energies of a volume element must be equal when summed over all wavelength, the first task Planck had to establish was, that one has equilibrium also for each wavelength separately. For this Planck considered an (approximately) infinitely extended, homogeneous and isotropic medium and argued that:

> The magnitudes e_n, a_n, and K_n [the intensity of radiation of frequency n are independent of position. Hence, if for any single color the absorbed were not equal to the emitted energy, there would be everywhere in the whole medium a continuous increase or decrease of the energy radiation of that particular color at the expense of the other colors.[80]

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[78] Planck, *Vorlesungen* (ref. 57), p. v. "Hierbei bin ich öfters, wo es mir sachliche oder didaktische Gründe nahelegten, von der sonst üblichen Art der Betrachtung abgewichen..." The English edition, Theory of heat radiation, New York 1959 (first published 1914), p. xi, translates "sachliche oder didaktische Gründe" rather opaquely as "the matter presented or considerations regarding the form of presentation." Pages of the translation that we follow in most cases are cited in brackets.

[79] Planck, *Vorlesungen* (ref. 57), p. 2 (2).

[80] Planck, *Vorlesungen* (ref. 57), p. 27 (25).

But this would be in clear contradiction to equilibrium. This is obviously a new type of argument relying on the application of symmetry principles: homogeneity and isotropy are exploited to establish Kirchhoff's relation for homogeneous media.

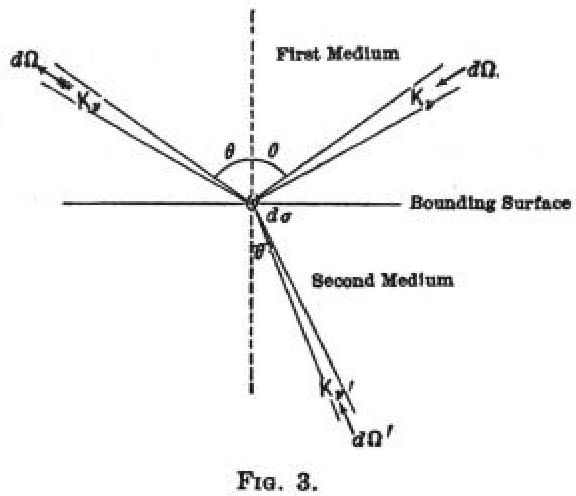

FIG. 3 Drawing used by Planck in his proof.[81]

Next, Planck considers two infinite media of different refraction index bordering at each other. After some subtle discussion of the situation at the bordering surface drawing again on Helmholtz' reciprocity theorem, Planck uses this case to establish the independence of the relation from the material properties of the media (refraction indices). In Planck's words the crucial insight was, that with respect to the second medium

> ... *the ratio of emissive power to absorbing power of any body is independent of the nature of the body.* For this ratio [in the second medium] is equal to the intensity of the pencil passing through the *first* medium which ... does not depend on the second medium at all. The value of this ratio does, however, depend on the nature of the first medium...[82]

Again, this argument is very much one of applying symmetry considerations to general principles. Finally, Planck argued that one could consider "n emitting and absorbing adjacent bodies of any size and shape whatever in the state of thermodynamical equilibrium" and hence decomposed the space in which the radiation were considered in more and more general ways that may approximate any physical situation.[83]

What remained from the experimental thinking of Kirchhoff, Helmholtz and Pringsheim? The last point of stepwise generalization at least bears still some relation to experimental strategies. His discussion of Kirchhoff's law in 23 subsections, treating a host of cases of

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[81] Planck, *Vorlesungen* (ref. 57), p. 35 (33), reproduction from English edition.

[82] Planck, *Vorlesungen* (ref. 57), p. 43 (40).

[83] Planck, *Vorlesungen* (ref. 57), p. 39 (37).

absorbing, diffusing, diathermaneous etc. media and arrangements of various media with bordering surfaces of different properties still correspond to an experimental test series. But otherwise Planck did completely without diaphragms, lenses, mirrors, prisms etc., only the choice of bordering surfaces of regions of different material composition constitute the conception of experiment-like set-ups. The kind of experimental thinking style we found before with Kirchhoff, Helmholtz, and even Pringsheim has given way for a transitional style that combined experimentally motivated stepwise treatment with analysis of general principles.

Interestingly, Planck's text did not raise the point Pringsheim used against Richarz' fix of Helmholtz' argument, i.e. the new understanding of radiating ether that invalidates all considerations that rely on irradiations of surfaces only. As Planck avoided the use of surface elements, he still employed the language of ray optics in considering pencils passing through media; but since no devices were required to confine radiation of a certain kind (direction of propagation, wavelength, polarization...) like diaphragms, mirrors, prisms etc., his considerations applied for each volume element and hence allowed each of them to emit or absorb radiation of arbitrary direction.

Keeping a low profile, Planck only noted that Kirchhoff's and Pringsheim's proofs had not considered the cases of absorbing and diffusing media, no word on the conclusiveness of their arguments.[84] So, by 1906 one has Pringsheim's generally accepted simple proof, which he had published in many versions in journals for physics, mathematics, electrochemistry, scientific photography etc. and Planck's authoritative book. The issue was settled now, wasn't it?

For most physicists it may have, but even in the standard physics encyclopedia no clarity was reached. Wilhelm Wien finished his reference article on the "Theory of Radiation" in 1909 but his argumentation clearly fell behind the state of the discussion reached with Planck. Picking elements of Pringsheim's reasoning he first derived the law for the total radiation (which is, however, just energy conservation). Then he noted that there were no difficulty to complete the proof such that it pertains to each wavelength and suggested to use a thin plate like Kirchhoff's but in order to consider whether the radiation pressure (a favorite effect of Wien) would move the plate for different distributions of the energy over the wavelength. (If this happened a *perpetuum mobile* could be constructed.) Not only did Wien revive an experimental thinking style of the 19th century he moreover subscribed to an odd ontological foundation:

> That Kirchhoff's law is valid for each wavelength has its foundation in the fact that we possess instruments in order to disperse radiation according to the wavelengths it contains. For this reason is the radiation of each spectral region independent of the existence or radiation of other spectral regions.[85]

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84 Planck, *Vorlesungen* (ref. 57), p. 43 (40).

85 Wien, *Theorie* (ref. 39), p. 282-357, here p. 285-287, citation on p. 287.

Does our possession of instruments make the laws of nature?

As we have seen, Planck did not discuss the relation of his proof to previous ones in his book or elsewhere before 1906. But later when his derivation was challenged and he felt obliged to respond, he, at the same time, would begin to comment on Kirchhoff and Pringsheim. It was the Göttingen mathematician David Hilbert who made Planck break his silence on his predecessors.

ON MATHEMATICAL THINKING: HILBERT CLAIMS THE FIRST VALID PROOF IN 1912

In fall 1912 David Hilbert was widely recognized as the world-leading mathematician. With Henri Poincaré dead, he was also the most prominent mathematician who had his eye on the recent developments in physics. What Hilbert's full plans and actions regarding a reshaping or modernization of physics as a whole were and how his attitude underwent several changes, cannot be discussed here; we will only treat this issue as far as it pertains to his involvement in the discussion of the foundation of radiation theory.[86] Generally known as the person who identified the problem of the axiomatization of physics as on of the challenges to 20th century mathematics, Hilbert was remembered well by a number of the 1912 participants of the meeting of the *German Association of Natural Scientists and Physicians*. Those who had witnessed his lively dispute with Ludwig Boltzmann on a hydrodynamics problem of the stability of a liquid in a vessel on this meeting nine years before will have recalled the amusement the audience entertained at a major fight of two authorities over a subtlety, at least in the physicists' reading.[87]

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[86] Recently, Hilbert's interest, work, and research politics in physics has found much attention. Cf. e. g. Leo Corry: David Hilbert and the axiomatization of physics (1894-1905), Archive for History of Exact Sciences, 51 (1997) 83-198; Leo Corry: Hilbert and physics (1900-1915), in: The symbolic universe. Geometry and physics 1890-1930, edited by Jeremy Gray Oxford 1999, 145-188; Arne Schirrmacher: The Establishment of Quantum Physics in Göttingen 1900-24. Conceptional Preconditions - Resources - Research Politics, in: History of Modern Physics. Proceedings of the XXth International Congress of History of Science (Liège 20-26 July, 1997) edited by Helge Kragh et.al., Turnhout 2001.

[87] A report on the meeting in the *Naturwissenschaftliche Rundschau* 18 (1903) p. 553-556, gave the following characterization of this Hilbert-Boltzmann dispute:

> At the end [of the talk] the speaker and Mister L. Boltzmann (Vienna) engaged in an extremely lively argument. According to an old experience nothing can raise one's own self-esteem as much as watching accepted authorities that quarrel on a question and the fight of words of both persons were received by the audience with noisy amusement. It should be just to say that the current case of stability can be readily maintained in the physical-experimental sense, while the question of the "transcendental" stability, where even an infinitesimal particle must not gain a finite velocity from a infinitesimal momentum, must remain undecided.

So, for the physicists in general Hilbert's 1903 problem was rather a subtlety, i.e. something "transcendental" and not many would enter a discussion about it like Boltzmann.

Initially, Hilbert had proposed to lecture at the 1912 Münster meeting on the application of integral equations to the kinetic theory of gases but changed his topic to radiation theory on short notice. Since this were another field of physical knowledge whose mathematical principles were not jet analyzed and whose foundation needed the mathematical tool of integral equations, as he had recently realized, with the same necessity as the kinetic theory of gases. This case would demonstrate "the fruitfulness and strictness of the method even simpler and more convincingly."[88]

Hilbert's interest in radiation theory and Planck's work, however, did not arise out of the blue but can be traced back at least to 1906, when he together with Hermann Minkowski studied Planck's new book.[89] Minkowski actually gave a lecture course on heat radiation in the summer term 1907 at Göttingen where he told the students, that

> ... with this course I do not only address physicists but to even higher degree pure mathematicians, who are usually more or less inclined to stay distant of these fields. It is in particular my intention, and Professor Hilbert, too, is of similar opinion and pursues similar aims, to win over the pure mathematicians to the inspirations that flow into mathematics from the side of physics. It is not improbable, that we will treat in the seminars of the next year's mathematical-physical theories especially of heat radiation.[90]

This quote shows both, the role Minkowski played for Hilbert's motivation to deal with physics and that it were initially not the physicists that they wanted to win over for mathematics, but that the pure mathematicians should be inspired to study modern physics in order to find new fruitful mathematical problems.

Apparently, this program was not followed exactly in the proposed way, as Minkowski turned more and more to relativity theory and Hilbert lectured much on (continuum) mechanics, while continuing and perfecting his research on integral equations that led to a book in 1912.[91] Only when he was considering physics problems in order to serve as applications for his general theory of integral equations, some time around early 1912 he must have realized that radiation theory might be a most telling application.[92] His lecture course on "mathematical foundations of physics" became the detailed developing of these ideas. Paul Ewald, a student of Sommerfeld, was hired in March to work through

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88 Cod. Ms. D. Hilbert 586, p. 5. The reading manuscript deviates from the published paper only in some marked passages like that quoted here and in the following. Hilbert provided reprints of his Begründung der kinetischen Gastheorie, Mathematische Annalen, 72 (1912) 562-577, at the meeting.

89 Hilbert remarked that this book gave rise to his work on radiation theory, cp. David Hilbert: Begründung der elementaren Strahlungstheorie, Jahresberichte der Deutschen Mathematiker Vereinigung, 22 (1913) 1-20, here p. 18. Minkowski gave a talk on the development of radiation theory up to Planck at the Göttingen Mathematical Society on Dec. 11, 1906, see Jahresberichte DMV 18 (1907) 71.

90 Hermann Minkowski: Wärmestrahlung, handwritten notes to his lecture course summer 1907, Cod. Ms. D. Hilbert 707, p. 2.

91 David Hilbert: Integralgleichungen, Leipzig 1912.

92 Hilbert asked Einstein in a letter from Feb. 30, 1912, that he should send him his "theoretical works on gas and radiation theory." Einstein Collected Papers vol. V, p. 439.

the literature on the proofs of Kirchhoff's law and he reported to Hilbert on April 11th, 1912, few days before the term started:

> Concerning Kirchhoff's law, Planck's proof is the best known to me. Pringsheim's proof Planck calls full of gaps, Wien's hints in his encyclopedia article hardly can satisfy me.[93]

Ewald promised to find out about other proofs before he would come to Göttingen the week after next. With his physics assistant researching the literature, Hilbert developed his account on radiation theory and Kirchhoff's law in his lectures that were in turn worked out by his mathematics assistant Erich Hecke.[94] Only few days after the term had ended Hilbert submitted his paper "Foundation of the elementary radiation theory" both to the *Nachrichten* of the Göttingen Academy and the *Physikalische Zeitschrift*.[95]

When Hilbert four weeks later at the 1912 Münster meeting raised his voice to teach the physicists a lesson on the status of Kirchhoff's law and the proper way to found it, he thus was well prepared. It was a joint session of the mathematics and physics sections of the association that drew the largest audience of all talks of the meeting with 140 persons.[96] Hilbert told his audience about Kirchhoff's law, that

> ... my remarks will show at the same time, I think –what at least surprised me–, that the previous theoretical efforts of proof have not been at all on the right track and also how little even in the simplest special cases they have been able to make plausible the first law of Kirchhoff.[97]

This statement clearly raises a number of questions: First of all, what is the "right track" according to Hilbert? Why did he discard the derivations even for simple special cases? And why did he not even concede some plausibility to the derivations of Kirchhoff, Helmholtz, Pringsheim, Planck, and Wien? What, after all, was for Hilbert the central content of Kirchhoff's law that needed proof?

Later in his talk Hilbert made clear to the physicists, what in his eyes the share of labor between physics and mathematics in the establishment of Kirchhoff's law were:

> This law appears here as a deep mathematical truth, whose content was found in the physical experiment and has been postulated on the basis of physical combinations and because of

93 Ewald to Hilbert, April 11, 1912, Cod. Ms. D. Hilbert 98, item 1.

94 Cp. footnote 59.

95 Hilbert, *Begründung 1912* (ref. 1). It was submitted to the Göttingen Society on Aug. 22, 1912.

96 Verhandlungen der Gesellschaft deutscher Naturforscher und Ärzte 84 (1913) part II, p. 78. According to Physikalische Zeitschrift 13 (1912) p. 1009, 90 physicists were at the meeting and main interest for them was the joint session with the mathematical section with talks by Hilbert, Nernst and von Smoluchowski. In the discussions of these talks were involved: P. Koebe, A. Sommerfeld, Ornstein, Krüger, G. Mie, Löwe, M. Wien, Konen, Rubens, Kaufmann.

97 Cod. Ms. D. Hilbert 586 (ref. 1), p. 5f.

> successful predictions, whose proof, however, becomes possible only by means of the theory of integral equations.[98]

Apparently, for Hilbert the only currency that counted was mathematical truth and logical conclusiveness. The suggestive power or plausibility a physicist would find in the experiment-like structure of the arguments did not cast any spell over him. Apart from this, Hilbert explicitly provided a reason why all proofs before him had to fail: due to the lack of the necessary mathematical tools. Reversing the argument Hilbert added a most noteworthy statement that can be seen to imply a whole program of his understanding of the relation between mathematics and physics and that also can serve as an explanation for his interest in physics in the first place:

> If we did not have the theory of integral equations the theories of gases and radiation would lead to it with necessity.[99]

As we have mentioned in the introduction unlike nine years before no lively discussion of the bold claims resulted, but after a question not directly to the topic by the mathematician Paul Koebe, Sommerfeld with all his authority praised Hilbert for the coherence of his presentation. The physicist Merian von Smoluchowski on the same line recognized the "enormous progress" that had occurred through this work of Hilbert: "The physicists will be grateful to him for this."[100] As we will see, this exuberant impression did not last for long.

Why was there no immediate critical response? Possibly, physicist had to sit down first in order to grapple with the unusual presentation.[101] Sommerfeld, however, must have been already well informed about Hilbert's work, as Hilbert had invited him to give some lectures in his place in his course at the end of the term. The topic, he wrote in his letter, would be Sommerfeld's choice "preferably, however, on radiation theory and quantum theory" and for good money.[102] So, at least Sommerfeld, who was trained as mathematician under Felix Klein, did subscribe to Hilbert's proof. Most other physicist, when they made an effort at all to understand Hilbert's approach, did not agree, most notably Ernst Pringsheim and Max Planck.

How did Hilbert's proof work? First of all, Hilbert shifted the framework in which the physics occurred again in a different direction. His radiation would live in a arbitrary continuos medium with in principle variable values for emissive and absorptive powers, a and h, as well as for the speed of light q (or refraction coefficient n) in each infinitesimal

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[98] Hilbert, *Begründung 1912* (ref. 1), p. 1062.

[99] Cod. Ms. D. Hilbert 586 (ref. 1), p. 15f.

[100] Hilbert, *Begründung 1912* (ref. 1), p. 1064.

[101] The paper also went through without any criticism in the physics review journal: Fortschritte der Physik 1912, 2, 347f. (reviewed by Rudolf Seeliger, who at this time was qualifying as *Privatdozent* under Planck.)

[102] Hilbert to Sommerfeld April 5, 1912, Sommerfeld papers, Deutsches Museum, Munich. Sommerfeld was highly paid with 1000 Marks for two lectures.

volume element. Moreover, which actually had been a criticism already of Kirchhoff's proof as we have mentioned before, these values could in principle depend on the neighborhood of the volume element considered.[103] The task is presented as follows:

> The most important question that now arises is that of the possibility of thermal equilibrium and the conditions that are necessary among the three coefficients q, a, and h for the occurrence of equilibrium, resp.
> To settle this question we first calculate the total energy density that exists with our assumptions at any arbitrary position xyz due to emission and absorption of the matter.[104]

Thus Hilbert considers in all generality the radiation that arrives at a certain volume element and equates it in equilibrium with the emitted one. The emitted energy must hence be equal to the sum over all path that bring radiation that was emitted somewhere else and was partially absorbed by the medium on its way to this very volume element. The resulting equation is an integral equation for a certain combination of the emissive and absorptive power functions and the velocity of light, with a certain symmetric kernel or propagator (e^{-A}/S).[105]

$$h - \frac{a}{4pq^2}\iiint \frac{e^{-A}}{S} h(x_1 y_1 z_1) dx_1 dy_1 dz_1 = 0$$

The general theory of integral equations that Hilbert just had put forward in his book now teaches how this equation yields a relation for the three position dependent functions *q*, *?*, and *??*and hence immediately provides Kirchhoff's law (with position dependent velocity of light).

$$\frac{q^2 h}{a} = \text{const.}$$

As this consideration would hold for all wavelength and temperatures, this combination must be a universal function of them, Hilbert concludes.

In our brief outline of Hilbert's proof we have omitted one point, Hilbert introduced rather in passing. Only four lines under his conclusion that none of the prior attempt of proof were flawless, Hilbert requires that the exchange of energy would only take place by radiation "that we will suppose to be of the same constant frequency."[106] But having said this, Hilbert had presupposed that the radiation is in equilibrium for each frequency independently. Did he thus presuppose what he wanted to prove? Apparently, Hilbert

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103 Kayser, *Handbuch* (ref. 7), p. 30.

104 Hilbert, *Begründung 1912* (ref. 1) p. 1058

105 Hilbert, *Begründung 1912* (ref. 1) p. 1059. *S* describes, roughly speaking, the evolution of a ray from xyz to $x_1y_1z_1$, A the absorption along the path, $A = \left|\int a \; ds\right|$.

106 Hilbert, *Begründung 1912* (ref. 1) p. 1057.

was after something else in his proof. Sooner or later Hilbert, however, would face this criticism.

Compared with the other approaches we have discussed, the following two points in particular characterize Hilbert's approach. As Planck had already dismantled all equipment of the experimenters workshop (the diaphragms, mirrors, prisms, plates...), he still stuck to the concepts of ray optics, where single pencils are considered that occasionally cross a boundary from one region to another of different material composition. So Hilbert, firstly, with his view that each tiny volume element can have its own absorptive, emissive, and refractive properties departed fully from the classical view of mutual irradiations of surfaces and provided an appropriate model for radiating ether. The most striking and essential second difference, however, must be seen in the fact, that Hilbert's approach was completely free of any experimental thinking style. There was simply nothing to be manipulated. In contrast to the experimentally influenced style of reasoning we have found before, his treatment rather demonstrates the opposite: a mathematical thinking style that first sets up a general manifold of possible situations and solutions and then imposes conditions (here of light propagation and equilibrium) that provides the solution set. The reason that the derived law holds is in particular that of the mathematical necessity, neither a mechanism nor a conceived sequence of experimental actions that exhibit the causal relations.

ON NOT LEARNING A LESSON: THE HILBERT-PRINGSHEIM POLEMICS 1912-1914

Apparently, neither Planck nor Pringsheim attended Hilbert's Münster presentation and nobody spoke up for them while their efforts to prove the law had been so bluntly invalidated. Let us first consider Pringsheim's reaction, which is rather well known, and then compare it with Planck’s, which is not.[107]

When Pringsheim heard of Hilbert's claims he must have been anything but pleased. His name stood for a simple and convincing proof of Kirchhoff's law like no other. He had published his proof several times in a number of journals ranging from the official presentation documented in the proceedings of the German Physical Society over specialized journals on scientific photography or on electrochemistry up to a journal on the mathematics and physics directed to high-school teachers.[108] Kayser in his handbook on spectroscopy after all the criticism to earlier proofs had finally concluded that

[107] The Pringsheim-Hilbert controversy was discussed by Max Born in his Hilbert und die Physik, Naturwissenschaften, 10 (1922) 88-93, p. 90f. Cp. also Leo Corry: Hilbert on Kinetic theory and radiation theory (1912-1914), The mathematical intelligencer, 20 (3) (1998) 52-58. The exchange between Hilbert and Planck is documented only in letters by Planck that were removed from Hilbert papers after his death and resurfaced only in the year 2000.

[108] Cp. Poggendorff vol. IV, 1194-1195 and vol. V, 1006.

Pringsheim's proof would give no reason for objections, and Wien in his encyclopedia article drew to some extent on this account also.[109]

That Pringsheim could not fully grasp Hilbert's argument as he could find it e. g. in the *Physikalische Zeitschrift* is apparent from the fact that the Breslau mathematician and former student and colleague of Hilbert, Constantin Carathéodory was asked to present Hilbert's paper in the Breslau physics colloquium, where he spoke in November 1912. But also Carathéodory could not convey Hilbert's ideas convincingly and did not understand Pringsheim's objections. Only four weeks later after "laborious discussions" he was able to grasp the main point, which he promptly communicated in a long letter to Hilbert.[110] The point Pringsheim raised may seem surprising, as it had nothing to do with Hilbert's questionable assumption of equilibrium for each wavelength separately, but rather attacked a completely new aspect (like in the Richarz case before). Considering the energy balance for one volume element, it was said, on had to consider both the energy that is exchanged via radiation and the energy that is exchanged by conduction. While the first can be decomposed according to wavelength, the latter cannot. Carathéodory concluded that there would still be an integral equation for this problem but it would no longer yield Kirchhoff's law.[111]

How serious this dispute became can be seen from the fact that eventually Carathéodory joined the Breslau physicist Rudolf Ladenburg and Max Born who stayed over Christmas in his hometown to discus Hilbert's reply letter. They finally agreed that Born should try to speak with Pringsheim to mediate in the conflict.[112] Born knew both sides well, as he had been Hilbert's private assistant in Göttingen and had also worked with Pringsheim in the Breslau laboratory he ran together with Otto Lummer. Born had both studied mathematics with Hilbert and learned to do experiments with black-bodies under Pringsheim and was hence the most appropriate mediator.[113] Born returned to Göttingen few days after his meeting with Pringsheim. Apparently as a reaction to these discussions Hilbert decided to publish an extended version of his paper in the journal of the *Association of the German Mathematicians*.[114]

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[109] Cf. Kayser, *Handbuch* (ref. 7) p. 27 (The proof is sketched on pp. 37-38.) and Wien, *Theorie* (ref. 39), here p. 285.

[110] Carathéodory to Hilbert, Dec. 12, 1912, Cod. Ms. D. Hilbert 55, item 4.

[111] Cod. Ms. D. Hilbert 55, item 4. A careful reading of Hilbert's publication reveals that this criticism is not one of his argument but of his assumptions, since Hlbert had explicitly required that there were no heat conduction. Hilbert, *Begründung 1913* (ref. 89) p. 2

[112] Born to Hilbert, Jan. 7, 1913, Cod. Ms. D. Hilbert 40A, item 4.

[113] Born deserted, however, soon from Lummer's and Pringsheim's laboratory after he had caused some damage to instruments that made Lummer "furious" at him, Pringsheim was, however, only "mildly annoyed." Born, in retrospect, characterized Pringsheim as a "quiet thinker, elegant in manners and attire, cautious and reserved in his statements, modest and unobtrusive." Max Born: My life, New York 1978, 125 and 123, resp.

[114] Hilbert, *Begründung 1913* (ref. 89).

The new part is marked by a footnote referring to Pringsheim's 1903 version of his proof. Carathéodory had pointed Hilbert's attention to this paper in his letter and had suggested that although much could be said against his proof, the guiding idea may be usable. As we have noted before Ewald had analyzed already Pringsheim's proof for Hilbert but possibly had overlooked the probably a bit more suggestive presentation from 1903. In any case, Hilbert stated that this paper had been news to him and motivation to append some pages that compare different approaches of proof. While he made explicit that his specific integral equation crucially depended on his assumptions, he did not refer to Pringsheim's objection that Carathéodory had told him (inclusion of heat conduction), but he moreover unveiled that the proper integral equation would be at stake in the same way also when one did not require equilibrium for each wavelength separately but only for the total energy. Before we turn to Hilbert's view of Planck's proof, we will first go on with Pringsheim's response to Hilbert's formalization of his account. While Carathéodory told Hilbert that with the amended paper the question now appeared to him fully clarified and "must satisfy every physicist," Pringsheim at the same time was composing a harsh critical article against Hilbert's approach.[115]

Pringsheim's "Remarks on a paper of Mister D. Hilbert..." appeared in *Physikalische Zeitschrift* in April 1913.[116] What Pringsheim annoyed so much was that Hilbert in his paper on radiation theory in the same journal had claimed the only reasonable proof of Kirchhoff's law. Only in the amended version published in the journal of the *German Association of Mathematicians* he had pointed out that his discussion was intended as an axiomatic treatment and later he would even claim that from his Münster talk on radiation theory "a presentation of this discipline can be directly drawn, that satisfies the modern [neueren] requirements of axiomatic treatment after the model of geometry."[117] In this axiomatic way he made now clear the different approaches of Pringsheim, Planck, and himself. As his main axiom was the requirement of separate equilibrium for each color, Planck's he saw basically in the local determination of the coefficients, while Pringsheim's were the postulate of the existence of matter for each given absorption function. In essence Hilbert then demonstrated that while his axiom would clearly suffice to derive the law, Planck's would fail as well as Pringsheim's. Only the latter two axioms combined would also do the job.[118]

In his first article criticizing Hilbert, Pringsheim raised three points: First, he deprived Hilbert's derivation of its generality:

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[115] Carathéodory to Hilbert, April 4, 1913, Cod. Ms. D. Hilbert 55, item 5.

[116] Ernst Pringsheim: Bemerkungen zu der Abhandlung des Herrn D. Hilbert "Begründung der elementaren Strahlungstheorie", Physikalische Zeitschrift, 14 (1913) 589-591, dated April 15, 1913.

[117] David Hilbert: Bemerkungen zur Begründung der elementaren Strahlungstheorie, Nachrichten von der königlichen Gesellschaft der Wissenschaft zu Göttingen (math.-phys. Klasse) 1913, 409-416 and Physikalische Zeitschrift 14 (1913) 592-595, here on p. 592. On geometry as a model for axiomatic treatment of physics cp. also footnote 132 below.

[118] Hilbert, *Begründung 1913* (ref. 89), p. 19.

> ... an equation is derived that coincides formally with Kirchhoff's law, which, however, treats only an ideal and experimentally not realizable limiting case, in which the whole radiation present is monochromatic. The meaning of Kirchhoff's law, however, lies in the fact that in the only physically interesting case of mixed radiation, that consists of oscillations of infinitely many different frequencies, for each single frequency Kirchhoff's law is satisfied.[119]

As a consequence Hilbert's claim quoted above that the law were a "deep mathematical truth" only revealed by the tool of integral equations was denied. In Pringsheim's eyes he missed the proper "meaning" of Kirchhoff's law. It is interesting to see that Pringsheim employs the argument on the experimental feasibility here; his style of experimental thinking was, hence, still at work.

Pringsheim criticizes next, that for a physicist Hilbert's axiom were far too fundamental that any physicist would have "tacitly assumed" it, moreover it is rather already what should be proven:

> Therefore is the content of axiom 1, which according to Mister Hilbert is the basis of his derivation of Kirchhoff's law, physically equivalent to Kirchhoff's law.[120]

The problem raised here is, clearly, the difference between "physical equivalence" and the work to relate the two in this way equivalent statements mathematically. Pringsheim also rehashed further criticism in line with the experimental thinking style (besides the statement, that it were experimentally not possible that all radiation would be monochromatic): Commenting on a certain step in an indirect proof by contradiction where Hilbert fixed variables for the sake of simplicity, this was for Pringsheim physically the same as to postulate a body which absorbs without dispersion that cannot exist.

And, third, Pringsheim commented directly on Hilbert's axiomatization of his approach. He could not see that Hilbert's axiom III would meet his assumptions. In particular he had always considered radiation of an extended body surrounded by empty space and not within a medium.[121]

The reader of Pringsheim's "remarks" only had to turn the page in the *Physikalische Zeitschrift* to read Hilbert's "remarks" that were, however, on his own paper and not Pringsheim's critique. This was only addressed in a brief footnote in the middle of this paper by simply pointing out that Pringsheim's objections appeared to him in no way justified.[122]

How should they? Let us mention two points. First, in an axiomatic framework all conclusions are already in the axioms, otherwise they could not logically be deduced.

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[119] Pringsheim, *Bemerkungen* (ref. 116), p. 589.

[120] Pringsheim, *Bemerkungen* (ref. 116), p. 590.

[121] Pringsheim, *Bemerkungen* (ref. 116), p. 591.

[122] Hilbert, *Bemerkungen* (ref. 117), p. 235. Hilbert's paper was received by the journal three weeks after Pringsheim's critique; apparently Hilbert was asked to respond to it by the editors.

Thus Pringsheim's reproach of equivalence does not score, from the mathematical point of view. And, second, is it really necessary within a mathematical calculation of a physics problem to check for each step whether it corresponds to a physical existing situation? As much as such a requirement obviously corresponds to the experimental thinking style, the mathematical validity of an argument cannot depend of the (intermediary) ontology in this way adjoined.[123]

On the other hand it becomes clear that for Hilbert the main point in Kirchhoff's law cannot have been the problem of spectral composition as it was initially for Kirchhoff and later for Pringsheim. For him it was rather the validity of the relation for arbitrary media with varying physical properties, as it had been before for Kirchhoff to some extent and definitely for Planck. The latter had solved the decomposition question on few lines but dedicated many pages on various different cases of the arrangement of media. In a way also Pringsheim realized that his view was nearer to Kirchhoff than to Planck, who had already some common ground with Hilbert:

> Following Planck's lead Mister Hilbert considers the radiation within an absorbing substance and talks about the absorption coefficient as about a function of the space coordinates, I, however, following Kirchhoff treat the radiation in the empty space and consider the absorbing power of an extended body, ...[124]

Pringsheim was even more annoyed after he had read Hilbert's brief footnote with the wholesale repudiation of his criticism. He sent a second statement against Hilbert to the journal in July 1913 that now turned criticism into polemics and half-truths. He first tried to establish that Hilbert only turned to the axiomatic point of view in a tactical move and not for inner reasons. Hilbert had done this only after Carathéodory had communicated to him on Pringsheim's instigation "that in his alleged derivation of Kirchhoff's law the essential physical content of this law" were "tacitly assumed."[125] This argument some physicist would probably have bought, any mathematically minded scientist, however, must have recalled how much Hilbert stood for axiomatics and with reference to his 1900 Paris address, his aim of an axiomatization of physics was no secret. Pringsheim than turned to the applicability of the axiomatic method to physics in general, thus to the core of Hilbert's motivation in the first place to deal with physics at all. Reiterating his point that Hilbert's derivation were bound to very special conditions (single wavelength), he mentioned that one "had to conclude that, strictly speaking, even all five axioms of Mister Hilbert together were not sufficient in order to derive Kirchhoff's law generally." Furthermore, if everything Hilbert needed for his derivation should be put into axioms one would end up with far to much axioms: "I believe" he wrote

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[123] A nice parallel can be drawn to the problem in the history of calculus to accept imaginary numbers, i.e. not *real* quantities, in intermediary steps of solving higher order equations in the 16th century.

[124] Pringsheim, *Bemerkungen* (ref. 116), p. 590.

[125] Pringsheim, Ernst: Über Herrn Hilberts axiomatische Darstellung der elementaren Strahlungstheorie, Physikalische Zeitschrift, 14 (1913) 847-850, here p. 847.

> ...that we always will arrive at this difficulty when we try to found a physical discipline axiomatically. For this reason, I believe, that physics is no appropriate field for the axiomatic method.[126]

Thus for Pringsheim Hilbert's 6th problem was nothing but a fictitious problem. He concluded in a manner not in line with the personality he otherwise exhibited:

> Therefore all conclusions Hilbert draws from his axioms appear to me partly incorrect, partly physically meaningless.[127]

Picking Hilbert's work into pieces was certainly no pleasant work, he added, but done to prevent his fellow physicists to take Hilbert's errors as truth due to the high standing Hilbert's name would enjoy "in the mathematical world."[128]

On learning a lesson: The Hilbert-Planck exchange 1912-13

The shortcoming of Hilbert's treatment could have easily been cured by Planck's concise argument mentioned above, that in an isotropic medium and thus also in the vacuum one wavelength must not gain on the expense of another. (For Pringsheim this was already the proof of Kirchhoff's law.) But it was exactly this argument for which Hilbert criticized Planck in a footnote of the enlarged version of his Münster talk. Hilbert actually called it a gap in Planck's proof.[129] The exchange between Hilbert and Planck, however, developed completely differently from that of Hilbert and Pringsheim. It started when few days after the 1912 meeting Hilbert sent Planck a reprint of the paper his talk was based on. Planck answered on a postcard in October that since the production of the second edition of his book on the theory of heat radiation were too far advanced he, unfortunately, could not to take into consideration his "interesting method."[130] In two more letters of October 1912 and January 1913, which mainly dealt with Planck's participation in a Göttingen congress Hilbert was organizing, Planck came to the defense of his pupil Max Abraham. His treatment of black-body radiation in his textbook on *Electromagnetic theory of radiation* of 1905 (second revised edition 1908) had been criticized by Hilbert in his teaching and in a letter to Planck. But the point raised was again no central one in the proof of Kirchhoff's law.[131]

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126 Pringsheim, *Darstellung* (ref. 125), p. 848.

127 Pringsheim, *Darstellung* (ref. 125), p. 849. For a characterization of Pringsheim's personality cp. Born in ref. 113.

128 Pringsheim, *Darstellung* (ref. 125), p. 849 f.

129 Hilbert, *Begründung 1913* (ref. 89), footnote 1, p. 18.

130 Postcard Planck to Hilbert Oct. 4, 1912, Cod. Ms. D. Hilbert 308A, item 1.

131 Max Abraham: Theorie der Elektrizität. Bd. 2: Elektromagnetische Theorie der Strahlung, Leipzig 1905, 2nd ed. 1908. Letters Planck to Hilbert Oct. 20, 1912 and Jan. 24, 1913, Cod. Ms. D. Hilbert 308A, item 2 and 3. Planck explained to Hilbert that equation (227) on p. 340 of the second edition of Abraham's book is a

Only after Hilbert had criticized Planck himself, the latter commented on shortcomings of Hilbert's approach. Hilbert's criticism was based on an argument typical for the mathematical style: He constructed a solution for a and h, and q that satisfied Planck's axiom (as had defined it), that, however, did not obey Kirchhoff's law.[132] Consequently, there must be as gap in Planck's derivation, Hilbert concluded. This approach, however, did not help at all to determine where the gap were, and Hilbert only conjectured that Planck's expense argument possibly could not be maintained in the general case of inhomogeneous medium or an arrangement of bordering homogeneous media.[133] Planck in turn maintained that it were in fact possible "to proceed step-by-step to the general case of arbitrarily bordering homogenous media" without any need to use Pringsheim's axiom III; one could simply do with the laws of reflection and refraction, which were an "essential merit" of his approach. Now turning to Hilbert he noted in concluding his letter:

> The physical significance of your method of proof I merely can see in its application to inhomogeneous media. But on the other hand here the difficulty arises that in reality in such media the propagation of energy is not determined by the principle of fastest arrival you use since determinate light paths do not exist at all, but rather "diffusion" of light occurs that neither you nor Pringsheim take into account.
> I would be very pleased if you could tell me your view on these points. For, I would rather not give the impression to the outside as if I agreed with your view as it is published.[134]

Hilbert must have responded to this letter without changing his views much[135] since some days later Planck put forward his criticism in much detail again. And this letter is most interesting. Planck first turned the tables on Hilbert, who should better take care of his gaps:

> In your "proof of impossibility" I see a gap in the fact that your equation (26) does not by far comprises the content of my axioms.
> The essential ones are the following:
> 1. In an arbitrarily limited body with finite absorptive and emissive powers for each temperature *a single* state of thermal equilibrium is possible (maximum of entropy and minimum of free energy, resp.).

simple consequence of the second law of thermodynamics, which is employed to further restrict the possible functional form of the radiation formula. Further criticism on Abraham Hilbert made in his lecture course (cp. footnote 59).

[132] This technique of constructing counterexamples is a typical ingredient of Hilbert's axiomatic method. Cp. his demonstration of the independence of the parallel axiom in Euclidean geometry in: David Hilbert, Grundlagen der Geometrie, (Festschrift zur Feier der Enthüllung des Gauss-Weber Denkmals in Göttingen), Leipzig 1899, many editions, reprints and translations, ch. II, § 10.

[133] Hilbert, *Begründung 1913* (ref. 89), p.18.

[134] Letter Planck to Hilbert April 4, 1913, Cod. Ms. D. Hilbert 308A, item 4.

[135] Hilbert acknowledged the criticism regarding the velocity of propagation in his third communication, Hilbert, David: Zur Begründung der elementaren Strahlungstheorie (Dritte Mitteilung), Nachrichten von der königlichen Gesellschaft zu Göttingen (math.-phys. Klasse) 1914, 275-298, here p. 277; also in Physikalische Zeitschrift 15 (1914) 878-889 and Gesammelte Abhandlungen, vol. 3, p. 238-257.

2. h, a, and q depend only of the nature of the matter (your axiom II.)[136]

The main point Hilbert missed in axiomatizing Planck's proof were the first, which actually determined already the radiation for each wavelength since in equilibrium each characteristic quantity is determined by the temperature, in particular the function for the radiation density.

The important observation that we will draw from Planck's letter is, that by stating his two essential axioms ("my axioms") and in arguing in terms of gaps, Planck explicitly accepted the axiomatic treatment as the arena to settle the dispute. Hilbert, though not very lucky with his technical arguments, could have celebrated at this point his winning over of Planck to the axiomatic method. But was Planck really converted to the new faith? Planck scored, too, as he convinced Hilbert of the validity of his treatment: Hilbert wrote in his second paper on radiation theory in a footnote that he completely retracted his criticism. Planck, however, only succeeded by adapting to Hilbert's style of reasoning which was the axiomatic treatment.[137]

What merits are left from Hilbert's approach, didn't he presuppose Kirchhoff's law in its original version and only extended it for inhomogeneous media? Hilbert himself gave later a rather clear characterization for both his aims and results. It were necessary to treat the radiation theory with the axiomatic method, he noted,

> ...because this theory was the only one of the older physical theories, that had not undergone such an analysis until then and therefore was stricken with unclarity both in the forming of notions and in the way of proof.[138]

In this sense his treatment goes back to earlier axiomatizations of physical theories like those he discussed in his 1905 lectures.[139] He, however, held back the other, possibly even more important, motivation that we first found in Minkowski's statement and which determined Hilbert's research political actions of the years from 1911 on: the expectation that modern physics would provide fruitful new problems for mathematics and in this way would heighten range and prestige of his discipline.[140]

Concerning the results of his approach, Hilbert pointed to the following:

> One of the most noteworthy results of my first communication lies in the fact that the statement, the ratio $q^2 h / a$ has for each point of a system in thermal equilibrium the same value, can be

136 Letter Planck to Hilbert April, 15, 1913, Cod. Ms. D. Hilbert 308A, item 5.

137 Hilbert, *Bemerkungen* (ref. 117), p. 593.

138 Hilbert, *Begründung 1914* (ref. 135), p. 275.

139 Hilbert taught in the summer term on "Logical principles of mathematical thinking." The lectures were worked out both by Max Born and Ernst Hellinger and treated the axiomatization of mechanics, thermodynamics, kinetic theory of gases, electrodynamics, and psychophysics among others. These lectures have been studied extensively in: Corry, *Hilbert 1997* (ref. 86).

140 Cf. Schirrmacher, *Establishment* (ref. 86).

> inferred from an integral equation, without that any *transport* of matter or *change* of its physical nature has to be carried out for the proof, like this is otherwise always done in the course of the proof of Kirchhoff's law.[141]

Thus Hilbert himself had already realized that his style of reasoning was completely free of the physicists' experimental thinking style and for him it obviously meant a major advance to dispense with thinking in experimental terms.

The debate on the proof of Kirchhoff's law in general and the exchange between Planck and Hilbert in particular provides a good example of how the mathematization of physics developed and how it came to an (temporary) halt. As much as Hilbert succeeded methodologically, namely, he made Planck argue in an axiomatic way and only by this the discrepancies of the various proofs could be made obvious, physicists like Planck still did not embrace this method of thought. Only then we can understand that Planck who wrote in the same letter from April 15, 1913, first of "my axioms" and then continued by the statements that certain axioms were, however, simply "inappropriate for the foundation of a proof of Kirchhoff's law." That one Hilbert had started from were "completely arbitrary," and Pringsheim's assumption of the existence of a continuous sequence of materials with respect to certain physical properties were "strictly speaking clearly wrong." Nonetheless, and to Hilbert's astonishment, Planck added that Pringsheim's proof were in his opinion still not only the "simplest and most transparent way of proof" but also "factually the most profound" since it derived the law from its real root that was the second law of thermodynamics. This contradictory finding must have encroached Hilbert's willingness to follow physical argumentation and the claim, by which Planck put himself in line with his teacher, that this view was already the opinion of Kirchhoff who only made his proof "such complicated" because he wanted to make it independent of this shaky assumption on our imperfect material world, will have exhibited very clearly to Hilbert that Planck was lost for the project to bring to bear the axiomatic method in modern physics. It was quite clear that the physicists stood together on this issue and were willing to reinterpret their heritage the way they liked.

From our point of view Max Planck's specific ability seems to have been both in the case of Kirchhoff's law as well as in the case of his own radiation formula, to cultivate a certain approach that appears to lie somewhere in the middle of Kirchhoff's purely experimental style of thinking and Hilbert's purely mathematical style of reasoning, or rather superposes elements of both. It combined the reasoning from general principles with the procedures of stepwise generalizations. No matter whether we call this approach a conciliatory style, intermediary style, or transition style (from the point of view that there were a constant and gradual mathematization process at work), it was obviously highly appropriate for the physics of this period. When it later came to the foundations of quantum mechanics matters, however, would have shifted further into Hilbert's direction.

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[141] Hilbert, *Begründung 1914* (ref. 138), p. 276.

The retraction of the criticism of Planck's argument and Pringsheim's polemics from July 1913 were not the last word on this issue. This had Hilbert almost one year later. In his "third communication" on the foundation of radiation theory he dealt with Pringsheim in an almost one-page long footnote insisting on the correctness of his formulations and on Pringsheim's misunderstandings. One point, however, that also Pringsheim had criticized was now analyzed in great detail, the problem of intrinsic reflection. Hilbert had made Wilhelm Behrens tackle this problem who actually qualified for the *Privatdozent* under his guidance with this work in November 1913.[142] So, Hilbert gave in this paper "under rigorous consideration of reflection new and elementary proofs of Kirchhoff's law" and also solved the question "of equal importance" of the freedom from contradictions of the axioms among themselves and the laws of optics.[143] While the axioms remained basically the same (characterizing his, Planck's and Pringsheim's approaches), the proof now employed a different strategy.[144] The central integral equation (see p. 30) had in particular lost its indispensability and did not play a major role anymore. In doing so Hilbert integrated Planck's and Pringsheim's work as well as his former papers finally into a satisfactory analysis of their relation.

While the section on the proof of Kirchhoff's law in Hilbert's third communication could still be seen in line with the above identified purely mathematical style of reasoning that we found characteristic for Hilbert, the demonstration of the freedom from contradictions means a complete turn. To give an example, in the section on the compatibility of the axioms with the elementary laws of optics the following descriptions of thought experiments can be found:

> Now we imagine that the plane e and the points A, A' are fixed ... and rotate the system in such a way that...
> (...)
> We now imagine, that the space on the one side of the plane e is filled with a substance with optical coefficients q, a, h, and on the other side with a substance with optical coefficients q_*, a_*, h_*; furthermore a ray form O meets the plane e at A and is then refracted to B and reflected to C.[145]

[142] Wilhelm Behrens: Lichtfortpflanzung in parallel geschichteten Medien, Mathematische Annalen, 76 (1915) 380-430. Behrens wrote in the introduction that this work, that showed how the laws of radiation theory can be derived from Maxwell's theory in approximation, were motivated by Hilbert's publications, p. 382 f.

[143] Hilbert, *Begründung 1914* (ref. 138), p. 276 f.

[144] The coefficients are taken as functions of parameters p that describe the physical nature of the matter, $a(p)$, $h(p)$, $q(p)$, and it is shown that differentiation with respect to this parameters vanishes for the combination q^2h/a .

[145] Hilbert, *Begründung 1914* (ref. 138), p. 291.

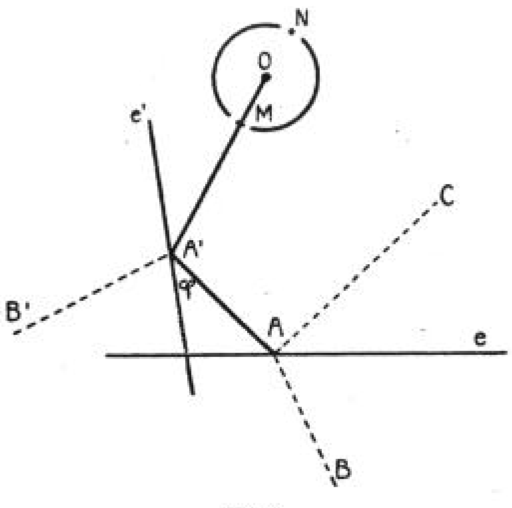

FIG. 4 Drawing used by Hilbert used to illustrate an argument for demonstrating the independence of axioms.[146]

Although Hilbert tried to keep the door of the experimenter's workshop shut, he at this stage obviously took over much of Planck's style with some relation to experimental thinking. There is even one ironic instance, though not characteristic for the whole paper, where Hilbert in a sense revives Kirchhoff's initial postulate of a one-wavelength plate: He considers an axiom D'' (which is a variant of Pringsheim's) that postulates the existence of substances that reflect all radiation except for a single wavelength.[147]

Hilbert had temporarily become a theoretical physicist. His teachings were no longer advertised with titles like "mathematical foundations of physics" (summer 1912 and winter 1912/13) or "seminar on the axioms of physics" (1912/13), but simply as "theory of electron movement" (1913), "electromagnetic oscillations" (1913/14), "selected topics of statistical mechanics" (1914) etc.[148] This, however, only lasted until 1916 when the "principles of physics" meant the foundations of Einstein's theory of general relativity. It was in fact Einstein who in 1916 also put forward a new understanding of emission and absorption in the context of quantum theory. Pringsheim died in 1917 and according to the obituary of one of his colleagues he had stopped the quarrel though the arguments of his opponent had not convinced him for all his life.[149] What Hilbert had learned in these years was that the "inspirations that flow into mathematics from the side of physics"

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[146] Hilbert, *Begründung 1914* (ref. 138), p. 290.

[147] Hilbert, *Begründung 1914* (ref. 138), p. 297. See also ref. 36.

[148] Cp. Verzeichnis der Vorlesungen auf der Georg-Augusts Universität zu Göttingen.

[149] Clemens Schäfer: Ernst Pringsheim, Physikalische Zeitschrift, 18 (1917) 557-560, here p. 559.

can only be gained in first doing physics the physicists' way. He came to acknowledge the legitimacy of Planck's style and realized the problems with axiomatization at work.

ON HAVING LEARNED A LESSON: EINSTEIN AND THE FATE OF A PROTO QUANTUM PROBLEM

There were few authorities Hilbert had not challenged at one point or other and the dispute that has attracted most attention by historians of science is the (alleged) priority dispute with Einstein on the correct Lagrange function for general relativity.[150] It was actually Hilbert's next publication in the Göttingen Royal Society's journal after his third paper on radiation theory, which gave rise to a certain resentment between him and Einstein. Hilbert intended

> ...to present a new system of fundamental equations of physics in the sense of the axiomatic method, that are of ideal beauty and that contain ... the solution of the problems of Einstein and Mie at the same time.[151]

So by November 1915 Hilbert was back to his axiomatic approach in physics but he also had learned some lessons from his exchanges with Pringsheim and Planck. Probably for these experiences he never claimed priority over Einstein.[152] From our analysis of the discussions on radiation theory it may follow that also in the Einstein-Hilbert dialogue both the question of the difference in the understanding of the problem as well as the aims, and the dfference of reasoning styles are possibly more substantial than the decision over priorities. Rather than being in a direct competition Hilbert and Einstein had different aims and followed different paths in their researches that met at one point which for Einstein meant a major breakthrough for general relativity, for Hilbert, however, only a certain step in a program of a unified theory of matter.[153]

Returning to radiation theory, it is also due to Einstein that the disputes on the proof of Kirchhoff's law evaporated. His 1916 paper on "Radiation emission and absorption according to quantum theory" finally let the 19th century proto quantum problem of the proof of the relation between absorption and emission disappear. On the basis of Planck's oscillator model, Einstein defined emission and absorption coefficients A_m^n and B_m^n as the probabilities that in the course of a transition from energy level m to n

[150] Cf. in particular Leo Corry, Jürgen Renn and John Stachel: Belated decision in the Hilbert-Einstein priority dispute, Science, 278 (1997) 1270-1273, and Tilman Sauer: The relativity of discovery: Hilbert's first note on the foundations of physics, Archive for the History of Exact Sciences, 53 (1999) 529-575.

[151] David Hilbert: Die Grundlagen der Physik (Erste Mitteilung), Nachrichten von der königlichen Gesellschaft der Wissenschaften zu Göttingen (math.-phys. Klasse), 1915, 395-407, here p. 395.

[152] Actually the first reference in Hilbert's paper (ref. 151) was to Einstein's paper with the correct Lagrangian.

[153] Cp. also David Rowe: Einstein meets Hilbert: At the crossroads of physics and mathematics, Mainz Mathematik Preprint 8/2000 to appear in Physics in Perspective.

radiation with the energy $hn = E_m - E_n$ is emitted and the absorption of such an energy quantum gives rise to the change of state, resp.[154] In terms of these coefficients, however, no simple equivalent exists to Kirchhoff's law. A universal function cannot be found, as the theory of thermal equilibrium cannot fully be recovered in the quantum description.[155] In this sense Einstein threw away the ladder Planck had climbed.

CONCLUSION

In the introduction we have raise three main points to which we now return: the history of a law with its development in content and foundation, the determination of Planck's position within the various approaches to the radiation law, and the different approaches or styles applied to this example of theoretical foundation.

Our history of Kirchhoff's radiation law has exhibited a full cycle from the birth to the dissolution of a physical law. As its birth was in the context of colored flames and radiating star atmospheres –phenomena to which the abstracted law did no longer apply–, it vanished with the application of a theory to emission and absorption phenomena, that it had itself given birth to: the quantum theory that had developed out of Planck's determination of Kirchhoff's universal function. The quantum theory of radiation emerged as a new research field. It is interesting to see, how the scientists involved in our story ousted its results from their memories. Max Born, for example, who in 1913 took over Hilbert's criticism of Planck's proof and who told his students ten years later still that Hilbert had given the rigorous mathematical proof of Kirchhoff's law (although using Pringsheim's idea), rehabilitated Kirchhoff in 1929 fully, as the scientist who had "on the basis of indisputable thermodynamical conclusions proven, that radiation of the interior or a glowing oven that comes out of a small hole must have a spectrum of universal kind..."; he did later not comment on the whole issue in his autobiography at all, nor did Hilbert's biographer.[156] Planck had already fixed his view in the letters to Hilbert in 1913 while Sommerfeld, who after World War II in his last years of life finally condensed his legendary teaching into a number of textbooks, saw no problem in essentially going back

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[154] Albert Einstein: Strahlungs-Emission und Absorption nach der Quantentheorie, Verhandlungen der Deutschen Physikalischen Gesellschaft, 18 (1916) 318-323, on p. 321.

[155] Agassi, *Kirchhoff-Planck* (ref. 19), p. 36. It is rather by accident that classical (or semi-classical) laws like Rutherford's scattering formula can be recovered unaltered after quantization.

[156] Max Born: Die Theorie der Wärmestrahlung und die Quantenhypothese, Naturwissenschaften, 1 (1913) 499-504, on p. 501; Kinetische Theorie der Materie (winter term 1922/23) worked out by Luise Spieker, Göttingen Mathematical Institute, p. 170; Über den Sinn der Physikalischen Theorien, Naturwissenschaften, 17 (1929) 109-118, on p. 114; Born, *Life* (ref. 113); Constance Reid: Hilbert, New York 1970.

to 1859 when he simply argued with the use of a filter transparent for one wavelength only.[157]

During this life cycle of Kirchhoff's law, however, neither the statement of the law nor its foundational roots remained constant. Rather, a number of different interpretations and foundations were found, that, though often coexisting at the same time, still exhibit a distinct development of new understandings and new identifications of the issues physicists and mathematicians felt obliged to prove.

Development of the content of Kirchhoff's law in relation to objects and tools used in proof

proof by	**on what relation**	**"for each l"-problem**		**"in arbitrary medium"-problem**		**other issues**
		objects	**tools**	**generality**	**tools**	
Kirchhoff 1859	e/a	one-l-plate*		–		
Kirchhoff 1860/62	e/a	diatherm. plate*	Fourier theory	homogeneous medium		
Helmholtz	e/a	prism*		–		
Pringsheim	$u(l)$	cavities	∫-argument*	–		
Planck	e/a	–	expense argument	piecewise homogeneous media		
Hilbert 1912	e/a			general inhomogeneous medium	∫-equation	
Hilbert 1913	e/a			general inhomogeneous medium	∫-equation	axiomatic method
Hilbert 1914	e/a			general inhomogeneous medium	differential argument	axiomatic method
(Einstein)	$E_n \rightarrow E_m$	oscillator...		–		
Sommerfeld	$u(l)$	l-filter*		–		

* not tenable assumptions or arguments

The table summarizes our findings concerning the objects of proof. It indicates that the statement Kirchhoff initially wanted to prove, the "for each wavelength"-problem, and the statement Hilbert later established, the "in an arbitrary medium"-problem, had no overlap in content, though both were called Kirchhoff's law. And we also find that the instruments used, i.e. both the real or conceived objects and the mathematical tools, have

[157] Arnold Sommerfeld: Thermodynamik und Statistik (Vorlesungen über theoretischen Physik, Band V), Wiesbaden 1952. p. 131 f.

also changed this much that there is no common ground even among Kirchhoff and Planck.

We have not only seen in the case of Hilbert that he changed his interest in and his approach to physics, but also for Kirchhoff we saw very clearly how his own typical style of doing theoretical physics (or at least the phenomenological approach attributed to him) was modified due to the tenacity of the problem. When we have deliberately spoken of styles of thinking and styles of reasoning in our analysis these must consequently not be taken to identify *personal styles* that could capture a scientist's specific way of treating all his problems in a certain constant manner, but rather his approach, techniques, and mental models applied to a specific field or set of related problems. These styles that we tried to distinguish in this study rather emerged in an interaction of personal approach, e. g. as it had developed in a scientist from his study within a discipline, school, or tradition and the treatment of previous problems, with the very nature of the field or set of problems considered.

In the case of classical radiation theory we were basically able to distinguish four styles of reasoning in the proofs, that to great extent can be characterized as *disciplinary styles* that relate the ways scientists establish a physical law to the different disciplines or different stages within a disciplinary development they belong to. When we add the often praised phenomenological style we get five *disciplinary styles of foundational reasoning*. They in particular put Planck's work on the correct radiation formula into context.

(P) *The phenomenological style* (Neumann, Kirchhoff). This influential and possibly mainly German style was a leading paradigm first in mechanics and later in electrodynamics. The phenomenological view, however, failed in its pure interpretation in the radiation problem.

Kirchhoff's approach to radiation theory deviated from the main rules of the phenomenological method he generally stood for as a prototype. The subject matter required concessions. Cotton and Pockels justified these with reference to a higher good, the "suggestive value" leading to the discovery of new facts and laws, and the simplificatory power of fictitious bodies, resp.[158]

(E_1) *The thought experimental style.* The intricate thought experiments of Kirchhoff and Helmholtz correspond to the principles of experimental physics of exhibiting nature's behavior most explicitly in prepared concrete situations that are recorded and analyzed. The resulting foundational style of reasoning most strikingly exhibits a case of experimenting theory.

[158] Pockels, *Kirchhoff* (ref. 66), p. 256 f., Cotton, *status* (ref. 7), p. 267.

With Pringsheim a second type of experimental theorizing was found. Even more than Kirchhoff with his mathematical skills and Helmholtz (as an outspoken empiricist), Pringsheim brought the influence of real experimentation to the fore:

(E_2) *The style of real experimentation.* The experiments that provided Pringsheim's argument did not rely on idealizations and improperly conceived objects but actually could be performed in the laboratory. (They, clearly, need not for the sake of proof, as the argument is logical, not empiric.) This together with its dispensing with idealizations is the clearest distinction to this first experimental style.

Planck had no relation to experimentation but rather decided to become a theoretical physicist at a time theoretical physics was not yet a separate field. For these reasons he was not a follower of the phenomenological school, which was deeply rooted in experimentation. As an independent worker in particular in the field of thermodynamics he was rather unimpressed by Kirchhoff and Helmholtz who he acknowledged as authorities. His main pillars for the physics building were rather general principles and universal constants. Physical knowledge should be independent of human actions and thus also manipulations in experiments. But Planck was still a physicist used to the stepwise attack of complex and complicated physical phenomena: first solve a simple paradigmatic case employing the main principles, then generalize to more complex and hence more natural or real cases. His distance from experimentation, however, was a factor that turned out favorable in the case of radiation theory.[159]

(C) *The conciliatory style*, hence, combines aims of the phenomenological style (economic description of observable phenomena without reference to special particularly microscopic imaginations) with the application of general principles. The results are approached in step-wise generalizations when necessary.

The specific task Hilbert saw for a mathematical physics was to remove all unnecessary physical ornament from the discussions in order to identify physical assumptions (axioms) and to transform the deduction of the law into a purely mathematical problem. His axiomatic approach in which no objects but only mathematical formulae had to be manipulated in such a way that first the most general case and its corresponding space of possible solutions was set up and by imposing constraining relations then the actual solutions were identified, did not relate to Planck's use of paradigmatic simple situations to start from (the infinitely extended homogeneous medium for Kirchhoff's law, the oscillator for his radiation function). Hilbert's main interest concerned the question what

[159] A closer comparison with Woldemar Voigt may show this, whose papers on the topic appear often too handicapped by the intricate underlying experimental situation in order to arrive at conclusions of generality. Cp. e.g. Woldemar Voigt: Über die Proportionalität von Emissions- und Absorptionsvermögen, Annalen der Physik, 67 (1899) 366-387, and his Allgemeines über Emission und Absorption in Zusammenhang mit der Frage der Intensitätsmessungen beim Zeemann-Effekt (Nach Beobachtungen von C. Försterling), Nachrichten von der königlichen Gesellschaft der Wissenschaften zu Göttingen, 1911, pp. 71-89.

assumptions were sufficient to logically derive a certain statement or law. But also Hilbert became interested in the involved problems of the body of theoretical physics in those years when it was widely felt that the foundations had become shaky.

(M) *The mathematical style*, thus, is the reduction of the proof to a general and purely mathematical problem that can be solved without reference to any contingent physical circumstances by application of established mathematical knowledge.

The concept of style proposed here operates rather on a local disciplinary level and is hence clearly distinct from global styles of scientific thinking like Alastair Crombie's that are rather general modes of thinking found in the whole western scientific tradition.[160] That there is a general polarity between the experiment (or the laboratory) and the rationality and methodology of mathematics is a point raised in Ian Hacking's discussion of styles of (scientific) reasoning. Of the four basic styles Hacking discusses, two of them capture part of our findings: the mathematical style and the laboratory style.[161] This distinction of two poles, experimentation and instruments vs. mathematics, however is rather crude and makes it difficult e.g. to accommodate Planck's work. Styles for Hacking are ways to explore, structure, and explain the world and they furthermore are seen to determine ontology, objectivity, and rationality:

> The style ends as an autonomous way of being objective about a wide class of facts... It provides new criteria of truth, new grounds for belief, new objects about which there can be knowledge.[162]

As we have seen from the case of radiation theory, there is good reasons to distinguish scientists' ways of reasoning by their different sets of objects used (we can call this the ontology of their approach) and that the debates on these objects are typical for each new style introduced. (See appendix for a more detailed classification.) Truth and objectivity, however, were not at stake at least in a strong sense in our example. Although one can argue that Kirchhoff's initial understanding of the statement of his law and Hilbert's later reading had no overlap in content, it were still not incommensurate views on the same law according to completely different styles of thinking. We have seen that rather a gradual shift in what was considered the central statements of the law occurred in history. Apart from these differences to Hacking the concept of styles employed in this

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[160] Alistair C. Crombie: Styles of scientific thinking in the European tradition. The history of argument and explanation especially in the mathematical and biomedical sciences and arts (3 vols.), London 1994. Cp. also his brief account: Designed in the mind. Western visions of science, nature and humankind, History of Science, 26 (1988) 1-12.

[161] Vicedo, *Styles* (ref. 5) p. 241. Taxonomic and historico-genetic styles do not apply here.

[162] Ian Hacking: Statistical language, statistical truth, and statistical reason: The self-authentication of a style of scientific reasoning, in: The social dimension of science, edited by Ernan McMullin, Notre Dame1992, 130-157, on p. 133.

study shows that it can in the same way be used as a "new analytical tool" by historians and philosophers interested in history and philosophy of justification of scientific laws.[163]

This insight that there are different styles to approach the same statements of physics – and so we return to the historical treatment of our topic–, occurred often already to the scientists themselves. Woldemar Voigt, who at Göttingen in 1912 had to cope with a new physics "colleague," the deeply into physics immersed Hilbert, did not hesitate to comment on this issue in a ceremonial address on occasion of the university anniversary, which he had to deliver in his function of rector of the university. He did not speak of mathematical and experimental styles, however, but on a "left-standing" and a "right-standing direction:"

> With respect to the treatment of theoretical physics two directions can be observed. The first (which I would like to call the left-standing one) is exercised by men who come from the side of mathematics and who occasionally occupy a mathematical chair, in any case who are more or less distant from observations. These men will naturally put the main emphasis on clarity and freedom of contradictions of the foundations, on the consistency of the construction, on the general theorems that can be derived. The other direction (the right-standing one) is represented by men that are at home in experiment and observation, and who accordingly put the main emphasis on the relation of theory with experience as the cause of theory, who hence also accept mathematical derivations of less rigor, if only these impart the understanding of an observed phenomenon.[164]

This lucid characterization actually was given before Hilbert told the physicists about his views on their proofs of Kirchhoff's law.

[163] Ian Hacking: Style for the historian and philosopher, Studies in the History and Philosophy of Science, 23 (1992) 1-20, on p. 1.

[164] Woldemar Voigt: Physikalische Forschung und Lehre in Deutschland während der letzen hundert Jahre (Festrede zur Jahresfeier der Universität am 5. Juni 1912), Göttingen 1912, p. 12 f. The left/right classification that extends to politics, society, and modernity in particular applied to Hilbert and Voigt is worth a separate analysis that cannot be given here. For an unorthodox characterization of the "right-standing direction" cp. Russell McCormmach: Night thoughts of a classical physicist, New York 1982.

APPENDIX: CLASSIFICATION OF STYLE ELEMENTS

In order to clearly distinguish the four different styles of reasoning found in the proofs of Kirchhoff's law we here collect and classify the elements that can be taken as defining for the different styles. The proofs we considered differed in four respects: objects, assumptions, tools, and actions. Concerning the objects we can distinguish real and fictitious ones, the latter either being idealizations or imaginations that do actually contradict the assumed theory. As the following table shows, the styles we found in the proofs, the experimental styles of Kirchhoff and Helmholtz (E_1) and Pringsheim (E_2), resp., Planck's conciliatory style (C), and Hilbert's mathematical style (M), employ different sets of objects with different ontological status. Related to the objects are assumptions on the physical role and determination of the objects. Experimental styles, clearly, often assume mathematical relations without demonstration of its validity. The methodological assumptions also distinguish the different styles clearly. While the mathematical tools are less discriminating, the actions of procedure and of reasoning again exhibit the differences.

There are three ways to view these results. First, one can read off the characteristics for every styles; this we have done to some extent already above. Second, the contextualizaton of Planck can be made more specific. And finally, one can read off the implicit meaning that objects and concepts carry with respect to their status in different styles of reasoning.

Elements of the styles of reasoning in the proofs of Kirchhoff's law

category	*(ontological) status*	*examples found (relating to style)**
objects	typical real	radiating bodies (E)
		diaphragms (E_1)
	special real	line gratings (E_1)
		cavities (E_2)
	general	pure radiation (E_2/C)
		media containing radiation (C/M)
	typical fictitious (idealizations)	(completely) black-body (E/C)
		ideal mirror (E_1/C)
	special fictitious (contradict assumed theory)	one-wavelength-plate (E_1)
		completely diathermaneous plate (E_1)
		completely transparent prism (E_1)
assumptions	physical	mutual irradiation of surface elements (E)
		radiation within (empty) space (E)
		local determination of physical properties (E/C)
	mathematical	reciprocity, unproven (E/C)
		integral argument (Pringsheim, E_2)
		generalization taken for granted (E_1/C)
	methodological	phenomenological treatment (E/C/M)
		reasoning with imaginary bodies has "suggestive value" leading to the discovery of new facts and laws (Cotton, E_1)
		simplificatory power justifies fictitious idealizations (Pockels, E_1)
		hypothesis or axioms must not be "arbitrary" (Planck, C)
		the set of hypothesis or axioms must be independent, in particular not contradictory (Hilbert, M)
tools	mathematical	Fourier theory (E_1)
		symmetry principles (C)
		theory of integral equations (M)
		reciprocity, proven (M)
actions	conceived experimental	replacing objects (E)
		varying parameter (E)
		compare temperature at two points (E)
	of reasoning	stepwise procedure (E/C)
		proof by contradiction (M)
		mathematical necessity (M)

* E_1, E_2 = experimental (E = E_1 and E_2), C = conciliatory, M = mathematical