Conceptualizing Paradigms: On Reading Kuhn's History of the Quantum

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In this article, I discuss the criticisms raised against Thomas Kuhn's *Black-Body Theory*. These criticisms concern two issues: how to understand Planck's position with regards to the quantization of energy in 1901, and how to understand the book's relation to *The Structure of Scientific Revolutions*. Both criticisms, I argue, concern the notion of a paradigm: the first concerns how Boltzmann acted as an exemplar for Planck, and the second whether the book provides a paradigm change. I will then argue that both criticisms presume a conceptualization of paradigms that does not align well with Kuhn's conceptualization of it in both *Structure* and later work: they assume, more specifically, that sharing a paradigm presupposes sharing an interpretation of it, and that paradigm changes are essentially identical to gestalt switches. On the basis of this, I will then argue that the criticisms are misguided, that Kuhn's position regarding Planck's work is in fact quite close to the indetermination-view developed by some of his critics, and that the book fits *Structure* quite well. In conclusion, I will then reflect on how the narrative provided in Black-Body Theory connects with Kuhn's views on the relation between history and philosophy of science.

Keywords: Thomas Kuhn; black-body theory; Max Planck; history of the quantum.

Contents

Introduction

16 years after *The Structure of Scientific Revolutions*[1] (third edition used here)[2], Thomas Kuhn published *Black-Body Theory and the Quantum Discontinuity, 1894-1912*[3] (second edition used here)[4], a historical study of how the quantum entered physics. When the book appeared, the standard view was that in 1901, Max Planck introduced the quantum by conceptualizing black-body radiation in terms of "linear electrical oscillator[s] with energy restricted to integral multiples of the energy quantum hv, v being the oscillator frequency and h the universal constant later known by Planck's

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[1] Kuhn, Thomas S., *The Structure of Scientific Revolutions* (Chicago: The University of Chicago Press, 1962).

[2] Kuhn, Thomas S., *The Structure of Scientific Revolutions Third Edition* (Chicago: The University of Chicago Press, 1996).

[3] Kuhn, Thomas S., *Black-Body Theory and the Quantum Discontinuity, 1894-1912* (Oxford: Oxford University Press, 1978).

[4] Kuhn, Thomas S., *Black-Body Theory and the Quantum Discontinuity, 1894-1912 Second Edition with a new Afterword* (Chicago: The University of Chicago Press, 1987).

name".[5] According to Kuhn, however, "the concept of restricted resonator energy played no role in [Planck's] thought".[6]

Black-Body Theory received two kinds of responses. One, first formulated by Martin Klein in the 1979 symposium on the book,[7] and recently repeated by Olivier Darrigol[8] and Massimiliano Badino[9], states that Kuhn misrepresented Planck's views on resonator microphysics. The second, first phrased by Trevor Pinch in the same 1979 symposium,[10] and recently repeated by Jochen Büttner, Jürgen Renn and Matthias Schemmel[11] and Adam Timmins,[12] comes down to the claim that *Black-Body Theory* goes against the framework provided by *Structure*.[13]

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[5] Kuhn 1987, p. 350.

[6] Kuhn 1987, p. 126.

[7] Martin J. Klein, Abner Shimony, Trevor J. Pinch, and Thomas S. Kuhn, 'Paradigm Lost? A Review Symposium', in *Isis* 70.3 (1979), 429-440.

[8] Olivier Darrigol, 'Continuities and Discontinuities in Planck's *Akt der Verzweiflung*', in *Annalen der Physik* 9.11-12 (2000), 951-960.; 'The Historian's Disagreement over the Meaning of Planck's Quantum', in *Centaurus* 43.3-4 (2001), 219-239.

[9] Massimiliano Badino, 'The Odd Couple: Boltzmann, Planck and the Application of Statistics to Physics (1900-1913)', in *Annalen der Physik* 18.2-3 (2009), 81-101; *The Bumpy Road: Max Planck from Radiation Theory to the Quantum (1896-1906)* (Dordrecht: Springer, 2015).

[10] Klein, Shimony, Pinch and Kuhn 1979.

[11] Jochen Büttner, Jürgen Renn and Matthias Schemmel, 'Exploring the Limits of Classical Physics: Planck, Einstein, and the Structure of a Scientific Revolution', in *Studies in History and Philosophy of Modern Physics* 75.2 (2003), 37-59.

[12] Adam Timmins, 'Between History and Philosophy of Science: The Relationship between Kuhn's *Black-Body Theory* and *Structure*', in *HOPOS* 9 (2019), 371-387.

[13] For other, mostly critical, reviews of *Black-Body Theory* along these lines, see John Nicholas, 'T.S. Kuhn Black Body Theory and the Quantum Discontinuity, 1894-1912', in

In this paper, I will argue that both criticisms assume a conceptualization of paradigms according to which sharing a paradigm presupposes sharing an interpretation of the concepts that figure in it, and changing a paradigm is essentially identical to gestalt switches. On the basis of both *Structure* and Kuhn's later work, I will argue, however, that this conceptualization is misguided: sharing a paradigm does not entail sharing an interpretation of it, and Kuhn himself repeatedly stressed that one should not read too much into the analogy with gestalt switches, particularly since it hides how the scientific community shapes an individual scientist's perspective. I will then argue that if these points are taken into account, Kuhn's characterization of the emergence of the quantum in *Black-Body Theory* aligns quite well with *Structure*, and that Kuhn's presentation of Planck is in fact quite close to the indetermination-position developed by his critics. In conclusion, I will then argue that these criticisms in part arise because they

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Philosophy of Science 49.2 (1978), 295-297; Peter Galison, 'Kuhn and the Quantum Controversy', in *The British Journal for the History of Science* 32.1 (1981), 71-85; and Joseph Agassi, 'The Structure of the Quantum Revolution', in *Philosophy of the Social Sciences* 13.3 (1983), 367-381. More positive reminiscences can be found in Richard Staley, 'On reading Kuhn's *Black-Body Theory and the Quantum Discontinuity, 1894-1912'*, in *Shifting Paradigms: Thomas S. Kuhn and the History of Science*, ed. by Alexander Blum, Kostas Gavroglu, Christian Joas and Jürgen Renn (Berlin: Edition Open Access, Max Planck Institute for the History of Science, 2016), 203-210 and Norton M. Wise, 'A Smoker's Paradigm', in *Kuhn's Structure of Scientific Revolutions at Fifty: Reflections on a Science Classic*, ed. by Lorraine Daston and Robert J. Richards (Chicago: The University of Chicago Press, 2016), 31-41. For a discussion of the book's reception, see Stephen G. Brush, 'Thomas Kuhn as a Historian of Science', *Science and Education 9* (2000), 39-58. For a discussion of how *Black-Body Theory* is to be situated within Kuhn's career, see chapter 7 of K. Brad Wray, *Kuhn's Intellectual Path: Charting The Structure of Scientific Revolutions* (Cambridge: Cambridge University Press, 2021).

do not sufficiently take into account Kuhn’s views on how historical and philosophical texts are to be read, and that as a consequence they have approached *Black-Body Theory* too much as a philosophical work, rather than as a historical one.

Kuhn’s *Black-Body Theory*

Kuhn’s Discussion of the Quantum Discontinuity

When Max Planck turned to black-body physics in 1894, the field was primarily concerned with thermal radiation. This radiation arises when a body, heated to a certain temperature T, emits light and heat. The goal was to formulate an adequate distribution law providing the radiation energy distribution at different temperatures and frequencies v (or wavelengths λ). [14] To investigate this, use was often made of a black body. This is “a cavity with perfectly absorbing (i.e., black) walls, [so that] its interior will be filled with radiant energy of all wavelengths”.[15] The first distribution law generally considered as acceptable was proposed by Wilhelm Wien in 1895.[19]

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[14] At the time, thermal radiation claims were expressed in terms of either wavelength or frequency, with wavelength inversely proportional to frequency. Planck was the first to express such claims in terms of the frequency v instead of the wavelength λ, Clayton A. Gearhart, ‘Planck, the Quantum, and the Historians’, in *Physics in Perspective* 4 (2002), 170-215, p. 176.

[15] Kuhn 1987, p. 3. For a discussion of the experimental study of black bodies, see Dieter Hoffmann, ‘On the Experimental Context of Planck’s Foundation of Quantum Theory’, in *Centaurus* 43.3-4 (2001), 240-259.

[19] For discussions of the theoretical and experimental context in which Wien’s distribution law and earlier attempts were formulated, see Kangro, Hans, *Vorgeschichte des Planckschen Strahlungsgesetzes: Messungen und Theorien der spektralen Energieverteilung bis zur Begründung der Quantenhypothese* (Wiesbaden: Franz Steiner Verlag, 1970) and Badino

Planck turned to black-body radiation because he believed that it could provide more insight into the irreversibility of the second law of thermodynamics, more specifically regarding the question whether the claim that entropy cannot decrease was probabilistic or absolute. On a probabilistic interpretation, it was possible, although highly improbable, whereas on an absolute interpretation, violations of the second law were simply impossible.[20] Planck, one of the few adherents of the absolute interpretation,[21] believed that by conceptualizing black-body radiation in terms of the interaction between electromagnetic resonators and the electromagnetic field,[22] he could show that "the interaction of such a resonator as it came into equilibrium with an electromagnetic field was irreversible, so that using only Maxwell's laws, one would be led to an increase in a suitably defined electromagnetic entropy".[23]

Ludwig Boltzmann soon pointed out, however, that the Maxwell equations by themselves could not provide irreversibility.[24] Planck therefore adapted his approach, modeling it on Boltzmann's derivation of his *H*-theorem in his kinetic gas theory. Boltzmann's derivation relied on a specific assumption called 'molecular disorder', which consisted of a mathematical expression laying down conditions for irreversibility, and the physical hypothesis that all irreversible processes actually obey

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2015, p. 33-38.

[20] Kuhn 1987, p. 25.

[21] Kuhn 1987, p. 28.

[22] For discussions of Planck's use of such resonators, see Kuhn 1987, p. 29-37, Badino 2015, p. 41-45 and Seth, Suman, *Crafting the Quantum: Arnold Sommerfeld and the Practice of Theory, 1890-1926* (Cambridge: The MIT Press, 2010), p. 41-45.

[23] Gearhart 2002, p. 175.

[24] Kuhn 1987, p. 77.

it.[25] This allowed him to prove his *H*-theorem “which demonstrated […] the irreversible approach of a gas to equilibrium”.[26] Planck introduced a very similar hypothesis, called natural radiation, which he conceptualized as follows (quoted by Kuhn):

> If the theory here developed is to be made useful for the general explanation of irreversible processes …, it is above all necessary to bar once and for all by a positive stipulation in advance, all radiation processes which do not display the characteristic of irreversibility. After carrying out this task mathematically [*Addendum by Kuhn*: i.e., finding a mathematical expression of the necessary and sufficient conditions for irreversibility], it is then necessary to introduce the physical hypothesis that all irreversible processes in nature actually satisfy the stipulation under all circumstances. That step will be completed […] by the introduction of the concept of natural radiation.[27]

Given this constraint, Planck could then elaborate an equation linking resonator energy and field intensity, which provided an equation that expressed conditions for when an equilibrium between radiation and field energy at a specific frequency *v* was obtained.[28]

$$\square_{\square}\frac{\square^{\square}}{\square^{\square}}\square_{\square}$$

Planck then proved that the radiation process, conceptualized in terms of resonators interacting with the field in a way constrained by the hypothesis of natural radiation and adhering to the mathematical condition for equilibrium, was indeed irreversible. By introducing a function that, he claimed, provided the electromagnetic entropy of a single resonator, he could then derive Wien’s distribution law.[29]

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[25] Kuhn 1987, p. 67.

[26] Kuhn 1987, p. 352. For discussion of Boltzmann’s approach, see Kuhn 1987, p. 38-71 and Badino 2015, p. 48-51 and p. 65-71.

[27] Kuhn 1987, p. 78.

[28] Kuhn 1987, p. 84

[29] Kuhn 1987, p. 87.

Soon, however, a few issues emerged. First, Planck had provided no proof that his electromagnetic entropy function actually tracked entropy. He had rather "assumed that his [electromagnetic] function […], just because it increased monotonically to a maximum, was *the* thermodynamic entropy", but this assumption was disputed.[30] Second, it was unclear how Planck could justify his assumption that the total resonator energy was divided equally among all individual resonators.[31] Third, after experiments by Otto Lummer and Ernst Pringsheim in 1898 had already suggested results that deviated from Wien's distribution law, new experiments by Heinrich Rubens and Friedrich Kurlbaum in 1900, which indicated systematic deviations, convinced Planck that Wien's law had to be adapted.[32] This led him to a new distribution law, which "has continued to agree with observation ever since".[33] His earlier theoretical derivation still had to be adapted, however, to address the issues raised, in particular concerning the various ways in which the total resonator energy could be distributed. Boltzmann again acted as inspiration for Planck, this time through his "combinatorial argument, [in which he] had divided the total energy of a gas among its component molecules".[34]

To calculate the number of ways in which energy might be distributed among molecules, Boltzmann's theory conceptualized the total energy E as divisible into a finite number P of energy elements of equal size ε (so that $E = P\varepsilon$).[35] An energy distribution would then ascribe to each of the N molecules an amount of energy

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[30] Kuhn 1987, p. 352.

[31] Kuhn 1987, p. 99.

[32] Gearhart 2002, p. 180.

[33] Kuhn 1987, p. 97.

[34] Kuhn 1987, p. 99-100.

[35] While their size had to be fixed for calculations to be possible, the specific size was, for Boltzmann, of no importance, Kuhn 1987, p. 127.

molecules, such that the distribution could be denoted by a list of numbers w_0, w_1, w_2, w_3, …, w_P representing the number of molecules having an amount of energy $0, \varepsilon, 2\varepsilon, 3\varepsilon, \ldots, P\varepsilon$. Such a distribution could be achieved in many different ways, each of which Boltzmann called a complexion. By means of known combinatorial techniques, Boltzmann then obtained the following result:[36]

> Counting the complexions corresponding to a state and dividing them up by the total number of possible complexions gives the probability of that state. Boltzmann proves that the equilibrium distribution is the most probable one, i.e., the one presented by the largest number of complexions.[37]

According to Kuhn,[38] Planck proceeded as follows. He first divided the number of resonators in sets corresponding to their frequency: a set of N resonators with frequency v, a set of N' resonators with frequency v', one of N'' resonators with frequency v'', etc. A particular distribution was then taken to distribute the total energy over these different sets: energy E for the set with frequency v, energy E' for the set with frequency v', energy E'' for the set with frequency v'', etc. His goal was then to find a way to calculate the entropy for such a particular distribution, and to see how its maximum varied with the distribution of energies over frequencies.

To compute the entropy of a particular distribution of energies $E, E', E'', \ldots$, over the sets of different frequencies. Planck had to divide up these different energies into elements of finite size: ε for E, ε' for E', ε'' for E'', etc. In contrast to Boltzmann, however, who could vary element size (see footnote 47), Planck's new distribution law

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[36] Boltzmann took this case, where energy elements of fixed size are divided over individual molecules, to be a fictional one. He immediately showed that it could be made more realistic "by allowing the molecules to take on continuous values of energy", Kuhn 1987, p. 49.

[37] Badino 2015, p. 93.

[38] Kuhn 1987, p. 104-110.

forced him to fix element size to $\varepsilon = hv$, with h a new universal constant and v the frequency of the set of resonators under consideration.[39] Planck then defined a complexion as a list of numbers denoting, for a particular set of N resonators with frequency v and energy E, how many resonators were assigned ε, how many 2ε, etc. After elaborating how to compute the number of ways energy elements could be distributed over one such set, and how this was to be combined with the numbers for other sets of resonators with other frequencies, Planck could then straightforwardly follow Boltzmann in finding the most probable distribution, which was then the equilibrium distribution, i.e. the one with maximum entropy. This provided Planck with a new entropy function, from which he could derive his new distribution law.[40]

Discussions about the early quantum have focused in particular on the fact that Planck was forced to fix the size of the energy elements ε to hv. The standard view in Kuhn's time was that in this way, Planck had introduced the quantum in the form of the claim that an individual resonator's energy can only take on discrete, discontinuous values (i.e. integral multiples of hv). According to Kuhn, however, Planck in 1901 did no such thing. Planck's combinatorials were only concerned, according to Kuhn,[41] with the distribution of energies E over sets of resonators with different frequencies: the restriction $\varepsilon = hv$ only concerned the size of the subdivisions of the energy continuum, not the energy of individual resonators. As we have seen (footnote 49), Boltzmann had shown that his energy elements allowed him to capture the energy continuum, and Planck believed, according to Kuhn,[42] that the same would eventually be achievable for black-body radiation. Hence, Kuhn claimed, for Planck $\varepsilon = hv$ "did not therefore bring

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[39] Kuhn 1987, p. 105.

[40] Kuhn 1987, p. 105.

[41] Kuhn 1987, p. 104, 357-360.

[42] Kuhn 1987, p. 128.

to mind anything like quantization",[43] nor had "the concept of restricted energy […] played [any] role in his thought" while deriving his distribution law.[44] To argue for this, Kuhn pointed out that Planck saw his 1901 derivation of his new distribution law[45] as a further elaboration of his earlier work:[46]

> [T]he papers themselves make no explicit mention of such concepts [as quantization or discontinuity], and his next relevant paper is not easily reconciled with the assumption that he nevertheless had them in mind. That paper, which appeared late in 1901, was described in its title as a 'Supplement' to the one in which, at the start of 1900, he had presented his proof of irreversibility and his demonstration that the Wien law would follow if his candidate for entropy function were unique. After a brief introduction, both the paragraphs and formulas of the 'Supplement' were numbered to continue where those of the earlier paper had stopped. What he showed in those paragraphs was that his new entropy function, like the older one he had thought unique, could only increase monotonically with time. The role of his new probabilistic argument was, as he saw it, simply to fill a gap in the theory he had completed in 1899. It demonstrated that the new function was *the* thermodynamic entropy […].[47]

Planck's 1901 work, Kuhn argued, was inextricably linked with his earlier work.[48] Without the Maxwellian framework developed there, Planck's combinatorials could provide no information about possible energy distributions, since he then had no way to

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[43] Kuhn 1987, p. 351.

[44] Kuhn 1987, p. 126.

[45] Max Planck, 'Über das Gesetz der Energieverteilung im Normalspectrum', in *Annalen der Physik* 4 (1901a), 553-563; 'Über die Elementarquanta der Materie und der Elektrizität', in *Annalen der Physik* 4 (1901b), 564-566; 'Über irreversible Strahlungsvorgänge (Nachtrag)', in *Annalen der Physik* 6 (1901c), 808-831.

[46] Kuhn 1987, p. 116.

[47] Kuhn 1987, p. 353.

[48] Kuhn 1987, p. 117.

link resonator energy to field energy.[49] He equally well relied on this framework to link his combinatorial arguments, concerned with a multitude of resonators, with the entropy function he had originally formulated for a single resonator, and in his elaboration of the probability of a state in terms of the notion of natural radiation.[50]

The point becomes even clearer, according to Kuhn, when one looks at Planck's first major publication on the topic afterwards, namely his 1906 *Lectures on the Theory of Thermal Radiation*.[51] These lectures, Kuhn claimed, "still include all the main elements developed in the research program he had pursued from 1894 through 1901".[52] Moreover, Kuhn argued, while Planck there recognized that his theory necessitated fixing energy element size, he did not see this as restricting the energy values of individual resonators: these could vary continuously, as he emphasized by conceptualizing the attribution of an energy element to an individual resonator in terms of that resonator's energy lying within a given, continuous energy region.[53]

In the second part of the book, Kuhn therefore turned to the question when the quantum entered physics, and how Planck's name got connected to it. At first, others did not see Planck's work as imposing restraints on resonator energy either: while most of his readers between 1901 and 1906 pointed out the success of Planck's distribution law, only very few even mentioned the fixed energy element size.[54] Things only started

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[49] Kuhn 1987, p. 118.

[50] Kuhn 1987, p. 118-125.

[51] Planck, Max, *Vorlesungen über die Theorie der Wärmestrahlung* (Leipzig: Verlag von Johann Ambrosius Barth, 1906).

[52] Kuhn 1987, p. 117.

[53] Kuhn 1987, p. 129.

[54] Kuhn 1987, p. 134-140. For an extensive discussion of the reception of Planck's law, on which Kuhn based his discussion, see Elizabeth Garber, 'Some Reactions to Planck's Law,

to change with discussions concerning the Rayleigh-Jeans distribution law.[55] This law was a consequence of Lord Rayleigh's argument, in 1900, that Wien's distribution law wrongly entailed that energy would no longer increase with temperature above a certain threshold. Rubens and Kurlbaum soon showed that while this criticism of Wien's law was experimentally correct, the alternative distribution law derivable from it equally well suffered problems.[56] Rayleigh himself in a sense agreed: the law's derivation relied on the equipartition theorem – the claim that "in any mechanical system each degree of freedom will on average possess the same kinetic energy" –, and he was not sure whether the theorem applied here.[57]

Around 1904, however, James Jeans used Rayleigh's results in his work on the specific heat of gases to argue that "no other equilibrium distribution of radiation energy [besides Rayleigh's] can be compatible with classical theory".[58] From this he inferred that "the physical situations studied in black-body experiments were therefore not cases of equilibrium at all", hence concluding that both Planck's theory and most black-body experiments were invalid.[59]

In 1905-1906, Paul Ehrenfest argued that Planck had in fact produced two different entropy functions: an electromagnetic and a combinatorial one. The problem was that "Planck's criterion for an entropy function has been simply that it increase steadily to a stationary state, [and hence] he has no basis on which to choose between

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1900-1914', *Studies in History and Philosophy of Science* 7.2 (1976), 89-126.

[55] Kuhn 1987, p. 144.

[56] Kuhn 1987, p. 144-147.

[57] Kuhn 1987, p. 146.

[58] Kuhn 1987, p. 150.

[59] Kuhn 1987, p. 149. Many were critical of Jeans's claims, such as Rayleigh himself, who criticized its reliance on the equipartition theorem,) Kuhn 1987, p. 148-149.

them".[60] To overcome this, Ehrenfest argued, more detailed assumptions were required concerning the exchange of energy between resonators with different frequencies[61] One such assumption, according to Ehrenfest, could be a restriction of individual resonator energy to multiples of hv.[62]A similar claim was made by Albert Einstein, who argued that "[a]nalyzed in classical terms, […] Planck's black-body model could lead only to the Rayleigh-Jeans law".[63] Einstein made this claim on the basis of thermodynamical work he had carried out earlier, which convinced him that one could obtain both the Rayleigh-Jeans law, if one let resonator energy vary in a continuous way, and Planck's law, if resonator energy was restricted to integral multiples of ε.[64]

Ehrenfest and Einstein where thus the first to explicitly link Planck's distribution law with restricted resonator energy. According to Kuhn, however, they did this from an "isolated position": they were "too young and little known for their opinions to carry much weight on so potentially controversial a point".[65] Things only started to change when better established figures started engaging. The first to do so was Hendrik Lorentz, who argued, in his 1908 Rome lecture, that "[r]igorous and straightforward application of the laws of mechanics and electromagnetic theory [shows] that the Rayleigh-Jeans law must describe the distribution of energy in the field for all [wavelengths] $\lambda > \lambda_0$, where λ_0 could be chosen arbitrarily close to zero".[66]

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[60] Kuhn 1987, p. 155.

[61] Kuhn 1987, p. 159.

[62] Kuhn 1987, p. 166-169.

[63] Kuhn 1987, p. 170.

[64] Kuhn 197, p .183-184.

[65] Kuhn 1987, p. 188-189. The only other physicist to follow them was Max von Laue, equally young and unknown according to Kuhn.

Lorentz's conclusion did receive widespread response, mainly in negative terms: Wien, Lummer and Pringsheim all pointed out that "the Jeans law is experimentally impossible", because it wrongly entailed that black bodies should be clearly visible in the dark.[67] Lorentz at once conceded this point, stated that Planck's law was therefore the only tenable one, and suggested that its theoretical problems could be overcome by restricting resonator energy,[68] hence putting the issue on the table: "[o]nly after Lorentz's Rome lecture does the physics profession at large seem to have been confronted by […] the need to choose between Jeans's theory and a non-classical version of Planck's".[69] One consequence of Lorentz's conclusion was that it entailed Planck's first public mention of restricted resonator energy: in a letter to Lorentz, he stated that "the energy of the resonator at a given instant is *ghv* (*g* a whole number or 0)". [70] Similarly, Wien and Jeans equally well explicitly stated, following Lorentz's lecture, that Planck's theory required discontinuous resonator energy.[71]

As such, on Kuhn's narrative, by 1910 at least a few scientists were committed to the quantum in some form. At the same time, however, they no longer formed merely a bunch of individual scientists, but rather a community concerned specifically with quantization. Kuhn argued for this, first of all, by showing that by 1911, the quantum had become a more general topic: they now no longer worked solely on black-body radiation, but equally well on specific heats, atomic structure, and other forms of

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[66] Kuhn 1987, p. 191. Like Rayleigh, Lorentz had reservations about the application of the equipartition theorem in this case.

[67] Kuhn 1987, p. 193.

[68] Kuhn 1987, p. 193-194.

[69] Kuhn 1987, p. 195.

[70] Kuhn 1987, p. 198.

[71] Kuhn 1987, p. 202-205.

radiation.[72] Moreover, this went together with a few public events that consolidated the quantum as a topic for physical research, such as the 1911 *Naturforscherversammlung* in Karlsruhe and the first Solvay Congress in Brussels.[73] Finally, different rederivations of Planck's law in explicitly quantized terms emerged (besides the attempts by Ehrenfest, Einstein and Lorentz, Kuhn also discussed another one by Lorentz,[74] and one by Planck from 1911[75]). The result of all this, according to Kuhn, was the following:

> By the years 1911 and 1912, with which this volume closes, all or virtually all those physicists who had devoted significant attention to cavity radiation were persuaded that it demanded some Planck-like theory, which would, in turn, require the development of a discontinuous physics. Though no one claimed to know what the shape of the next physics would be, the men concerned all recognized that there could be no turning back.[76]

What did Planck do?

Black-Body Theory put the question whether Planck had introduced the quantum or not on the table. Kuhn was taken as representing the continuity-claim that Planck had relied on a continuous conception of resonator energy, against the prevalent discontinuity-

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[72] Kuhn 1987, p. 207-228.

[73] Kuhn 1987, p. 230-232. The *Naturforscherversammlung* was the yearly meeting of German natural scientists and doctors. See Querner, Hans and Schipperges, Heinrich, *Wege der naturforschung 1822-1972* (Berlin: Springer, 1972) for an older historical discussion. For discussions of the Solvay Congress, see Diana Kormos Barkan, 'The Witches' Sabbath: The First International Solvay Congress in Physics', in *Science in Context* 6.1 (1993), 59-82, Staley, Richard, *Einstein's Generation: The Origins of the Relativity Revolution* (Chicago: The University of Chicago Press, 2008), p. 397-422 and Seth 2010, p. 139-173 (footnote 25).

[74] Kuhn, 1987, p. 102-103.

[75] Kuhn 1987, p. 235-244.

[76] Kuhn 1987, p. 144.

claim that Planck had introduced the quantum. Soon after, a third position emerged, often called the indetermination-view.[77] According to this view, Planck's work could not be characterized in terms of a commitment to either the quantum or to continuity, since he explicitly refrained from any microphysical commitment,[78] or because the conceptual framework presupposed by such a commitment was not yet available.[79] They criticized Kuhn's continuity-characterization of Planck's reasoning for the following reason:

> If, as Kuhn insists, Planck was faithfully following Boltzmann's procedures, he should have reached the Rayleigh-Jeans law instead of Planck's law, for in Boltzmann's gas case the size of the cells (the counterpart to Planck's energy-elements) disappears from the final entropy formula.[80]

While Boltzmann, we have seen (page 9), relied on energy elements to derive his relation between probability and entropy, these elements did not figure in the final formulation of the relation. If Planck was indeed following Boltzmann, as Kuhn is supposed to have claimed, then the combination of continuous resonator energy and his formal equilibrium-condition would have led him to the Rayleigh-Jeans law.[81] Given that Planck did not derive the Rayleigh-Jeans law, he cannot have been reasoning in continuous terms.

This does not entail, however, that Planck was consciously following Boltzmann in quantized terms, according to the indetermination-argument. Rather, Planck was not faithfully following Boltzmann. As Badino has shown, Boltzmann's combinatorial

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[77] Darrigol 2001 (footnote 8) offers an overview of the different positions.

[78] Badino 2009 (footnote 9).

[79] Büttner, Renn and Schemmel 2003 (footnote 11).

[80] Darrigol 2001, p. 232.

[81] Darrigol 2000, p. 957; 2001, p. 227, 233.

framework can be interpreted in two ways: first, as concerned with the distribution of indistinguishable energy elements over resonators, which entails that energy can only be emitted and absorbed in discrete units; or, second, as concerned with the distribution of resonators over distinguishable energy cells, in which case resonators can take on any energy value.[82] Planck did not follow Boltzmann here because while Boltzmann’s focus was on the distribution of energy over individual molecules, Planck’s distribution did not concern individual resonators but only sets of frequencies.[83] Hence, he could remain silent about their microphysics: on his account, they had “indeterminate internal structure”,[84] and their behaviour “belongs to a domain of phenomena, namely the micro-phenomena, which Planck was unwilling to investigate”.[85] And it was precisely the ambiguity in Boltzmann’s formalism that allowed Planck to remain silent about such microphysical questions, according to Badino:

> [T]he ambiguity does not speak directly for a commitment of Planck toward continuity or discontinuity. Instead, Planck might have integrated the combinatorial procedure precisely because its formal ambiguity implies that the combinatorial formalism is independent of particular physical assumptions.[86]

The Relation to Structure

Kuhn himself pointed out in the afterword to the second edition of *Black-Body Theory* that he had refrained from using the vocabulary developed in *Structure* because he did not want to “constrain historical evidence within a predetermined mold”.[87] Still, he was

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[82] Badino 2009, p. 85-86.

[83] Badino 2009, p. 89.

[84] Darrigol 2001, p. 233.

[85] Badino 2009, p. 82.

[86] Badino 2009, p. 86.

[87] Kuhn 1987, p. 363

"generally […] well satisfied by the extent to which my narrative fit the developmental schema that *Structure* provides": one could discern a crisis in the attempts to reconcile Planck's law with classical physics, and the start of a revolution in 1906.[88] However, according to Büttner, Renn and Schemmel, few commentators agreed: many rather "felt a certain relief that, apparently, even Kuhn himself was no longer taking his approach so seriously since he had in fact renounced his own terminology". [89]

Büttner, Renn and Schemmel then argued that if a quantum-revolution had occurred in terms of *Structure*, it had to be in the form of "a sudden and total turnover that eludes further analysis" and which "changed both the conception of the objects of physical research and the language to designate them".[90] In terms of *Black-Body Theory*, this meant that "Einstein's derivation of the error in Planck's classical derivation led, according to Kuhn, immediately to the establishment of a quantum-derivation of the law".[91]

They claimed, however, that this reading did not fit the history. First, there was no real crisis preceding what Kuhn saw as the revolutionary moment: it was only gradually that scientists became aware of incompatibilities between Planck's law and classical physics. Moreover, this awareness only really became a shared moment of crisis in 1911, around the time of the Solvay Congress. Finally, the turn to the quantum was not a sudden turnover eluding analysis. It rather resulted out of careful, time-consuming theoretical analyses by different scientists of the connections between Planck's law and other domains. This also provided more evidence for the indetermination-position, they claimed: it was only after Planck's and Einstein's work,

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[88] Kuhn 1987, p. 363.

[89] Büttner, Renn and Schemmel 2003, p. 40.

[90] Büttner, Renn and Schemmel 2003, p. 38-39.

[91] Büttner, Renn and Schemmel, 2003, p. 39.

when scientists started investigating these connections, that the conceptual vocabulary required to distinguish quantum from classical emerged. Before, “what actually had been quantized remained rather unclear. Due to the unspecific nature of the resonators this remained an open question”.[92] They summarized their position as follows (a claim recently repeated in very similar terms by Adam Timmins[93]):

> According to Kuhn’s theory, a sudden gestalt switch that can usually be ascribed to an individual ends a period resulting from anomalies and brings about a new paradigm. [… I]n the early history of the quantum discovery, breaks with classical physics were rather the result of the gradual and tedious exploration, not just by an individual scientist but also by the scientific community.[94]

Conceptually Refining Paradigms

Both criticisms of *Black-Body Theory* essentially concern the notion of a paradigm: regarding what Planck did, it comes down to how Boltzmann’s work acted as an exemplar; and regarding the relation to *Structure*, it concerns whether the book provides a paradigm change. Now, as Kuhn himself pointed out on different occasions (e.g. in the 1969 postscript to *Structure*,[95] as well as in his 1974 essay *Second Thoughts on Paradigms*[96]), his original conceptualization of paradigms had its issues. As he put it in 1977, when he was writing *Black-Body Theory*:

> Unfortunately, in that process [of writing *Structure*], paradigms took on a life of their own, largely displacing the previous talk of consensus. Having begun simply as exemplary problem solutions, they expanded their empire to include, first, the

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[92] Büttner, Renn and Schemmel, 2003, p. 49.

[93] Timmins 2019, p. 386.

[94] Büttner, Renn and Schemmel 2003, p. 56.

[95] Kuhn 1996, p. 174.

[96] Kuhn, Thomas S., *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: The University of Chicago Press, 1977), p. 293.

> classic books in which these accepted examples initially appeared and, finally, the entire global set of commitments shared by the members of a particular scientific community. That more global use of the term is the only one most readers of the book have recognized, and the inevitable result has been confusion: many of the things there said about paradigms apply only to the original sense of the term. Though both senses seem important to me, they need to be distinguished, and the word "paradigm" is appropriate only for the first.[97]

Kuhn elaborated the distinction between exemplary problem solutions and global sets of shared commitments in more detail in his later work in terms of the distinction between exemplars and disciplinary matrices.[98] This conceptualization was not new, nor was it an abandonment of the ideas put forward in *Structure*: there as well, he had characterized paradigms as "accepted examples of actual scientific practice [… which] provide models from which spring particular coherent traditions of scientific research".[99] What he now rather did was refining these ideas by conceptually dividing up paradigms into exemplars ('accepted examples of actual scientific practice') and disciplinary matrices ('coherent traditions of scientific research').

One reason why Kuhn originally introduced paradigms, in the form of recognized concrete solutions to specific problems, was to account for how scientists learned how to apply abstract theoretical formalisms and concepts to concrete phenomena. He argued that they do not learn how to work with a theory by just studying its laws and equations, but rather through "the study of applications, including practice problem-solving both with a pencil and paper and with instruments in the laboratory".[100] In this way, they acquired a practical understanding of the central

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[97] Kuhn 1977, p. xix-xx.

[98] Kuhn 1996, p. 176-182; 1977, p. 297.

[99] Kuhn 1996, p. 10.

[100] Kuhn 1996, p. 47.

theoretical concepts. That scientists agree on the exemplars embodying these concepts does not mean, however, that they necessarily have a shared interpretation of these concepts. As Kuhn put it (a claim also found in later essays[101]):

> Scientists can agree […] in their *identification* of a paradigm without agreeing on, or even attempting to produce, a full *interpretation* or *rationalization* of it. Lack of a standard interpretation or of an agreed reduction to rules will not prevent a paradigm from guiding research.[102]

This lack of a shared interpretation is not a problem, according to Kuhn: as long as the scientists involved agree that the concrete exemplars adequately illustrate how to proceed in practice, it does not matter whether their interpretation corresponds to that of their colleagues. In fact, one should not even presuppose that it is always possible to construct such a shared interpretation. As Kuhn put it in *Structure*, "[t]he coherence displayed by the research tradition in which they participate may not imply even the existence of an underlying body of rules and assumptions that additional historical or philosophical investigation might uncover".[103]

This last quote also indicates that this interpretative indeterminacy does not disappear when one moves from concrete exemplars to disciplinary matrices. Such matrices, which consist of, among other things, the exemplars, formalisms, models and values shared by a scientific community, can equally well subsume different interpretations, as Kuhn argues in different places by pointing out that scientists who share certain theoretical values – e.g. accuracy, simplicity, fruitfulness – can still differ on how these are to be made concrete in particular situations.[104] Sharing a paradigm only presupposes that scientists agree that specific scientific achievements are

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[101] Kuhn 1977, p. xix, 285.

[102] Kuhn 1996, p. 44.).

[103] Kuhn 1996, p. 46.

exemplars to be followed. Over time, such agreements will transform such a group of scientists in a community with a disciplinary matrix, since, as Kuhn points out in *Structure*, “[i]n learning a paradigm the scientist acquires theory, methods, and standards together, usually in an inextricable mixture”.[105] Again, this close connection between sharing a paradigm and forming a community united by a disciplinary matrix is emphasized by Kuhn both in *Structure*[106]and in his later work.[107]

What did Planck do?

In *Black-Body Theory*, Boltzmann’s probabilistic derivation of his law for the distribution of energy over molecules acted as an exemplar for Planck’s derivation of his distribution law. While no one disputes that Boltzmann inspired Planck, Kuhn has been criticized because he is read as claiming more specifically that in following Boltzmann, Planck also took over his continuous conception of energy for his resonators. In that case, Planck should have derived the Rayleigh-Jeans law rather than his own distribution law.

This criticism assumes that a paradigm comes with a particular interpretation: following Boltzmann’s derivation entails a commitment to his conceptualization of energy, and hence Kuhn is read as claiming that Planck explicitly conceptualized the energy of individual resonators in continuous terms, just as Boltzmann had done for individual molecules. However, in his discussion of how Planck followed Boltzmann,

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[104] Kuhn 1996, p. 185; 1997, p. 324; *The Road Since Structure: Philosophical Essays 1970-1993, with an Autobiographical Interview* (Chicago: The University of Chicago Press, 2000), p. 134.

[105] Kuhn 1996, p. 109.

[106] Kuhn 1996, p. 94.

[107] Kuhn 1996, p. 176; 1977, p. 294.

Kuhn always stressed that Planck primarily took over formal-mathematical techniques. This can be seen in particular in his discussion of how Boltzmann's approach to the definition of molecular order – elaborating a formal validity condition and then stipulating that this formula covers the concept's scope – inspired not only Planck's concept of natural radiation (see page 7), but equally well his definition of probability.[108] In both cases, Kuhn stressed that what Planck took over from Boltzmann was primarily how to elaborate a mathematical condition governing thermal equilibrium.[109]

Kuhn then explicitly argued, moreover, that taking over these techniques did not entail taking over any explicit interpretation of the microphysics underlying their use. In contrast to Boltzmann's explicit reliance on a continuous energy conception for his molecules, the question concerning the precise constitution of his resonators was of no real significance for Planck, according to Kuhn: "Planck's concern […] had been and remained with radiation. His resonators were imaginary entities, not susceptible to experimental investigations. Their introduction was simply a device for bringing radiation to equilibrium".[110] On Kuhn's account as well, Planck was rather undetermined with regards to the energy of individual resonators, as can be seen from the fact that Kuhn equally well stressed in different places that Planck's combinatorials only concerned sets of frequencies (as Badino and Darrigol did in their indetermination-arguments, see page 18).[111] And in line with Darrigol's indetermination-claim that "for Planck the significance of the energy elements was an open question, having to do with

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[108] Kuhn 1987, p. 122.

[109] Kuhn 1987, p. 78, 88, 121.

[110] Kuhn 1987, p. 117-118.

[111] Kuhn 1987, p. 104-105, 108-109, 117-118, 359.

the electrodynamics at a finer, non-observable scale",[112] Kuhn argued as well that it was an open question for Planck how the constant *h* fixed the size of the energy element *ε*,[113] and that Planck believed that this "puzzle posed by his theory would be solved by research on the microscopic detail of the emission process, thus by electron theory".[114] As such, Kuhn's characterization of Planck seems in fact quite close to the indetermination-view: Planck took over Boltzmann's formalism but remained silent about how it should be interpreted theoretically.

This now also shows why, on Kuhn's account, Planck did not arrive at the Rayleigh-Jeans law. The derivation of this law, according to Kuhn, relied on quite a lot of (microphysical) assumptions that were, at the time, either open for discussion or of no direct significance for what Planck was interested in, i.e. radiation in thermal equilibrium. First, the derivation's reliance on the equipartition theorem was disputed, as we have seen, because it was unclear whether the theorem was applicable beyond the case of gases (see page 13 and footnotes 76 and 85). It also presupposed a mechanical ether-conception that could not readily be applied to black-body radiation.[115] And there was quite some experimental evidence against it (as the reaction to Lorentz's derivation of it shows, see page 15), and overcoming this would require the reconceptualization of many well-established thermodynamical laws.[116]

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[112] Darrigol 2001, p. 234.

[113] Kuhn 1987, p. 131.

[114] Kuhn 1987, p. 131.

[115] Kuhn 1987, p. 151.

[116] Kuhn 1987, p. 150.

Planck’s law, on the other hand, “was known to be in excellent quantitative agreement with experiment”,[117] and “[his] model seemed free of such difficulties”.[118] As such, it becomes clear why Planck, on Kuhn’s account, did not end up with the Rayleigh-Jeans law: in following Boltzmann, Planck took over certain techniques that could be useful for the questions he was interested in, but that did not commit him to any microphysical assumptions that Boltzmann had made for the problems of his concern, and without an explicit commitment to these assumptions, there was no reason why Planck should have ended up with the Rayleigh-Jeans law.

The Relation to **Structure**

Kuhn’s claim that *Black-Body Theory* aligned well with Structure was criticized, we have seen, on the ground that the book provided no revolutions in the form of a sudden, all-encompassing gestalt switch, nor any real crises preceding it. Rather, as Timmins put it, “[t]he transition from classical physics to its quantum counterpart seems, contra Kuhn, to have been a remarkably rational affair”,[127] in which scientists, according to Büttner, Renn and Schemmel, investigated the question left open by Planck’s work, i.e. “what actually had been quantized”.[128]

As the quote from Büttner, Renn and Schemmel on page 20 shows, this criticism presumes a conceptualization of paradigm change that essentially equates it with gestalt switches. Already in *Structure*, however, Kuhn stressed that the notion of gestalt was only “a useful elementary prototype for what occurs in full-scale paradigm shift”, and

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[117] Kuhn 1987, p. 150.

[118] Kuhn 1987, p. 151.

[127] Timmins 2019, p. 379.

[128] Büttner, Renn and Schemmel 2003, p. 49.

nothing more than that.[129] In fact, in different places in *Structure* Kuhn pointed out that one should not read too much into it, since "[t]hat parallel [between gestalt and paradigm] can be misleading".[130] The primary difference, according to Kuhn, is that scientists "do not see something *as* something else; instead, they simply see it. […] In addition, the scientist does not preserve the gestalt subject's freedom to switch back and forth between ways of seeing".[131]

Moreover, both in *Structure*[132] as well as in later work,[133] Kuhn emphasized that how a paradigm structured a scientist's way of seeing could not be separated from the scientific community of which they are a part. As he put it in *Second Thoughts on Paradigms* (1974), "[a] paradigm is what the members of a scientific community, and they alone, share. Conversely, it is their possession of a common paradigm that constitutes a scientific community of a group of otherwise disparate men".[134] The reason for this is that scientists enter a community by learning how to apply the exemplars shared by the community. In this way, "the student [becomes] an inhabitant of the scientist's world, seeing what the scientist sees and responding as the scientist does".[135]

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[129] Kuhn 1996, p. 85.

[130] Kuhn 1996, p. 85. As Kuhn pointed out, it was N.R. Hanson who introduced the notion of gestalt in his *Patterns of Discovery* (Cambridge: Cambridge University Press, 1958). Later on, he again stresses the differences between paradigm switches and gestalt switches, e.g. Kuhn 1996, p. 111-114.

[131] Kuhn 1996, p. 84.

[132] Kuhn 1996, p. 10-11, 43, 46, 94, 111, 163-173.

[133] Kuhn 1996, p. 176; 1977, p. 278, 296, 308-309; 2003, p. 103, 131, 147.

[134] Kuhn 1977, p. 294.

[135] Kuhn 1996, p. 111.

Black-Body Theory now offers an extensive historical account of the combined emergence of the quantum as a way of seeing certain phenomena and of the accompanying scientific community. As the quote on page 16 indicates, this process resulted around 1911 with a group of scientists who shared a commitment to the development of what they saw as a Planck-like discontinuous physics, without necessarily agreeing on what such a physics would look like (which again emphasizes how sharing a paradigm does not presume sharing an interpretation). This becomes especially clear in the second part of *Black-Body Theory*, where Kuhn emphasized, for example, the importance of the different attempts to rederive Planck's distribution law, which all in a way linked the law with the idea of a discontinuity.[136] Equally important in this regard, as Kuhn showed through an extensive bibliographical analysis, were the different attempts to elaborate links between Planck's law and domains besides black-body radiation (specific heats, atomic structure, other forms of radiation, etc.).[137]

Kuhn then showed how this conceptual elaboration of Planck's law as an exemplar was closely connected to the formation of a community, by emphasizing, for example, how Lorentz, because of his position in the community, could put the quantum-question on the table, whereas Ehrenfest and Einstein could not because of their isolated position (see page 14). Equally important in this regard is Kuhn's repeated emphasis on the importance of different social events (see page 16). The result was that, by 1911, a community had formed around different rederivations of Planck's law that emphasized the need for a quantization of resonator energy, in such a way that, even though Planck himself in 1901 was not explicitly committed to it, his work became identified as its source.

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[136] Kuhn 1987, p. 102-103, 155, 170, 235-244.

[137] Kuhn 1987, p. 206-232.

Once one realizes that *Structure* did not conceptualize paradigm change as essentially identical to a gestalt switch, one can very well describe *Black-Body Theory* as concerned with revolutionary change, i.e. with what Kuhn described in the 1969 afterword to *Structure* as “a certain sort of reconstruction of group commitments”: it narrates how a group of scientists, over time, became committed to the idea that energy, at least in certain cases, had to be quantized, and to the claim that Planck’s distribution law, in its rederived form, offered an exemplar of how this was to be carried out, without necessarily agreeing on how this exemplar had to be interpreted precisely.[138] And this change occurred in response to what for some, e.g. Einstein and Ehrenfest, constituted a crisis, namely the fact that they were not able to solve the puzzles they were interested in when they tried to combine Planck’s distribution law with classical physics. This crisis became public after Lorentz’s 1908 work, and was resolved more or less around 1911, when the quantum was recognized as a topic of research that was to be investigated in a Planck-like way. Finally, that *Black-Body Theory* described this revolutionary change as extended in time, rather than as an all-encompassing sudden turnover, should not be seen as an abandonment of *Structure*. Already in the introduction to *Structure*, Kuhn described revolutions as extended in time: since they involve “the reconstruction of prior theory and the re-evaluation of prior fact, an intrinsically revolutionary process […] is seldom completed by a single man and never overnight”.[139]

Concluding Remarks

In this paper, I have discussed the two major criticisms raised against Kuhn’s *Black-Body Theory*, namely that it misrepresented Planck’s position in 1901 and that it

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[138] Kuhn 1996, p. 181.

[139] Kuhn 1996, p. 7.

abandoned the framework developed in *Structure*. Both of these criticisms concern the notion of a paradigm: regarding what Planck did, it concerns how Boltzmann acted as an exemplar, and regarding the relation to *Structure*, it concerns whether the book's narrative can be characterized in terms of a paradigm change. I have then argued that these criticisms presuppose a conceptualization of paradigms that does not align well with how Kuhn elaborated the notion both in *Structure* and in later work. They presume, more specifically, that sharing a paradigm entails sharing an interpretation of it, and that paradigm changes are in essence identical to gestalt switches. By showing that these presuppositions are not in line with *Structure*, I have then also argued that, in fact, Kuhn's position in *Black-Body Theory* is quite close to the indetermination-position developed by some of his critics, and that the narrative provided by *Black-Body Theory* does fit *Structure* quite well.

As we have seen, Kuhn claimed that the narrative provided by *Black-Body Theory* aligns quite well with the framework provided by *Structure*. At the same time, however, he refrained from casting this narrative explicitly in terms of *Structure*, since he wanted to avoid constraining the historical narrative in a predetermined philosophical mold. This is in line with how Kuhn claimed, in different essays,[140] that history and philosophy of science were significantly different disciplines. Whereas history aims at producing "a narrative, a story, about particulars of the past", and tries to "render plausible and comprehensible the events it describes", without "almost [any] recourse to explicit generalizations", philosophy aims at arguing for general statements with universal scope, and "to discover and state what is true at all times and places rather than to impart understanding of what occurred at a particular time and place".[141] And while Kuhn believed that both endeavours could benefit from inter-disciplinary dialogue

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[140] Kuhn 1977, p. 3-20, 105-126, 127-161.

[141] Kuhn 1977, p. 5.

and from practitioners switching between the two (as he himself had attempted throughout his career), he also claimed that “no one can practice them both at the same time”:[142] when one practices one approach, one takes on a perspective that is incompatible with that of the other (and he then compares switching between them with a switch in gestalt).[143]

As e.g. Stephen G. Brush and K. Brad Wray have pointed out,[144] Kuhn saw *Black-Body Theory* as purely historical in nature. Hence, the book should also be read in this way: it is important to consider the book as a whole, since its aim is to explain the emergence of the quantum by means of a narrative structure that links together many particular events. Most of the criticisms of the book discussed here, however, only focus on particular aspects of the book, and take these separately as arguments for more general, philosophical claims: hence, most of them discuss only Kuhn’s discussion of e.g. Planck or Einstein, but pay no attention to Kuhn’s discussion of different social events, nor to his bibliographical analysis of the quantum-literature between 1900 and 1916, nor do they acknowledge the importance he accords to the different rederivations of Planck’s distribution law. This is problematic, however, since it is only by combining all these elements together that we can come to see, according to Kuhn, how Planck’s law became an exemplar for the elaboration of a quantized physics.

For this reason, rather than immediately arguing that *Black-Body Theory* can be read in terms of paradigms, or comparing the aspects of paradigms highlighted here with those singled out in philosophical discussions of Kuhn’s work, I have mainly focused on presenting the narrative structure underlying *Black-Body Theory* (as is illustrated by the number of pages concerned with what Kuhn has said in *Black-Body*

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[142] Kuhn 1977, p. 5.

[143] Kuhn 1977, p. 5-6.

[144] Brush 2000; Brad Wray 2021, p. 126 (see footnote 13 for the full references).

Theory). This does not mean that there are no connections to be drawn with philosophical discussions regarding paradigms and the practice of science.[145] Elaborating these would require, however, a different approach. I hope that the discussion presented here can provide inspiration for such future reflections.

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Declaration of Interest

The author hereby declares that there are no known conflicts of interest associated with this publication.

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[145] One interpretation that is quite in line with the one presented here was offered by Joseph Rouse, *Knowledge and Power: Toward a Political Philosophy of Science* (Ithaca: Cornell University Press, 1987), p. 26-40. For other similar interpretations, see Lydia Patton, ‘Kuhn, Pedagogy, and Practice: A Local Reading of *Structure*’, in *The Kuhnian Image of Science: Time for a Decisive Transformation?*, ed. by Moti Mizrahi (London: Rowman & Littlefield, 2018), p. 125, note 4.