Opening up HPS-debates: On reading Kuhn and the history of the quantum

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Abstract

In this paper, I discuss the reception of Kuhn's book on Planck and the quantum. Most criticisms of the book concern, I argue, how to understand Kuhn's notion of a paradigm, and they all read the notion, I claim by means of Joseph Rouse's work, in theory-driven terms. I then show that an alternative reading, in terms of a practice-focused reading of paradigms, is also possible, and is in fact to be preferred, since it is more in line with how Kuhn narrated the quantum-episode, it overcomes most of the criticisms raised against the book, and it allows us to open up new historical-philosophical research directions.

1 Introduction

Thomas Kuhn's *Black-Body Theory and the Quantum Discontinuity, 1894-1912* (first edition (1978), edition used here (1987)) has given rise to two related debates: first, can we say that Max Planck introduced the quantum?; and second, in how far is the book an application of the ideas developed in *The Structure of Scientific Revolutions* (first edition (1962), edition used here (1996))? These two questions emerge because Kuhn's *Black-Body Theory* argues against the following view:

> Planck, it has ordinarily been said, introduced at the end of 1900 the concept of a linear electrical oscillator with energy restricted to integral multiples of the energy quantum $h\nu$, ν being the oscillator frequency and h the universal constant later known by Planck's name. He had discovered that restricting energy levels to a discontinuous spectrum was essential to the derivation of the black-body radiation law he had introduced shortly before. (Kuhn, 1987, p. 350)

Kuhn claims, however, that "the concept of restricted resonator energy played no role in [Planck's] thought" until a few years after the publication of Planck's (1906) *Lectures on Thermal Radiation* (Kuhn, 1987, p. 126). Given that Planck's work is often seen as a revolution in the foundations of physics, the question what Planck did and whether it conforms to Kuhn's analysis of scientific change arises naturally.

I will argue here that at issue in these discussions is how a to understand a paradigm. Underlying most responses to *Black-Body Theory*, I will argue here by means of Joseph Rouse's (1987) work, we find a theory-driven reading of paradigms. On this reading, a paradigm constitutes a set of explicit theoretical commitments that tell scientists what there is and how to study it. Rouse argues, however, that an alternative, practice-focused reading is also possible. On this reading, a paradigm is to be seen more as a concrete achievement embodying a practice that scientists use to elaborate possible accounts of what there is and how to study it.[1] I will try to show here that this second reading is more in line with *Black-Body Theory*, that it addresses the criticisms raised, and that it is more fruitful, since it opens up new questions for history and philosophy of science.

2 The Reception of Kuhn's Black-Body Book

2.1 Kuhn on Black-Body Theory and the Quantum

At the time of Planck's work, black-body physics was concerned with the phenomenon of thermal radiation,[2] i.e. the fact that when a body is heated to a certain temperature T, it will emit radiation of a certain frequency ν (or wavelength λ).[3] The general aim was to find the precise relation between temperature and frequency, and for this, use was often made of a black body, which is "a cavity with perfectly absorbing (i.e., black) walls [...] maintained at a fixed temperature T, so that its interior will be filled with radiant energy of all wavelengths" (Kuhn, 1987, p. 3). The reason for this usage was that Gustav Kirchhoff had shown that the thermal radiation produced by a black body can be described in terms of "a universal function [...] giving the intensity of radiation at the frequency ν and at the temper-

[1]At first sight, one could say that Rouse's distinction aligns quite well with the distinction drawn by Kuhn (1977) himself, between paradigms as disciplinary matrices and paradigms as exemplars. I think there is a significant difference, however, since on Rouse's view, both readings take paradigms as exemplars embodying a disciplinary matrix: both on the practice-focused and on theory-driven view, paradigms are concrete achievements that scientists see as offering an approach to study other aspects of the same domain. They differ, however, on how to understand paradigms as offering guidance in this way. This is why I have decided to primarily use Rouse's work. I would like to thank an anonymous reviewer for suggesting this parallel to me.

[2]While my focus here is on how Kuhn discussed Black-body theory, I will also make use of later work, sometimes critical of Kuhn, to elaborate on certain technical notions.

[3]At the time, claims about thermal radiation were expressed in terms of either wavelength or frequency, with wavelength inversely proportional to frequency. It was "Planck [who] was the first to write the energy density and related quantities [of thermal radiation] in terms of the frequency ν instead of the wavelength λ" (Gearhart, 2002, p. 176).

ature T" (Galison, 1981, p. 72).[4] What is important here is the function's universality: it allows for the study of thermal radiation in general, since it entails that "[t]he radiation emitted by a black-body is [...] identical, in its intensity distribution, to the equilibrium radiation contained in a cavity of any material" (Kuhn, 1987, p. 4).

When Planck started working on black-body radiation in 1895, Wilhelm Wien had just proposed an expression for Kirchhoff's universal function, called Wien's distribution law, that was generally considered to be satisfactory (Gearhart, 2002, p. 177).[5] Planck's primary goal was to come up with a theory that could account for how black-body radiation achieves thermal equilibrium with its surrounding cavity walls. He aimed to do this, moreover, by means of an absolute, i.e. non-probabilistic, interpretation of the second law of thermodynamics. A probabilistic interpretation states that, even though it is highly improbable, it is possible that there are physical systems in nature that over time show a decrease in entropy S. An absolute interpretation does not allow any possible exceptions: "all isolated physical systems move irreversibly from states of lower to higher entropy" (Kuhn, 1987, p. 25). Planck thus aimed for a theory that "would yield an irreversible process, creating from an arbitrary initial distribution the desired universal distribution Kirchhoff had shown to exist" (Galison, 1981, p. 74).

Planck decided to model the interaction between the electromagnetic field in the cavity and the cavity's walls by means of electromagnetic resonators, i.e. linear oscillating dipoles that, when electromagnetic waves stimulate them, emit and absorb electromagnetic radiation and hence exchange energy with the field (Kuhn, 1987, p. 28-29). The interaction between resonators and field in the process towards thermal equilibrium was, Planck believed, irreversible, and hence it offered the promise of a theory based on an absolute interpretation of the second law (Gearhart, 2002, p. 175). Ludwig Boltzmann soon pointed out, however, that this could not be achieved: the Maxwellian electrodynamics governing the resonators-field interaction is completely reversible (Kuhn, 1987, p. 77). To overcome this, Planck introduced the concept of natural radiation, a formal condition ensuring that "the uncontrollable, irregular aspects of the evolution of the system were eliminated to yield a deterministic, irreversible evolution" (Darrigol, 2001, p. 220), analogous to Boltzmann's notion of molecular disorder in his derivation of an irreversibility theorem in gas theory.[6] This provided Planck with

[4]More specifically, this universal function, as Gearhart puts it, specifies the radiation density of black-body radiation, which is it's "electromagnetic energy per unit volume in a narrow frequency range [...,] a measure of how the intensity of the radiation varies with frequency or wavelength" (2002, p. 176).

[5]See Kangro (1970) for a very extensive discussion of the experimental context in which Wien's distribution law was developed.

[6]Gearhart summarizes Planck's concept of natural radiation as follows: "Since his resonators are damped, they do not respond to a single frequency but to a narrow range of frequencies, which he described in terms of Fourier components. The assumption of

a theory that entailed, by means of an irreversible entropy function, Wien's distribution law.

However, experiments soon showed systematic deviations from Wien's distribution law.[7] Moreover, around the same time Max Thiesen showed that theoretically speaking "an infinite number of choices of the [resonator entropy] function [S], and therefore an infinite number of black-body [distribution] laws were in fact compatible with [Planck's] irreversibility theorem" (Darrigol, 2001, p. 221). Reflecting on these issues soon brought Planck to an alternative distribution law, one which fitted all data available (Kuhn, 1987, p. 97). The way in which he proceeded entailed, however, that while he now had a successful distribution law, he had no black-body theory to account for it. To overcome this, Planck had to "consider the various ways in which [the total amount of resonator] energy might be divided between resonators as Boltzmann, in his combinatorial arguments, had divided the total energy of a gas among its component molecules" (Kuhn, 1987, p. 99-100).

Boltzmann's combinatorial arguments were concerned, more specifically, with how to compute, given a particular number N of molecules and a particular number P of energy elements ϵ that divide up the total amount of energy, the amount of possible combinations W that would give rise to a particular energy state of the gas. The more possible combinations there are for a particular state, the more probable that state is then considered to be.[8] What Boltzmann then did was relate, by means of a definition, the logarithm of the probability of a particular state, expressed in terms of the number of combinations that would give rise to it, to the entropy of that state: $S = k \ln W$, with k a universal constant (Gearhart, 2002, p. 181).

Planck, however, could not just apply Boltzmann's combinatorial arguments out of the box. Boltzmann could ascribe an arbitrary size to the energy elements, and eventually let them tend to zero ($\epsilon \to 0$), hence obtaining the energy continuum (Klein, 1962, p. 472). Planck, however, could do no such thing. Wien's displacement law, another law governing black-body radiation, demanded that "the size of the energy elements [...] must be fixed and proportional to frequency" (Kuhn, 1987, p. 104).[9] The size of Planck's energy elements was equal, more specifically, to the product of the frequency ν and a second universal constant h: $\epsilon = h\nu$. This provided Planck with a theory entailing his distribution law (Kuhn, 1987, p. 104-105).

Here we encounter the central point of debate that arose following Kuhn's

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natural radiation implies that these Fourier components [...] are independent and vary at random; this condition in turn places constraints on how one calculates the average resonator energy" (2002, p. 175).

[7]For an extensive discussion of these experiments, see Hoffmann (2008).

[8]For examples of how such combinatorial calculations work, see (Gearhart, 2002, 181-185) and (Badino, 2009, p. 83-85).

[9]Wien's displacement law describes how "the peak of the [frequencies/wavelengths-temperature] curves shifts to higher frequencies (shorter wavelengths) as the temperature of the radiating object increases" (Gearhart, 2002, p. 179).

Black-Body Theory. Before Kuhn, most people claimed that Planck, by setting the size of the energy elements to $\epsilon = h\nu$, had introduced the quantum, in the form of the claim that an individual resonator's energy can only take discontinuous values. Kuhn, however, argued that Planck should not be read in this way. While it is true that he had to divide the total energy in discrete elements of size $\epsilon = h\nu$, this does not entail that Planck believed the energy of the individual resonators to be quantized.

Kuhn gives the following arguments for this claim. First, both in his 1900-1901 papers and in his (1906) *Lectures*, Planck's argumentative structure built on his earlier theory for Wien's distribution law: as Kuhn puts it, "the events of late 1900 had not visibly changed Planck's view of the nature of the theory he had developed in the preceding years" (1987, p. 117). Second, concerning the theory's content as well, Planck did not see any discontinuity: his concepts and equations were firmly grounded in Maxwellian electrodynamics, which entails a continuous energy-spectrum, and Planck not only "needed to use Maxwell's equations, but [...] he was [...] unaware of the slightest awkwardness in doing so" (Kuhn, 1987, p. 118). And finally, even though Planck was forced to deviate from Boltzmann's approach concerning the size of the energy elements, he believed, according to Kuhn, that his discrete approach would in the end prove interchangeable with Boltzmann's continuous one, and as such "he felt justified in simplifying his combinatorial derivation by describing a discrete energy spectrum when the physical situation he had in mind called for a continuum" (1987, p. 128). As such, Kuhn concludes, it is clear that, at the time, Planck did not take resonator energy to be quantized in nature:

> As Planck's continuing emphasis on the close parallels between his theory and Boltzmann's suggests, his view of the radiation problem is still, in the *Lectures* of 1906, fully classical. [...] Both in his original derivation papers and, far more clearly, in the *Lectures*, Planck's radiation theory is incompatible with the quantization of resonator energy. That theory does require fixing the size of the small intervals into which the energy continuum is subdivided for purposes of combinatorial computation, and the restriction to a fixed size does isolate the main respect in which Planck's theory diverges from Boltzmann's. But the divergence does not, as developed by Planck, make radiation theory less classical than gas theory, for it does not of itself demand that the values of the resonator energy be limited to a discrete set. On the contrary, [...] any such restriction would conflict both with the global structure and with multiple details of Planck's argument. (Kuhn, 1987, p. 125)

2.2 The Reception of *Black-Body Theory*

2.2.1 What did Planck do?

As outlined in section 1, one debate following *Black-Body Theory* concerns the question what Planck did exactly in 1900-1901. A first position in this debate claims that "Planck worked from the very beginning with discrete elements of energy" (Badino, 2009, p. 81). The foremost representative of this discontinuist claim is Martin Klein – see his (1962) or his contribution to the (1979) *Black-Body Theory* symposium –, who argues that, even though Planck could have been thinking that his discrete energy elements were interchangeable with Boltzmann's energy continuum, fact of the matter is that they were not: "Planck never emphasized the quanta in his papers of 1900 and 1901, expecting that h would eventually be derived in some more basic way [...]. The quanta were there in his theory, nevertheless" (1962, p. 432). Kuhn's continuist claim forms the second position, and a third one is often called the indetermination-thesis. Its central claim is that the question whether Planck's work was classical or quantized does not make much sense: these concepts cannot be applied either because they were only developed later, or because Planck himself refrained from any explicit commitments with respect to this issue.[10] The following quote by Darrigol summarizes quite clearly the central issue in the debate, namely in how far Boltzmann acted as a paradigm for Planck:

> Kuhn's adversaries seem to have overlooked the gravest flaw of his argumentation. If, as Kuhn insists, Planck in 1900 was *faithfully following Boltzmann's procedures*, he should have reached the Rayleigh-Jeans law instead of Planck's law, for in Boltzmann's gase case the size of the cells (the counterpart of Planck's energy-elements) disappears from the final entropy formula. Then there must have been some inconsistency in Planck's application of Boltzmann's method. (Darrigol, 2001, p. 232; emphasis added)

The Rayleigh-Jeans law is a third distribution law and, as Klein (1962) showed, if one applies Boltzmann's combinatorial arguments with ϵ tending to zero to black-body radiation, one obtains this law instead of Planck's.[11] Central to this derivation is the equipartition theorem, a theorem relating

[10] It was Allan Needell who first developed this position in his doctoral dissertation. Unfortunately, I have not been able yet to consult this dissertation, so I cannot make any claims about Needell. This position can also be found in the work of Peter Galison (1981), Olivier Darrigol (2001), Clayton Gearhart (2002) and Massimiliano Badino (2009).

[11] A similar claim can also be found in Galison (1981). For this derivation see Klein (1962) or Gearhart (2002). As Gearhart (2002, p. 191) points out, the Rayleigh-Jeans law performs quite well in the long wavelength part of the spectrum, but leads to absurd results for the other parts.

energy to temperature to be found in work by Boltzmann that Planck knew of. Hence, strictly speaking, if Planck had been following Boltzmann as faithfully as Kuhn is taken to claim, he should have ended up with a different distribution law. There are two ways to handle this issue, according to Darrigol (2001, p. 232-233), neither of them favourable to Kuhn: one ends up either with the discontinuist or with the indetermination-view.

A first option is to argue that Planck's reasoning in fact deviated significantly from Boltzmann's because of the quantization that Planck was forced to introduce. On this account, found in the work of Klein (see page 6), one accepts that Planck's thinking was inconsistent: while Planck believed that he was carrying out a fully classical derivation, he in fact derived his distribution law on the basis of a theory combining classical elements with resonator quantization.

The second option claims, on the other hand, that Planck was consistent, and that he was, at the same time, following Boltzmann, but under a different interpretation. The starting point here is that, as Badino (2009) shows, Boltzmann's combinatorial arguments allow for two interpretations: the central formula can be read as concerning the distribution of both energy elements over resonators, or of resonators over energy elements. The first reading is discontinuist, since a resonator's energy can then be only an integer product of a discrete number of energy elements. The second reading is continuist, since then there is no constraint on where a single resonator's energy is to be located within an energy element (Badino, 2009, p. 85-86). However, this ambiguity on the formal level, it is then claimed, in fact argues for the indetermination-position:

> [T]he ambiguity does not speak directly for a commitment of Planck toward continuity or discontinuity. Instead, Planck might have integrated the combinatorial procedure in his general strategy precisely because its formal ambiguity implies that the combinatorial formalism is independent of particular physical assumptions. In other words, since the formal ambiguity leaves completely open the question of what is going on at the micro-level, it allows Planck to switch from one statistical model to another for purely computational reasons. (Badino, 2009, p. 86)

The central difference between Planck's and Boltzmann's approach, on this view, concerns the relation between macro- and micro-state in their respective theories. On Boltzmann's account, it was the micro-state dynamics of the individual gas molecules that provided the laws governing the macro-state, i.e. the gas. In Planck's case, on the other hand, it is mainly the macro-state assumption of natural radiation that allows for the derivation of the required radiation laws (see footnote 6). As Darrigol (2001, p. 235) points out, one does not need to specify the inner dynamics of the resonators to arrive at this result: the resonators are rather only introduced "because, for [Planck's] definition of a macro-state, it led to a much quicker calculation". In this way, Planck's hesitancy to specify, or his choice not to specify, the internal dynamics of his resonators allowed him to arrive at his distribution law in a way that was formally consistent with Boltzmann's approach, while also different with respect to its content. Moreover, Badino (2009) and Darrigol (2000) argue, this indetermination-reading of Planck is historically informed, since this approach to microphysics was typical of Planck's own style of doing physics, and of the local physics culture in which he was working.[12]

2.2.2 The Relation to Structure

Kuhn himself saw *Black-Body Theory* as "a narrative account of Planck's invention of the black-body theory known by his name and of that theory's development during the years when it and a closely-related theory of specific heats were the two exemplary applications of a still-to-be-developed quantum theory" (1987, p. 349). This narrative, according to Kuhn (1987, p. 363), fits *Structure* in a satisfactory way: insofar as there was a crisis, it concerned how Planck's derivation could be reconciled with the tenets of classical physics, and this led to the start of a revolution in 1906; and Boltzmann's combinatorial argument can be seen as a paradigm for Planck's elaboration of his black-body theory (1987, p. 363).

Jochen Büttner, Jürgen Renn and Matthias Schemmel (2003) on the one hand, and Adam Timmins (2019) on the other, have argued, however, that Kuhn's claim does not align well with his views on scientific change in *Structure*. *Black-Body Theory* argues, they claim, that the quantum-revolution happened not in 1900-1901 but rather in 1906: on this reading, the "rederivation [of Planck's law] by Einstein and Ehrenfest in 1906 from the assumption of the quantization of energy amounts to a scientific revolution[, and] this revolution essentially ended the crisis by discarding an old paradigm and establishing a new one" (Büttner et al., 2003, p. 38) (see (Timmins, 2019, p. 377) for a very similar claim). This entails, they continue, that preceding the work of Einstein and Ehrenfest there should be a period of pressing anomalies giving rise to crises that many recognized as problematic, and that the work of Einstein and Ehrenfest then all of a sudden overturned this situation, giving rise to a revolutionary Gestalt switch. A first problem, however, is that *Black-Body Theory* does not give any indications for any kind of crisis that preceded the quantum revolution in 1906. As Timmins puts it (see also (Büttner et al., 2003, p. 39)):

> Even if we accept Kuhn's argument that the date and occurence

[12] See Seth (2010) for an extensive discussion of Planck's principled style of doing theoretical physics and a comparison with Arnold Sommerfeld's more problem-oriented style.

> of the quantum revolution should be pushed back to 1906, we still find no signs of the crisis we would expect to find, given that the near-300-year-old paradigm of classical mechanics was about to be overthrown. (Timmins, 2019, p. 377)

Moreover, the book does not present the work by Einstein and Ehrenfest in 1906 as a revolutionary Gestalt switch either. In fact, according to Timmins, the whole crisis-revolution moment that is so central to *Structure* seems to be lacking, given that it is only with the Solvay conference of 1911, with which the book ends, "that the real crisis inspired by scientific work on the quantum emerges" (2019, p. 377).

3 A Practice-Focused Reading of Kuhn's *Black-Body Theory*

The discussion above shows that both debates sparked by *Black-Body Theory* concern how we are to understand paradigms. With regards to the first debate, concerning what Planck did in 1900-1901, this comes down to the question in how far we can say that Planck faithfully followed Boltzmann, as Kuhn is taken to have claimed (see the quote by Darrigol on page 6). And in the second debate as well, paradigms are a central issue: can we say, as a *Structure*-reading of *Black-Body Theory* would have us believe, that a new paradigm was established in 1906, even though there was no crisis nor a revolutionary Gestalt switch?

Most participants in these debates, we have seen, formulate their positions by distancing themselves from what they take to be Kuhn's continuist claim. In what follows, I will argue, however, that these readings of *Black-Body Theory* rely on a particular, theory-driven interpretation of paradigms, a reading of Kuhn's work that is contrasted by Joseph Rouse (1987) with a practice-focused reading.[13] My claim here is that *Black-Body Theory* can also be read in such practice-focused terms, and that on such a reading, Kuhn's claims are closer to the others' positions than often assumed.

3.1 Theory-Driven versus Practice-Focused

The central difference between theory-driven and practice-focused readings of Kuhnian paradigms concerns whether science is primarily a representationalist activity or not. The theory-driven view "treats science as the construction and appraisal of theories that aim to represent the world" (Rouse, 1987, p. 36). Some such theories can become fundamental for a discipline,

[13] Rouse himself does not use the terms theory-driven and practice-focused, but rather speaks about Kuhn_1, which I describe here as practice-focused, and Kuhn_2, which I here call theory-driven.

and in this way achieve the status of a paradigm, hence becoming "a set of theoretical doctrines constituting a world view" (Rouse, 1987, p. 27). Such a world view prescribes some beliefs and methods as essential, and it circumscribes a scientific community as "those scientists who accept the same paradigm and regard its theoretical doctrines as inviolable" (Rouse, 1987, p. 28).

Normal science, on this reading, takes on the form of removing the discrepancies between world view and world, or using the world view to elaborate representations of new phenomena. An essential feature of this view is that such "puzzle solving [...] cannot challenge the fundamental beliefs taken from the paradigm" (Rouse, 1987, p. 28). This only happens when, after repeated attempts, an important puzzle remains unsolvable, for then we enter a period of crisis, in which "the intelligibility of work within the field is increasingly threatened" (Rouse, 1987, p. 29). Scientists then try to overcome such crises by proposing solutions that are unorthodox from the point of view of the paradigm in crisis, and if such a solution is proposed, we enter a situation of polarization between the old and the new world view. Such situations cannot be resolved by rational argument, since such world views embody a scientist's most fundamental beliefs and values. The switch to a different world view is more like a sudden conversion, a Gestalt switch (Rouse, 1987, p. 29-30).

The practice-focused reading, on the other hand, does not conceptualize paradigms in representationalist terms: "[p]aradigms are not primarily agreed-upon theoretical commitments but exemplary ways of conceptualizing and intervening in particular contexts[, and a]ccepting a paradigm is more like acquiring and applying a skill than like understanding and believing a statement" (Rouse, 1987, p. 30). Paradigms, on this view, do not state what there is or how it has to be studied, but are rather concrete achievements that scientists, through training and education, can transform into skills that can help them open up new fields of research. What is essential here is that working with a paradigm does not commit scientists to any explicit consensus about fundamental beliefs: they share the use of certain techniques, concepts or theoretical claims, but it is not a requirement that they share a particular interpretation of these. Here Rouse (1987, p. 30-31) takes inspiration from the following claim by Kuhn:

> 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. [...] Indeed, the existence of a paradigm need not even imply that any full set of rules exists. (Kuhn, 1996, p. 44)

On this reading, scientists can disagree about the interpretation of a paradigm.

In fact, such disagreements, according to Rouse, are symptoms of a shared paradigm: it is a shared sense of "what is at issue, why it matters, and what must be done [...] [that] makes significant disagreements intelligible" (Rouse, 1987, p. 31). And conversely, the absence of disagreement does not entail that there is any consensus regarding a particular interpretation (Rouse, 1987, p. 32). As Kuhn puts it:

> The coherence displayed by the research tradiation 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. (Kuhn, 1996, p. 46)

This recognition of a shared sense of what is at stake also extends to what counts as a puzzle or anomaly: they are those issues that we recognize as something significant that is not yet understood in a clear way, i.e. we do not yet know how the issue relates to other issues that are taken to be of importance (Rouse, 1987, p. 32-33). If this proves persistent and we cannot find a way to deal with it, such issues can turn into crises, which are situations in which scientists "are no longer quite sure how to proceed: What investigations are worth undertaking, which supposed facts are unreliable artifacts, what concepts or models are useful guides for their theoretical or experimental manipulations" (Rouse, 1987, p. 34).

Such situations can be overcome by the recognition of a new scientific achievement as a possible exemplar for how to proceed. If this recognition proves successful, a new field of research possibilities and intelligible disagreements is then opened up, offered by a specific achievement taken to exemplify a (material or theoretical) model of approaching scientific problems. Studying and analysing such an exemplar brings scientists, more specifically, to search for ways to extend the paradigmatic approach in such a way that new issues can also be addressed (Rouse, 1987, p. 83). As Kuhn puts it:

> The [scientist] discovers [...] a way to see his problems as like a problem he has already encountered. Having seen the resemblance, grasped the analogy between two or more distinct problems, he can interrelate symbols and attach them to nature in the ways that have proved effective before. [...] Scientists solve puzzles by modeling them on previous puzzle-solutions, often with only minimal recourse to symbolic generalizations. (Kuhn, 1996, p. 189-190)

This modeling of new problem-solutions on a paradigmatic achievement should not be read as an algorithmic procedure offering rules for the resolution of new problems (cf. (Kuhn, 1996, p. 42-44) on why paradigms cannot be reduced to rules). It rather is an open-ended activity in which scientists, through "constructing, tinkering, and noticing" what seems to work and what not, try to extend the approach offered by the paradigmatic achievement in such a way that it allows them to construct a solution for new issues (Rouse, 1987, p. 40).

3.2 What did Planck do?

The central issue dominating the reception of Kuhn's discussion of Planck, we have seen, concerns the claim that if Planck was faithfully following Boltzmann, he should have arrived not at his own distribution law but rather, via the equipartition theorem, at the Rayleigh-Jeans law (see the quote on page 6). I will argue, however, that from a practice-focused reading, Kuhn's discussion of Planck's stance with respect to equipartition and the Rayleigh-Jeans law makes perfect sense: given their status at the time, it is understandable why Planck did not see it as an anomaly to be addressed. On the basis of this, I will then turn to the question of how, according to a practice-focused reading of Kuhn's *Black-Body Theory*, we should understand Planck as following Boltzmann. This will lead me to argue that, with respect to the notions of natural radiation and probability, Kuhn is much closer to the indetermination-position than is often assumed.

3.2.1 The Equipartition Theorem

As we have seen in section 3.1, on the practice-focused reading something can become an anomaly if it seems significant but it is not yet clearly understood how it relates exactly to the exemplar constituting the paradigm. Conversely, this also means that if something is not recognized as significant, it will not constitute an anomaly: "[a]n anomaly that does not show up in other contexts and does not seem closely connected with objects or techniques one regularly employs can easily be dismissed as an artifact" (Rouse, 1987, p. 33). Kuhn's discussion of equipartition and Rayleigh-Jeans suggests that he saw it in this way as well.

Lord Rayleigh first presented his law in 1900 in "a note [that] is both cryptic and incomplete" (Kuhn, 1987, p. 145). Planck does not mention it in 1900-1901, but he knew about it via papers of the experimenters who raised issues with Wien's distribution law (Klein, 1962, p. 466). These experimental results, however, also indicated to Planck that it was not really worth engaging:

> Rubens and Kurlbaum compared a number of proposed radiation formulas with their data and concluded that Rayleigh's was satisfactory only in the limit where it coincided with Planck's. Since the law, as proposed, was almost totally ad hoc, there was no further reason to take it seriously. Less than six months after it had been suggested, it was set aside. (Kuhn, 1987, p. 147)

In 1905 the Rayleigh-Jeans distribution law again popped up, this time in the work of James Jeans. Jeans' arguments for the law relied strongly on the equipartition theorem, and after he first developed them within the framework of his gas theory, he then extended them to the case of black-body radiation (Kuhn, 1987, p. 150). The way in which this was carried out, however, also entailed that, from Planck's perspective, the Rayleigh-Jeans law was seen as an artifact rather than an anomaly whose relation with Planck's theory had to be clarified. First of all, from the perspective of Jeans' derivation, the success of Planck's law, by that time "known to be in excellent quantitative agreement with experiment", was not explainable (Kuhn, 1987, p. 150). Moreover, accepting the theory behind Jeans' derivation would have meant "a high price for very little positive achievement", since it entailed that the thermodynamic arguments behind many of the well-established black-body radiation laws would have to be replaced (Kuhn, 1987, p. 150). Finally, the validity of the theorem on which Jeans heavily relied was also disputed, especially with regards to whether it could be taken to hold outside of the gas case. For these reasons, according to Kuhn, Planck did not see the equipartition theorem or the Rayleigh-Jeans law as pressing anomalies, and neither did many others: "[t]he Rayleigh-Jeans law [...] did not yet pose problems for more than two or three physicists" (Kuhn, 1987, p. 152).

On a theory-driven reading, ignoring such disagreeing claims is problematic, since such a disagreement is taken as a pressing anomaly: as we have seen on page 10, the theory-driven reading does not really allow for disagreements. On a practice-focused reading, however, such a disagreement in itself does not yet constitute an anomaly. This is only the case if the claim is also recognized as bearing on the paradigmatic achievement in ways that seem significant but not yet well understood. If this is not the case, it is perfectly reasonable, as Rouse puts it, that "[s]cientists are ignored, or read out of the community, not for disagreeing with others but for doing work that does not fit in with what others are up to" (1987, p. 32). As such, on the practice-focused reading of *Black-Body Theory*, it is not "the gravest flaw" in Kuhn's argumentation that Planck did not arrive at the Rayleigh-Jeans law even though he was faithfully following Boltzmann (see the quote on page 6).

Moreover, Kuhn also points out that a derivation of the Rayleigh-Jeans law would require more than just following Boltzmann faithfully and setting ϵ to zero, as Darrigol seems to assume. Particularly the equipartition theorem was an issue, since it relied on some "special hypotheses and approximations which were themselves often doubted", and because "even if equipartition could be shown to apply to general mechanical systems, one might legitimately doubt its applicability to the electromagnetic displacements postulated by Maxwell's theory" (Kuhn, 1987, p. 151). In what follows, I will argue that, according to Kuhn, Planck himself was undecided with respect to such micro-structural hypotheses and assumptions – and hence, that Kuhn was much closer to the indetermination-position than assumed. Given this, we can then come to see why, on a practice-focused reading of Kuhn's account, Planck following Boltzmann faithfully does not have to mean that he should have arrived at the Rayleigh-Jeans law.[14]

3.2.2 Natural radiation and probability

As we have seen in section 2.2.1, the indetermination-position tries to make sense of why Planck did not end up with the Rayleigh-Jeans law, while faithfully following Boltzmann, by claiming that Boltzmann's combinatorial arguments allowed Planck to follow Boltzmann formally without any commitments regarding the resonator dynamics (see the quote by Badino on page 7).

Kuhn's discussion of how Planck follows Boltzmann makes clear, however, that for Kuhn as well, the central point is that Planck follows Boltzmann in a formal sense. This becomes clear when we look at Kuhn's discussion of how Boltzmann's approach to the definition of molecular disorder served Planck to elaborate his analogous notion of natural radiation. As Kuhn puts it, Boltzmann introduces the concept by means of "examples [that] do not, of course, define molecular disorder or even make the concept very clear" (1987, p. 66). Boltzmann then does introduce a definition, in the form of the stipulation that any molecular arrangement for which a specific equation is valid is to be called molecularly disordered. It is this approach to definition, by means of a formal validity-condition, that Planck takes over from Boltzmann:

> That device – defining a concept as the condition required for the validity of a previously derived equation – is precisely the one Planck would use two years later to define his own related concept, natural radiation. In that case, as in this, its effect is to guarantee that the most probable distribution will be actualized. (Kuhn, 1987, p. 66)

Kuhn stresses this aspect of Planck's definition of natural radiation in different places (1987, p. 88;121), and he then argues that, for his application of Boltzmann's combinatorial arguments to the case of black-body radiation as well, Planck makes use of the same approach to define probability. Here, Kuhn points out, Planck deviates from Boltzmann's approach. Boltzmann, in his gas theory, could appeal to a specific theorem, Liouville's theorem, for a definition of equiprobable states (Kuhn, 1987, p. 55-56).[15] In Planck's case, however, no similar theorem was available. Only experiment could

[14] I would like to thank an anonymous reviewer for pushing me on this topic.

[15] See (Badino, 2009, p. 94) for a discussion of how Boltzmann uses this theorem.

provide information about which states could be taken as equiprobable, as follows:

> [E]xperimental confimation must provide information about the relative probability of the various possible sets of coefficients in the Fourier series that specifies the change of resonator configuration with time. This last characteristic is, of course, the one that renders Planck's "definition" of probability a refinement of his concept of natural radiation. An assertion about the relative probability of different sets of Fourier coefficients is an hypothesis about the relative frequency with which particular sorts of resonator motions occur in nature. It is thus an assertion of essentially the same sort as the one Planck had used to introduce natural radiation before. (Kuhn, 1987, p. 122)

These quotes show that Kuhn did not see Planck as just faithfully following Boltzmann. Planck was rather trying to extend Boltzmann's approach to a new field, and this required Planck to deviate, for example, from Boltzmann in how he conceptualized the notion of equiprobable states. Moreover, what is central here is that what Planck did take over from Boltzmann were primarily formal-mathematical devices. He did not faithfully follow Boltzmann, for example, with respect to the physical specification of the micro-states that make up the macro-state of a black-body radiation system. As Kuhn puts it:

> The recourse to combinatorials provided information only about the equilibrium distribution of *resonator* energy with frequency. Planck's concern, however, had been and remained with radiation. His resonators were imaginary entities, not susceptible to experimental investigation. Their introduction was simply a device for bringing radiation to equilibrium, and it was justified, not by knowledge of the physical processes involved, but by Kirchhoff's law, which made the equilibrium field independent of the equilibrium-producing material. (Kuhn, 1987, p. 117-118)

Kuhn thus subscribes to the indetermination-claim that Planck's theory did not commit him to any specific position with respect to the dynamics of his resonators. Kuhn does indeed claim that Planck believed that their energy should not be understood in quantized terms, but this in itself does not entail that Planck was working with a fully worked out, continuous resonator dynamics on the basis of which he then arrived at his claims about the macro-state of black-body radiation. As Kuhn (1987, p. 131) points out, the way in which the micro-state gave rise to the necessity that the constant h fixed the size of the energy elements ϵ was for Planck an open question, one that Planck believed could be investigated further by means of research into the dynamics of the electron (1987, p. 131-133).[16] As such, Kuhn is in fact very close to Darrigol's indetermination-claim that "for Planck the deeper significance of the energy-elements was an open question, having to do with electrodynamics at a finer, non-observable scale" (2001, p. 234).

The above suggests that Kuhn's presentation of Planck's work, and the role played by Boltzmann in it, should be read in a practice-focused way. It is not the case that Boltzmann's theory acted as a set of theoretical commitments that laid down the rules for Planck. It rather provided Planck with formal instruments that could be used to see how Boltzmann's approach could be extended, in an analogical and open-ended way, to the study of black-body radiation. This use of Boltzmann as a paradigm, moreover, did not commit Planck to any kind of specific interpretation of the formal apparatus used, as Kuhn argues by emphasizing that Planck did not express any explicit commitments with respect to the universal constant h or the dynamics of the resonators. As such, that Planck followed Boltzmann, on Kuhn's account, does not mean that he should have arrived at the Rayleigh-Jeans law, since such a derivation would have required micro-structural hypotheses and assumptions, whereas Planck, on a practice-focused reading of Kuhn's work, was uncommited with respect to such claims.

3.3 The Relation to *Structure*

In section 2.2.2, we have seen that the biggest issue with Kuhn's claim that *Black-Body Theory* fits the structure of *Structure*, is that Kuhn's narrative lacks any crises or revolutions, and that the period it covers seems more like a pre-revolutionary normal science phase: as Timmins puts it (see the quote on page 9), the real crisis, and subsequent revolution, only seems to occur after the 1911 Solvay conference, with which Kuhn's book ends (see also (Büttner et al., 2003, p. 39)). I will argue, however, that the narrative provided in *Black-Body Theory* does fit a practice-focused reading of *Structure*. My starting point here will be the fact that Kuhn does not see crises as a necessary condition for revolutions:

> Nothing important to my argument depends, however, on crises being an absolute prerequisite to revolutions; they need only be the usual prelude, supplying, that is, a self-correcting mechanism which ensures that the rigidity of normal science will not forever go unchallenged. (Kuhn, 1996, p. 181)

The phrasing in terms of a self-correcting mechanism suggests that, as Rouse puts it, the categories of normal science, crisis and revolution are "not historical periods but ways of practicing science" (1987, p. 34). While some

[16]The reason for this is that by means of his new theory, Planck had been able to compute a value for the electric charge e from experimental values for h and k (Kuhn, 1987, p. 111).

scientists working with a particular paradigm may have the impression that it is in crisis, because they do not see how to proceed further, others may continue working with the same paradigm in a normal science way. This difference in categorization, according to Rouse, depends on the scientist's historical judgment: "[h]ow revolutionary a new development is depends in part upon how one interpreted the paradigms preceding it" (1987, p. 35).

Read in this way, the claim that Planck's black-body theory seemed like normal science to most scientists is perfectly compatible with the claim that, at the same time, for some scientists "[a] crisis, to the extent that there was one, resulted from the difficulties in reconciling Planck's derivation with the laws of classical physics" (Kuhn, 1987, p. 363).[17] These experiences by individual scientists that they could not proceed further with the foundations of Planck's black-body theory can perfectly well be described as a revolution, which, in Kuhn's terms, is a "sort of change involving a certain sort of group commitment" (1996, p. 181): these scientists realized that, if they were to uphold Planck's distribution law as a paradigmatic scientific achievement, their commitments to how to proceed in fundamental physics had to change.

This brings us to a second issue, namely that Büttner, Renn and Schemmel (2003, p. 50), and Timmins (2019, p. 377), present Kuhn's *Black-Body Theory*-narrative as entailing that Einstein and Ehrenfest *established* the new quantum paradigm in 1906. The problem with this reading is that Kuhn himself does not seem committed to this claim. He rather states that "[t]he *start of the revolution* that produced the old quantum theory is moved from the end of 1900 to 1906" (1987, p. 363; emphasis added). The exact phrasing of this claim can well be read as stating that 1906 only saw the start of a process of paradigm establishment, unfolding over time. This reading is supported by the fact that, according to Kuhn, it was only with the 1911 Solvay conference that most of the scientists concerned were convinced that their practice should, in one way or another, incorporate the quantum, without necessarily agreeing on how to interpret it:

> 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 new physics would be, the men concerned all recognized that there could be no turning back. (Kuhn, 1987, p. 144)

Kuhn is read as claiming that in 1906 a new paradigm was established because both Büttner, Renn, and Schemmel (2003, p. 41) and Timmins (2019,

[17] See Garber (1976) and (Kuhn, 1987, p. 134-140) for discussions of how Planck's work was generally received at the time

p. 386) assume that, if *Black-Body Theory* has to be read in terms of *Structure*, the revolution should occur all of a sudden, in the form of a Gestalt switch. The problem, however, is that Kuhn's use of the image of Gestalt switches primarily concerns the psychology of an individual scientist's perception (see especially chapter X (Kuhn, 1996, p. 111-135)). It does not necessarily apply to how a revolution, historically, unravels itself: as Kuhn puts it, the transition of a new scientific approach to maturity "need not (I now think should not) be associated with the first acquisition of a paradigm" (1996, p. 179).[18]

As such, the personal recognition that some kind of quantum concept is needed can well be described in Gestalt terms. This does not exclude, however, that from a historical perspective, the revolution from classical to quantum was a process that spanned the period between 1905 and 1912 and that involved attempts to extend Planck's black-body work to new fields such as specific heats, the structure of radiation or atomic structure, as well as community building through events such as the 1911 Solvay conference (see chapter IX of *Black-Body Theory* (1987, p. 206-232)). And it resulted in an established quantum paradigm, to be understood not as a list of explicit theoretical commitments, but rather as a recognition by a community of scientists that the elaboration of fundamental physical theories had to be practiced in terms of the quantum, an approach for which Planck's theory had, by that time, become an exemplar through several reconstructions of how Planck supposedly had proceeded in 1900-1901 (see e.g. Kuhn's (1987, p. 102-103;205) discussion of Lorentz's 1910 reconstruction, or of the influence of the second edition of Planck's *Lectures* in chapter X). This does not mean, moreover, that all involved agreed exactly on how the paradigm should be interpreted, but rather that new techniques and puzzles were available for scientists to engage with in a normal science kind of practice.

The above indicates that, underlying the claim that *Black-Body Theory* does not fit the structure of *Structure* is a theory-driven reading of Kuhn's work: Kuhn is read as claiming that the quantum paradigm was established with the theories of Einstein and Ehrenfest, and that the theoretical claims of these theories then laid down the explicit commitments of the new paradigm. On such a reading, it is indeed difficult to discern any crises or revolutions, especially if these notions are interpreted as community-recognized pressing problems that disappear all of a sudden from the debate via a Gestalt switch. We have seen, however, that a practice-focused reading of Kuhn's work allows for a different interpretation according to which crises and revolutions can be extended over time. If *Black-Body Theory* is read in this way, it seems that we can, after all, claim together with Kuhn that there is a certain fit

[18]This could be connected with the claim, made by Kuhn in *The Road Since Structure* (2000, p. 57), that the concept of a revolutionary gestalt switch applies primarily to the historian's perspective, rather than to the scientist's. I would like to thank an anonymous reviewer for pointing out this passage to me.

with *Structure*.

4 Concluding Remarks: Suggestions for Further Research

In the previous section, I have argued that if Kuhn's *Black-Body Theory* is read in a practice-focused way, some of the criticisms raised no longer hold. The question can be raised, however, what the point is of trying to understand how Kuhn's interpretation of Planck is itself to be interpreted. In line with how Rouse presents his practice-focused reading of Kuhn, however, I believe that such interpretative activity can be valuable insofar that it can open up new research posibilities.[19] To conclude, I will therefore sketch, in a tentative way, how a practice-focused reading of *Black-Body Theory* can contribute to this.

The development of a paradigm The theory-driven reading of Kuhn's *Black-Body Theory* is very much focused on whether the work of a few individual theoretical scientists, especially Planck, Einstein and Ehrenfest, was decisive in the establishment of the quantum. And their primary interest, moreover, is in understanding the influence of the theories formulated by them. If, on the other and, we focus on the quantum-paradigm as primarily a practice, its establishment can be reconceptualized as a historical process that involved not only scientists in the role of theoreticians, but equally well in the role of experimentalists, conference organizers, journal editors, educators, etc. Scientists taking up these roles all contributed to the fact that, over time, a community emerged that came to recognize Planck's approach to black-body radiation as an example to follow and to extend. This reading, moreover, allows for the possibility that there was no shared interpretation of the quantum, and hence it opens up the question how these scientists, in these different roles, handled disagreements and, in their attempts to address them, drew and redrew the boundaries of the newly emerging paradigm.

One particular such discussion, that had not yet received that much attention, is the debate between Johannes Stark and Arnold Sommerfeld about the application of the quantum to the experimental and theoretical

[19] Rouse points out that both interpretations are to be found in Kuhn's work, and that Kuhn himself was probably split between the two. Rouse still thinks it worthwhile to elaborate a practice-focused reading, however, since it can help "the development of an interpretation of science whose roots in Kuhn are too often unnoticed" (1987, p. 27). Since then, a few other authors have followed in Rouse's tracks: see Patton (2018) for a recent overview of different practice-focused readings of Kuhn's work. And while they do not always explicitly describe themselves as such, I think that the work of e.g. Bruce Wheaton (1983), Richard Staley (2008; 2016) and Suman Seth (2004; 2010) on the history of relativity and the quantum can be described as in line with the practice-focused reading of *Black-Body Theory* elaborated here.

study of X-rays and canal rays.[20] While Stark believed that these rays could only be studied in quantum-terms, Sommerfeld claimed, in line with his commitment then to the electromagnetic worldview, that they could be accounted for in terms of the electron. Many physicists such as Planck, Wien and Einstein also got involved, but after a while, the debate got rather personal and vicious: Einstein summarized it in a letter to Jakob Laub as Stark producing pure dung and Sommerfeld overestimating the evidential value of particular phenomena (Wheaton, 1983, p. 130).

What is significant here is that we are dealing with a disagreement about how to approach particular phenomena, and that shortly after this dispute, Sommerfeld is taken to have abandoned the electromagnetic worldview in favour of a particular quantum-approach (Eckert, 2013, p. 176). This episode therefore seems to allow for the study of how paradigms are delineated, how they can transform over time, and how scientists, from different positions, contribute to this process.[21] Moreover, its importance also lies in the fact that it concerns the relation between the electron and the quantum, which Planck, as we have seen (footnote 16), saw as the way forward for the further development of his black-body theory. Studying this episode can therefore also provide us with more insight into the relation between electron theory and the quantum, which, according to Seth (2004), was an important but as of yet understudied factor in the development of the quantum-paradigm.[22]

The construction of an archive The first edition of *Structure* (1962) appeared when Kuhn was working on *Sources for History of Quantum Physics*, "an archival project that sought, both by interviewing and by making copies of original manuscripts, to preserve records on which future studies of the subject might be based" (1987, p. xi). As Badino (2016) shows, the archive has provided an invaluable source for much research in history and philosophy of physics. What has received less attention, however, is the way in which Kuhn's history and philosophy of science influenced, and was at the same time influenced by, his work on the archive: did it have any repercussions for the delination of the topics that were to be included, for the period

[20]Discussions of it can be found in Hermann (1966, 1967), (Mehra and Rechenberg, 1982, p. 99-113), (Wheaton, 1983, p. 116-132), (Kuhn, 1987, p. 222-226), and (Eckert, 2013, p. 171-176).

[21]Many of those involved spoke not only from a position of theoretical physicist: Stark was editor of the *Jahrbuch der Radioaktivität und Elektronik* (Mehra and Rechenberg, 1982, p. 100), Wien and Planck of the *Annalen der Physik* Hoffmann (2008), and Sommerfeld had recently obtained leadership of one of the few theoretical physics departments in Germany (Eckert, 2013, p. 171).

[22]What makes this extra fascinating, from an integrated history and philosophy of science point of view, is that this claim by Seth in turn gave rise to a discussion with Shaul Katzir about how to conceptualize a worldview philosophically: see Katzir (2005) and Seth (2005). This discussion could, it seems to me, benefit very significantly from a more elaborate understanding of what constitutes a scientific worldview, and how it relates to the practice-focused reading of a paradigm elaborated by Rouse.

covered, for who was to be interviewed, or for which questions to ask? And how did Kuhn's experiences of the archival work in turn influence his views on the early history of the quantum, which he covered not only in *Black-Body Theory* but also in a (1969) article with John Heilbron on Niels Bohr's work on the constitution of the atom, and, indirectly, through the work of his student Bruce Wheaton on the empirical roots of the wave-particle duality (1983). That there seem to be significant links between a practice-focused reading of Kuhn's historical-philosophical work and his archival work is suggested by the following claim from Anke te Heessen, who seems to be one of the few to have investigated Kuhn's archival work in detail (see also her (2018) article):

> These interviews did not only produce a new source, one that recorded memory. They also called forth the experience of the questioner, as much as they summoned the memories of the questioned. Kuhn's comments on going "into the field" and on the "hell" of interviewing marked a moment of conscious involvement, a convergence of (physics) research and the people who did it with an intensified awareness of "science as practice." "Science as practice" refers here not only to the past events but equally to Kuhn's own experience of fieldwork. To put it succinctly: the project made visible a historiography that was oriented towards practice. (te Heesen, 2020, p. 96)

In this way, we come to see how engaging in a practice-focused reading of Kuhn's work is not solely a form of Kuhn-exegesis. Bringing together Kuhn's work on *Structure*, on *Black-Body Theory* and on the *Sources for History of Quantum Physics* equally well offers us a way to study and evaluate how Kuhn, and those who have responded to his work in one way or another, have attempted to put the integration of history and philosophy of science into practice.

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