REFERENCES

FEYERABEND, P. [1958]: 'An Attempt at a Realistic Interpretation of Experience', *Proceedings of the Aristotelian Society*.
FEYERABEND, P. [1962]: 'Explanation, Reduction and Empiricism' in Feigl and Maxwell (*eds.*): *Minnesota Studies in the Philosophy of Science*, **3**.
FEYERABEND, P. [1970]: 'Consolations for the Specialist' in Lakatos and Musgrave (*eds.*): *Criticism and the Growth of Knowledge*.
FEYERABEND, P. [1975]: *Against Method*. London: New Left Books.
FEYERABEND, P. [1977]: 'Changing Patterns of Reconstruction', *British Journal for the Philosophy of Science*, **28**, 351–69.
HANSON, N. R. [1958]: *Patterns of Discovery*. Cambridge University Press.
HANSON, N. R. [1963]: *The Concept of the Position*. Cambridge University Press.
HOWSON, C. (*ed.*) [1976]: *Method and Appraisal in the Physical Sciences*. Cambridge University Press.
KUHN, T. S. [1962]: *The Structure of Scientific Revolutions*, University of Chicago Press.
KUHN, T. S. [1977]: *The Essential Tension*. University of Chicago Press.
LAKATOS, I. [1978]: *Proof and Refutations*. Cambridge University Press.
LAKATOS, I. [1978a]: *The Methodology of Scientific Research Programmes. Philosophical Papers, Volume 1*. Cambridge University Press.
LATSIS, S. (*ed.*) [1976]: *Method and Appraisal in Economics*. Cambridge University Press.
POPPER, K. R. [1963]: *Conjectures and Refutations*. London: Routledge and Kegan Paul.
RADNITZSKY, G. and ANDERSSON, G. (*eds.*) [1979]: *Progress and Rationality in Science*. Dordrecht: D. Reidel.
SABRA, A. I. [1967]: *Theories of Light from Descartes to Newton*. London: Oldbourne.

KUHN AND THE QUANTUM CONTROVERSY*

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1   INTRODUCTION

One of the major publishing events of the year in the history and philosophy of science has occurred with the recent appearance of Kuhn's book. The immediate interest it has aroused is reflected in the special symposium *Isis* (vol. 70, 1979) has just published including reviews by Martin J. Klein, Abner Shimony and Trevor Pinch. The backgrounds of these participants in history, philosophy and sociology suggest the wide range of audience that follow Kuhn's work. Indeed, for historians of physics, Kuhn's latest book represents a major and already controversial reassessment of the introduction of the quantum into physics. As such it touches on an issue of singular importance, one whose resolution will influence our whole account of early quantum theory including the roles of Planck, Ehrenfest, Lorentz, Einstein, Sommerfeld and Bohr in setting out the new physics. Philosophers will want to know what light this study throws on

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* Review of THOMAS S. KUHN [1978]: *Black-Body Theory and the Quantum Discontinuity, 1894–1912*. Oxford: Clarendon Press. £12.50. Pp. 356. Page references without date *below* are to this book.

Kuhn's previous work on the nature of scientific change. More generally, philosophers will ask what this case study illustrates about the succession of scientific theories.

I should add immediately that in making this detailed case study, Kuhn is not explicitly adding to the literature surrounding his earlier volume, *The Structure of Scientific Revolutions* or many of the essays reprinted recently in *The Essential Tension*. This is no accident, for as Kuhn often points out himself, he wears two hats, one as a historian of science and another as a philosopher of science. In *The Quantum Discontinuity* he is writing as a historian of modern physics. Of course, he is sensitive to certain philosophical issues that are relevant to the historical figures involved in the story. He is not, however, explicitly concerned with the philosopher of science's problems of providing a general model for theory change or accounting for the commensurability of scientific theories. Nowhere in this book does Kuhn refer to any specific model of scientific change. There is no reference to his own philosophical work or even to any of the terms he has introduced, such as paradigm, disciplinary matrix, crisis, revolution, or anomaly. As we shall see, this does not mean that Kuhn's earlier ideas are completely absent in this work. But before we can treat these issues—either historical or philosophical—it is necessary to review briefly the early history of the black-body problem.

One way to state the black-body problem is as follows: in a reflecting cavity containing radiation in equilibrium with the walls at a given temperature, what will the intensity of the radiation be at each frequency along the whole spectrum? The first steps towards placing the problem in its modern perspective were made by Gustav Kirchhoff in 1859. He was able to prove that, quite independently of the material of which the cavity was built, there existed a universal function $\rho(v, T)$ giving the intensity of radiation at the frequency v and at the temperature T. The twin questions in need of solution then were (1) What was the shape of this universal function, and (2) What physical mechanism led to the universality of this function? Progress came in several successive steps towards a resolution of the first question. In 1879 the experimentalist Josef Stefan conjectured from his data that the total radiation in the cavity could be given as a function of $\rho(v, T)$ as:

$$\int \rho(v, T)\,dv = \sigma T^4 \tag{1}$$

with σ a constant. Theoretical grounding for this relation came some five years later from Ludwig Boltzmann who approached the problem by invoking the techniques of thermodynamics. Then in 1893, in his so-called displacement law, Wien was able to place an even stronger constraint on the still unknown function $\rho(v, T)$ by showing that $\rho(v, T)$ could be treated as v^3 *times* a function of a single variable v/T:

$$\rho(v, T) = v^3 \phi(v/T), \tag{2}$$

where ϕ was a still unknown function of one variable.

Improvements in experimental technique were needed before further advances could take place. S. P. Langley and Friedrich Paschen soon provided the needed technical innovations and thereby were able for the first time to provide fairly accurate spectra of black bodies, probing down from the visible to the infra-red.

From his data Paschen suggested a form for $\rho(v, T)$ which soon reached Wien. Wien was struck by the similarity of Paschen's law with some of his own speculative theoretical work on ρ and, thus encouraged, published in 1896 a somewhat dubious derivation of the now-famous Wien distribution using an analogy with the Boltzmann distribution:

$$\rho(v, T) = av^3 \, exp(-bv/T), \tag{3}$$

with a and b experimentally determined constants.

However, even before this law was published, Planck's interest in the black-body problem had already been ignited. In an often cited passage from his scientific autobiography, Planck wrote that the problem of determining the nature of $\rho(v, T)$ 'represents something absolute, and since I had always regarded the search for the absolute as the loftiest goal of all scientific activity, I eagerly set to work.'

Kuhn's new volume traces Planck's work on this problem—to understand his scientific motivation for his interest and the motivation that determined the type and style of research he undertook. The story of Planck and the quantum has, of course, been told before, by Martin Klein in the greatest detail, but various aspects of the evolution of Planck's thought have also been traced by Jammer [1966], Hiebert [1968], Rosenfeld [1936], Kangro [1976], Goldberg [1976], Hermann [1971], Garber [1976], and others.

Kuhn acknowledges these debts in the preface, somewhat indirectly in the text and explicitly in the notes. Nonetheless, in general he refrains from directly addressing individual authors and spends very little time explicitly discussing the work of other historians of science. This sometimes makes it unclear precisely where Kuhn is introducing new information or interpretation and where he is presenting less contentious material in more detail. It is clear, however (as Kuhn acknowledges in the preface), that the background against which his book must be read is chiefly the collection of publications Martin Klein has written over the past twenty years, along with the extended study of black-body experimental work by Hans Kangro. In these works and in Klein's biography of Paul Ehrenfest, one finds the "standard" interpretation against which Kuhn presents his new analysis.

By comparing and contrasting the two interpretations explicitly, I want first in this review to point out where Kuhn is filling out the picture sketched by Klein and Kangro. Second, I would like to show where Kuhn is really innovative; and finally, to suggest why I think it has come to pass that two of the foremost historians of modern physics have arrived at such seemingly irreconcilable conclusions about the interpretation of Planck's work and the history of the quantum. The issue here is not how much credit to give whom for the discovery of the quantum discontinuity; it is that an understanding of how and when the quantum hypothesis was introduced should shed light on the factors that were crucial in such a major change in the foundations of physics.

2 PLANCK'S WORK BEFORE 1900

Despite the new interpretation Kuhn places on the emergence of the quantum discontinuity, his account begins on neutral territory adding to and filling out the account of Klein, Kangro, Jammer, *et al*. This agreed on part of the story

may be outlined as follows. Planck was concerned early in his career with the thermodynamics of Carnot and Clausius. Their work inspired Planck to write his doctoral dissertation on the second law of thermodynamics. In his thesis, Planck generalised Clausius's formulation of the second law, and began to study its many applications.

The law of the conservation of energy and the law that the entropy of an isolated system must increase are what Planck called 'absolutes'. These were statements about the world which could be taken as universally and without exception true. By contrast, Boltzmann interpreted the increase of entropy to be simply the tendency of a system to move towards its most probable configuration. One consequence was Planck's early antipathy towards the kinetic theory of gases (see Hiebert [1968]), as is evident from a paper Kuhn cites, written in 1891. Planck asserts:

> Anyone who has studied the works of Maxwell and Boltzmann—the two scientists who have penetrated most deeply into the analysis of molecular motion—will scarcely be able to escape the impression that the remarkable physical insight and mathematical skill exhibited in conquering these problems is inadequately rewarded by the fruitfulness of the results gained (Kuhn, p. 22).

Conjoined with Planck's disinclination to use the molecular hypothesis was his more personal concern that Boltzmann's statistical presentation of the second law clashed directly with his own 'absolute' interpretation. As a result, Planck was determined to discover a non-statistical explanation of the second law as it applied to particular problems.

From 1894 on, the problem that Planck felt was most in need of solution was one of the great problems in spectroscopy: how to explain the universal, experimentally determined distribution $\rho(v, T)$ of black-body radiation. His first attempts in 1895–6 to find a mechanism by which radiation came to the distribution $\rho(v, T)$ brought him to electrodynamics. Since Kirchhoff had shown $\rho(v, T)$ to be independent of the medium of the cavity, Planck felt free to base his work on the particularly simple assumption that the equilibrating mechanism was the radiation of Hertzian oscillators in the walls. As the charge oscillated, it would radiate and thus lose energy entirely through a non-mechanical process. It was Planck's hope that some suitable treatment of these charged oscillators would yield an irreversible process, creating from an arbitrary initial distribution the desired universal distribution Kirchhoff had shown must exist.

Planck articulated his electromagnetic investigations in a series of articles from 1897 to 1899. These became more and more elaborate but despite his best attempts, Planck found that initial conditions were always possible which would drive the distribution of intensities towards a lessening rather than an increase in entropy. Boltzmann could not have been overly surprised at this result: it is the precise analogue of a problem in molecular disorder—gases tend to become more disordered in general but specific initial conditions are always conceivable that allow a decrease in entropy. Just such a problem had led Boltzmann to his statistical interpretation. After some resistance, Planck finally conceded to Boltzmann's criticism, and statistical considerations began to enter Planck's work.

The juncture at which Planck introduces statistics is also the point at which he begins in earnest his study of Boltzmann's papers. One of the merits of the first section of Kuhn's book is that it describes, in some detail, the precise formal parallels between Boltzmann's work of 1877 and Planck's 1898 work. This also serves to buttress Kuhn's contention that Planck was influenced by the Boltzmann treatise somewhat earlier than most authors have suspected. Foremost among these parallels is Planck's use of the concept of 'natural radiation' which plays much the same role as Boltzmann's 'molecular chaos': both amount to the supposition of initial conditions which will guarantee an increase of entropy with time.

Finally, in 1899, by making use of his oscillator model and statistical methods, Planck's program seemed to have met with success. In that year he was able to supply a physical grounding of Wien's law by defining a quantity S_t which he defined as the electrodynamic entropy. The purpose of this definition was to allow him to derive Wien's law by making use of the thermodynamic relation $\partial S_t/\partial E = 1/T$. The difficulty now lay not with the initial conditions but in demonstrating the uniqueness of this function S_t, and in showing that it corresponded in its limiting behavior to the thermodynamic entropy. Resolving these two remaining problems, involving the uniqueness and justification of the basis of the electrodynamic entropy S_t, were the goals of Planck's paper of March 1900 (Planck [1900a]). However, experiments soon struck down his hopes; the recent work of Lummer and Pringsheim excluded the Wien distribution as incompatible with experiment. By constructing the first experimental black cavity and examining the spectrum of radiation in it, they had increased the accuracy of their experiments far beyond the then current capabilities of measurements of hot metals in the sun, deep in the infra-red region of the spectrum. As a result, they were able definitively to rule out the Wien-Planck distribution as irreconcilable with the evidence.

Despite the setback, Planck's new programme was sufficiently worked out by then to allow for a generalisation that was presented on 19 October 1900 at the German Physical Society (Planck [1900b]). At that meeting he announced his new candidate for the distribution function, justifying it by methods like that of March 1900, except for a modification of S_t. The function he proposed as the distribution $\rho(v, T)$ is the now famous Planck distribution,

$$\rho(v, T) = hv^3/(exp(-hv/kT)-1). \qquad (4)$$

This candidate for the form of the distribution was almost immediately verified by further experiments. Still Planck lacked a physical justification for the distribution and, as he later recalled,

> . . . on the very day when I formulated this new law, I began to devote myself to the task of investing it with a real physical meaning, and that issue led me of itself to the consideration of the relationship between entropy and probability, and thus to Boltzmann's line of thought (cited in Kuhn, pp. 98–99).

Kuhn rightly observes that Planck already had been making use of 'Boltzmann's line of thought' and statistics; what is new with this paper, as Kuhn points out, is a commitment to combinatoric arguments. But it is here, with the interpretation of Planck's justification of his new distribution law that Kuhn makes his

most controversial claim. Indeed, on the pages 98–140 lies the heart of his thesis. For it is here that Kuhn argues that Planck *never* intended in 1900–1 to introduce the quantum discontinuity into physics and that it was not until much later, around 1910, that Planck would be fully committed to the quantum point of view. Thus the arguments of both Kuhn and Klein depend critically on the years 1900–1.

3   CLASSICAL PHYSICS OR QUANTUM PHYSICS?

What happened in 1900–1? Is Klein correct in asserting, 'On December 14, 1900, Max Planck presented his derivation of the distribution law for black-body radiation to the German Physical Society, and the concept of energy quanta made its first appearance in physics' (Klein [1962], p. 459). By this Klein means that Planck was asserting that oscillators could only take on certain restricted amounts of energy, ε, 2ε, 3ε and so on. Or should we believe Kuhn, who summarises his thesis as follows:

> My point is not that Planck doubted the reality of quantization or that he regarded it as a formality to be eliminated during the further development of his theory. Rather, I am claiming that the concept of restricted resonator energy played no role in his thought until after the *Lectures* (Planck [1906]) were written (Kuhn, p. 126).

As sharp as this opposition sounds, even here Kuhn and Klein agree on more than may be apparent. First, Kuhn and Klein agree that in the 1900–1 work Planck was resorting to the combinatoric arguments of Boltzmann, more specifically to those of 1877, adapted to the problem of radiation. Second, they agree that at no point did Planck ever consider treating light as a collection of discrete particles—the entire dispute is over whether he quantised the resonator energies. Third, both Kuhn and Klein assert that Planck singled out for special attention his introduction into physics of a new universal constant.

But an even more important point of agreement is that both Kuhn and Klein more or less accept a reconstruction of Planck's original application of combinatorics to the radiation problem in 1900 (Planck [1900c]) which was first advanced by Rosenfeld. (See Rosenfeld [1936], cited by Kuhn, pp. 100-1 and by Klein [1962], p. 474.) According to this account, Planck began by searching for and finding a suitable 'entropy' S, which would yield his distribution *via* the thermodynamic relations. Then, the argument goes, using Boltzmann's definition Planck wrote

$$S = k \ln W$$

by which Planck (unlike Boltzmann) *defined* W, the number of possible system configurations. When the appropriate entropy S is inserted and the equation inverted to solve for W, a further restriction is needed to guarantee that W be composed of integers. That restriction is that the elementary division be proportional to v.

$$\varepsilon = hv \tag{5}$$

by which W takes the form

$$(N+P-1)!/((N-1)!\,P!). \tag{6}$$

This is precisely the number of distinct ways P identical things can be placed in N distinguishable boxes.

With this initial result, Planck next had to interpret his expression for W as a real probability. Kuhn clarifies this attempt, amending the account given by Klein in several significant respects. First, he points out that there are really two separate derivations of the probability in Planck's early works of 1900–1, one presented in his 14 December 1900 speech and the other in the *Annalen* articles of 1901 (Planck [1900*a*, *b*]). Second, Kuhn shows that when Planck deviates from Boltzmann's derivation of 1877 it is not the result of confusion; Planck has adopted a different but nonetheless coherent derivation.

Planck's derivation of his law differs from the now familiar one which resembles the Boltzmann derivation of the velocity distribution for a gas molecule. The 'modern' derivation can be presented as follows. Suppose we have a set of N resonators at a fixed frequency v, where w_j of the oscillators have j energy elements $\varepsilon = hv$. Then $\Sigma_j w_j = N$, the number of energy elements $P = \Sigma_j j w_j$, and the total energy $E = P\varepsilon$. It follows that the number of distinguishable arrangements of independent energy elements over the distinguishable oscillators for a given distribution $\{w_j\}$ will be

$$W = N!/(w_0!\dots w_M!).\qquad(7)$$

Assuming each configuration of energy elements to be equiprobable, W will be proportional to the probability of finding a given energy distribution, $\{w_j\}$. In equilibrium we expect to find the system in a state of maximum probability and so vary $\{w_j\}$ to find a maximum of W (consistent with the constraints of having N oscillators and P energy elements). The expression for a maximal W, W_{max}, then leads to an expression for entropy, $S = k \ln W_{\text{max}}$ from which we can use the thermodynamic relation $\partial S/\partial E = 1/T$ to obtain the Planck distribution.

This is the modern derivation. However, according to Kuhn, Planck was proceeding very differently in 1900–1. Kuhn's account of Planck's crucial papers is as follows.

In his first derivation, presented in the 14 December 1900 lecture, Planck began by considering oscillators of different frequencies. At each frequency, an energy E_v is distributed over N_v oscillators; Planck further assumes the energy at each frequency must be distributed as P_v elements of energy ε_v. This step alone does not imply quantisation for it simply allows one to count distinct ways of distributing energy. However, Planck further assumes that $\varepsilon_v = hv$ (this step will be discussed in detail in a moment). Thus at a particular frequency v, there are

$$R_v = (N_v + P_v - 1)!/((N_v - 1)!\, P_v!)\qquad(8)$$

ways of distributing the P_v energy units over the N_v oscillators.

The total number of ways to arrange a given total energy $E_{\text{total}} = \Sigma_v E_v$ is therefore the product of terms each of the form $(N_v + P_v - 1)!/(N_v - 1)!\, P_v!$, one for each frequency. Planck then maximises this product (acutally its logarithm) by varying the share of the total energy that oscillators at each frequency receive. This gives him the equilibrium energy distribution over oscillators at various frequencies. From this maximum value of the product of R_v's, $\Pi_v R_v$,

Planck defines an entropy, $S_v = k \ln (\Pi_v R_v)_{\max}$. Then he can invoke the thermodynamic relation $\partial S/\partial E = 1/T$ to get the distribution law. These last steps involved some lengthy calculations and Planck did not carry them through in the lecture, promising to present soon a simplified derivation which avoided the need to maximise.

By the time Planck published a derivation (Planck [1901a, b]) in the *Annalen* paper of 1901 (received three weeks after the December 1900 lecture), he had a new, shorter derivation. Kuhn's main point about this paper is that in several crucial respects it remains a continuation of the earlier lecture, a fulfilment of his promise to avoid the maximisation procedure, and only makes sense when seen in that light. Kuhn asserts Planck is still considering oscillators of several frequencies, although this is not clear in Planck's paper. As in the December lecture of 1900, Planck considers two arrangements of the energy elements to be the same if they correspond to the same distribution of total energy over the different oscillator frequencies. Planck's innovation in the [1901a, b] *Annalen* papers was to *assume* equilibrium between the oscillators at various frequencies. It was therefore no longer necessary to maximise the product of the R_v's, since equilibrium already guarantees that no energy will be exchanged between oscillators at different frequencies in such a way as to change the distribution of E_v's. Planck therefore can treat the entropy of a single frequency of oscillators. Since Planck's *Annalen* papers do not explicitly state his general problem these papers are often seen as foreshadowing the modern treatment involving oscillators of *one* frequency in equilibrium with the radiation field.[2]

In equilibrium Planck could then provide an expression for the entropy S_v associated with oscillators at a given frequency v: k times the logarithm of the number of distinct arrangements of energy over the N_v oscillators. Thus,

$$S_v = k \ln W \text{ (or as Planck writes } k \ln R)$$

$$= k \ln \{(N_v + P_v - 1)!/(N_v - 1)! \, P_v!\}$$

$$\sim k\{(N_v + P_v) \ln (N_v + P_v) - N_v \ln N_v - P_v \ln P_v\}$$

$$S_v = kN_v \left\{ \left(1 + \frac{E_v}{\varepsilon_v} \right) \ln \left(1 + \frac{E_v}{\varepsilon_v} \right) - \frac{E_v}{\varepsilon_v} \, nl \, \frac{E_v}{\varepsilon_v} \right\}. \tag{9}$$

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[1] The calculation can be done using lagrange multipliers and does lead to the Planck distribution as Kuhn shows on pp. 106–7.

[2] Kuhn's account conflicts with Klein's. Klein notes (Klein [1962], p. 473) that Plank's $W = $ (equivalently $R=$) $(P+N-1)!/((N-1)! \, P!)$ is not the same as the $W_{\max}$ which appears in the modern derivation of the Planck derivation or the Boltzmann gas case. (The modern derivation of the Planck law involves the use of oscillators of a single frequency of oscillators and *is* analogous to the Boltzmann case. In this derivation of the Planck law recall that $W_{\max} = N!/(w_0! \ldots w_p!)$ where the set $\{w_j\}$ gives the distribution of the number of oscillators with j energy elements that yields a maximum W.)

Klein points out that Planck's W is equal to the sum: $\Sigma N!/(w_0! \ldots w_p!)$, where the sum is over *all* values of $w_0 \ldots w_p$ satisfying the constraints on the number of oscillators and total energy. Klein then adds: 'It is natural though rather pointless, to ask why Planck deviated from Boltzmann at this particular stage.' According to Klein, Planck's only reason to write such an expression for W is that from it he could get the Planck distribution. According to Kuhn, Planck 'deviated' from Boltzmann because Planck had a completely different problem from Boltzmann's, one that is consistent with his work of three weeks earlier involving oscillators of many frequencies.

Applying the displacement law in the form $S = \phi(E_v/v)$, gives immediately the *result* $\varepsilon_v = hv$. Substituting hv for ε_v in equation (9) then yields S_v from which the Planck law can be calculated using $\partial S/\partial E = 1/T$.

This analysis of Kuhn's can be added to the earlier account of Klein; however, elsewhere there is real disagreement. According to Klein and others, *Planck had been forced to introduce the quantum* by fixing the size of ε. The reason Planck was able to accept this result stems, according to Klein, from Planck's unfamiliarity with the basic tenets of statistical mechanics. More specifically, Klein argues, if Planck had known about the equipartition theorem he would have realized he should be able to take the limit $\varepsilon \to o$ to remain consistent with classical physics. But as Rayleigh had shown in a paper in 1900, this led to a distribution law different from Planck's, and therefore was incorrect. Klein concludes,

> It is obviously of the very essence of Planck's work that ε could not be allowed to vanish, if the proper distribution law were to be reached. Planck apparently did not even consider the possibility of taking this limit. This is undoubtedly related to Planck's apparent unawareness of the equipartition theorem and all it implied . . . (Klein [1962], p. 474).

Kuhn also must account for Planck's not taking the limit $\varepsilon \to o$ as Planck would have to if he continued to follow Boltzmann. However, Kuhn's explanation is quite different. Where Klein cites Planck's lack of respect for equipartition, Kuhn suggests that Planck may have misunderstood the relevant passage of Boltzmann. The passage in question is in chapter two of the 1877 paper and deals with two ways of counting possible states. Kuhn writes (p. 128):

> In the first case, the energy of individual molecules was restricted to values $o, \varepsilon, 2\varepsilon, 3\varepsilon, \ldots$; in the second, molecules were described as lying in the range o to ε, ε to 2ε, 2ε to 3ε, and so on. Both cases led to the same combinatorial expression and, for large N and P, to the same distribution law. The two appear to be interchangeable, and Planck clearly thought that they were. As a result, 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.

Thus both Klein and Kuhn cite Planck's unfamiliarity with combinatorial reasoning to account for his not having taken the limit $\varepsilon \to o$. However, the conclusions they draw from this are quite different: for Klein, Planck's misunderstanding *allows* him to go forward with quantisation without realizing the full import of what he had done. For Kuhn, Planck's misunderstanding allows him to proceed 'fully classically' without ever meaning to have quantised resonator energy. As we shall see, it is the existence of two such distinct interpretations that makes the Planck episode interesting from a philosophical point of view.

The further evidence Kuhn presents is indirect. Kuhn asserts that Planck's arguments in his 1906 *Lectures On Heat Radiation* are simply developed in a different order than in the *Annalen* papers. Almost all the same ideas Planck had presented earlier are still there: charged linear oscillators in equilibrium with the radiation field, a combinatorial derivation of the entropy just like that of the

Annalen, an electromagnetic *H*-theorem, and the use of 'natural radiation' to restrict initial conditions. On the basis of this continuity with Planck's earlier work, Kuhn concludes that Planck's fundamental position did not change between 1901 and 1906. Then, by showing that in the *Lectures* of 1906 Planck definitely did *not* intend to quantise the energy levels of the oscillators, Kuhn concludes that he had no such intention in 1901.

Kuhn bolsters his assertion that in 1906 Planck did not intend to quantize the oscillator energy by several passages in the *Lectures*. For instance, Kuhn cites Planck's remark in the *Lectures* about 'the number of resonators with energy of a given magnitude (better: which lie within a given "energy region")' (p. 129). Or again at another point in the *Lectures*, Planck refers to 'the probability that the energy of a resonator lies *between* the values U and U+ΔU' (p. 129). Thus, if we accept Kuhn's premise that the 1900–1 work is continuous with the 1906 work, his case is very strong. Klein, however, challenges the premise. In a recent review in *Isis*, Klein acknowledges the fact that the *Lectures* reflect an effort on Planck's part to deny quantisation, but Klein maintains this is a retreat from Planck's earlier position as 'Planck's attempt to get around the discreteness that others were taking more seriously than he had intended' (Klein [1979], p. 432).

But quite independently of Planck's programme in 1906, classical physics was being challenged in a thoroughgoing way from another quarter. Before Planck had finished his *Lectures*, Einstein had published his paper on light quanta. At first, as Einstein later reported, he thought his new light quantum hypothesis contradicted Planck's work of 1901. Within the year, though, he realised the close connection between his own work and the Planck distribution and in 1906 published the now standard derivation of the energy of a gas of atomic oscillators, each with an energy proportional to their frequency of oscillation. His result was the same as Planck's up to the phase-space factor. By so doing, Einstein had certainly established the heuristic value of the light quantum hypothesis—what remained to be shown was the necessity of such an assumption.

Kuhn's account of this episode—the work of Einstein, Rayleigh, Jeans and Ehrenfest and their efforts to show the necessity of the quantum hypothesis—constitutes a good part of chapters 6, 7 and 8. However, they do not add much to what is already known through the work of Klein (especially [1963] and [1970]) and Miller [1976]. Kuhn does, however, present several previously undiscussed sections of Ehrenfest's notebooks (though in general Ehrenfest's papers are treated in more detail in Klein's biography). By contrast, one real contribution of this section of Kuhn's book is his discussion of the relation of Planck's quantum of action to the beginnings of the 'Old Quantum Theory'. Here in his treatment of Stark's often wild speculations, A. Sommerfeld's criticisms, and A. E. Haas's somewhat tentative first steps towards atomic structure, Kuhn has begun to establish the connections between the black-body problem and the early history of the Bohr atom. But, as Kuhn himself notes, it was really Planck's second quantum theory, his work after 1910, which exerted an influence on the history of the Bohr atom.

The Bohr atom was not, however, what Planck was concerned with when he set out to revise his earlier theory. Of his radical revision there is no doubt; the question of which way he was revising again divides Kuhn and Klein. Following Klein's interpretation of the 1900–1 papers, Planck after 1910 is retreating:

> ... Planck was fully committed to the quantum, but not necessarily to the quantum theory in Einstein's sense. Planck's work in the years after 1910, when he resumed publication in this field shows him holding fast to the quantum of action but retreating steadily from his earlier strict quantisation of the oscillator (Klein [1966], p. 28).

It is no surprise that Kuhn does not agree. Since Kuhn takes Planck to be doing strictly continuum physics, at least until 1908, *any* discussion of discontinuity represents a move towards the quantum as we now understand it. Thus, 'Planck himself did not publicly acknowledge the need for discontinuity until 1909 and there is no reason to suspect that he had recognised it until a year before' (p. 140). For Kuhn the first evidence of this acceptance of discontinuity comes in correspondence with Lorentz in 1908 when Planck tries to argue that the excitation of the oscillators occurs only after a certain 'threshold' but that emission takes place purely classically. Later in 1911, Planck reversed this and called for quantum emission and continuous absorption. Little by little, Planck, according to Kuhn, came to accept the discontinuity as an inevitable part of the new physics.

4 SUMMARY AND CONCLUSIONS

From the historical work now available to us, much of the story regarding the introduction of the quantum is clear. The role of the experimentalists Pringsheim, Rubens, Langley and Paschen in pushing the theorists towards a correct form of $\rho(\nu, T)$ has been established, as has the role of the early theorists, Kirchhoff, Wien, Stefan, and Boltzmann in placing theoretical constraints on the form of the law.

We also have more or less a consensus on Planck's early concern with the 'absolute' interpretation of the second law, and his corresponding antipathy towards the probabilistic interpretation; with all this we are in a better position to understand Planck's various attempts to circumvent the probabilistic arguments by his extended efforts to establish an 'electromagnetic *H*-theorem'. Up to 1900 and including Planck's fortuitous guess at the correct radiation formula, Kangro, Klein, Rosenfeld and now Kuhn provide a fairly detailed and harmonious account of the black-body problem.

With the publication of the *Lectures* in 1906, we once again have agreement. That is, even Klein in his *Isis* review seems implicitly to acknowledge the strength of Kuhn's argument that Planck was not speaking of quanta in the *Lectures*, as Klein only challenges the premise that the *Lectures* are continuous with the 1900-1 work. Thus there seems to be no real dispute about Planck's lack of commitment to quantisation in 1906. Nor is there any about his ambivalent position in the years following 1908 or 1909 up to his full acceptance of the quantum in 1912. Nor, for that matter is there a great difference between Kuhn's and Klein's descriptions of the papers of the time by Einstein, Ehrenfest, Lorentz, Rayleigh and Jeans in the years between 1905 and 1910. (However, if Kuhn is correct, then it was Einstein and not Planck who deserves credit for the introduction of the quantum discontinuity.) The only issue about this period is whether Planck's less than full commitment to the quantum between 1906 and 1912 or so should be seen as a retreat or as an advance from his work of 1900-1.

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However, Kuhn and Klein interpret that work of 1900–1 in what seem to be two radically differing ways: Klein says Planck was forced to introduce the quantum; Kuhn says Planck was continuing with a programme of classical continuum physics. But even here Klein and Kuhn *agree* that one way or another Planck was not aware of the full consequences of Boltzmann's work. Kuhn points to Planck's treatment of Boltzmann's continuous and discrete divisions of phase space as yielding the same results; Klein points to the fact that Planck never in the crucial period 1900–1 shows a commitment to the equipartition law and its 'catastrophic' consequences. In short, the dispute seems to me to be much more localised than it seems at first, and is over the issue of how Planck misunderstands statistical mechanics and thereby does not take limit $\varepsilon \to 0$. The most important aspect of this 'localized' disagreement is that for Klein the quantum was introduced by Planck on 14 December 1900; for Kuhn, not until 1909.

I would like to suggest a third interpretation, drawing on both Kuhn and Klein's work. In 1900–1, the question of the continuum *vs.* discreteness as such, which for us is of such overwhelming interest, was entirely peripheral to Planck's other concerns.[1] Planck was interested in the electromagnetic *H*-theorem, in the use of combinatorics and statistics, and in initial condition restrictions to save his decade long programme of research. He was worried about the definition of oscillator entropy, about its uniqueness and justification, worried about giving a coherent derivation of his new law using the tools of Boltzmann with which he was not yet fully familiar.

In pursuing his goals, in 1900–1 Planck was drawn in a series of rapid steps to the techniques of Boltzmann that he had shunned for so long. Without ever having conducted a rigorous examination of the foundations of Boltzmann's work, he found the techniques aided him in passing from his own earlier research programme to a distribution law that was from the start overwhelmingly confirmed by experiment. As he had never really bothered with the physical interpretation of the foundations of statistical mechanics when he was debating Boltzmann, he was not inclined to begin such an examination at the very moment these techniques were crowning with success his ten year struggle with the blackbody problem. And it was just such a critical examination which would have been necessary for Planck to arrive at a correct view of the continuum problem. I suspect a lot of things were on Planck's mind about how physics would have to be revised, but that the question of the quantum discontinuity was not among them.

There is, in this involved and technical chapter in the history of modern physics, an importance which goes beyond setting the record straight on Planck. Above all, there is a historiographical problem: it is not always possible to impose a self-consistent, fully articulated set of beliefs on a scientist's view of his problem, especially at periods of great upheaval. That is, I am not sure it is meaningful to say Planck was doing *either* 'classical' *or* 'quantum' physics. The details of the oscillatory model were certainly beyond Planck's programme; he could not even say for certain what the oscillators were or why they were oscillating, let alone whether such processes were *classical*.

Both Kuhn and Klein seem to want to construe the Planck of 1900 as having

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[1] It might be noted that this was not true for everyone; Ernst Mach, for instance, was interested in this problem. See, for instance, Mach [1960], pp. 588 ff.

a position that is both coherent and fully articulated even with respect to concerns like the continuum which were not a central part of his research. And in Kuhn's work, at least, this kind of effort to expose the coherence of past authors' work goes back to his earliest work in the history of science. In his recollections at the beginning of *The Essential Tension*, Kuhn writes,

> I offer [students] a maxim: When reading the works of an important thinker, look first for the apparent absurdities in the text and ask yourself how a sensible person could have written them. When you find an answer, I continue, when those passages make sense, then you may find that more central passages, ones you previously thought you understood, have changed their meaning (Kuhn [1977], p. *xii*).

It is this quest for coherence in the history of science, for an implicit world view, that I suspect links together much of Kuhn's work—both philosophical and historical. For by searching for consistent world views, Kuhn was bound to see changes of science as shifts in world view: sudden and incommensurable Gestalt-like changes.

> What my reading of Aristotle seemed therefore to disclose was a global sort of change in the way man viewed nature and applied language to it, one that could not properly be described as constituted by additions to knowledge or by the mere piecemeal correction of mistakes. That sort of change was shortly to be described by Herbert Butterfield as 'putting on a different kind of thinking cap', and puzzlement about it quickly led me to books on Gestalt psychology and related fields (Kuhn [1977], p. *xiii*).

From Gestalt psychology it was a short step to Kuhn's early concept of the paradigm in its most central employment: the use of an exemplary problem solution as a guide to methodology and ontology in the solution of other problems.

If there was ever an example of such a use of a paradigm it would seem to be Kuhn's account of Planck's use of Boltzmann's 1877 paper. And I think that the problem of the search for coherence in Planck's thought is intimately tied to the view of scientific change portrayed in Kuhn's philosophical work. The difficulties that are attached to such a view seem to be shared by both the historical and the philosophical Kuhn.

Kuhn's analysis of Planck's reluctant introduction *malgré lui* of the quantum is a remarkable parallel to his much earlier (1957) analysis of the Copernican revolution. In a language that will seem strikingly familiar to the reader of *The Quantum Discontinuity*, Kuhn wrote there, twenty years earlier:

> The *De Revolutionibus* was written to solve the problem of the planets, which, Copernicus felt, Ptolemy and his successors had left unsolved. In Copernicus' work the revolutionary conception of the earth's motion is initially an anomalous by-product of a proficient and devoted astronomer's attempt to reform the techniques employed in computing planetary position (Kuhn [1971], pp. 135–6).

In the case both of Planck and Copernicus, the conservative and technical problem-solver is forced against his own intentions to introduce a revolutionary concept merely as a 'by-product' of other concerns. Such an analysis gives a great coherence both to Planck's and to Copernicus's work; in both cases we

can see their accomplishments as the working out of their respective problems by the adaptation of earlier techniques. In one case this paradigmatic solution (by which I mean the work that served as an exemplar) is Boltzmann's 1877 treatise; in the other it is Ptolemy's *Almagest*.

There is in all of this what I take to be a unifying theme in Kuhn's writings; I cannot therefore accept his repeated protests that his history and philosophy are the products of completely separate concerns. This theme is that scientific change occurs primarily through the technical applications of paradigmatic solutions to technical problems. Revolutionary shifts in world view come, reluctantly and against all expectation, out of just such 'normal science'. Thus, though at the beginning of this review I said Kuhn's philosophy is never explicitly presented, it is deeply embedded implicitly in his view of the history of science. Moreover, Kuhn's work is perhaps best seen as just this sort of continuous interaction between the history and philosophy of science.

From the hot summer day in 1947 when, according to his account, Kuhn first read Aristotle as a solver of the outstanding problems of his day, through *The Structure of Scientific Revolutions* in 1962, Kuhn was articulating this view of scientific change. We see it again in 1957 with the first publication of *The Copernican Revolution*, and once more in 1978 with *The Quantum Discontinuity*. Throughout all these works, Kuhn is searching for an account of scientific change by exhibiting a coherence in the work of pivotal scientists, a coherence within their works and a coherence with past exemplary problem solutions.

The advantage of this approach is that a sympathetic reading of an author often brings us closer to the author's original meaning. However, a sympathetic reading should not blind us to the fact that, especially in periods of rapid scientific change, the ideas of an individual also change and are often not fully developed; not all consequences are drawn, and not all assumptions articulated. It is, therefore, often true as the example of Planck has shown, that self-contradictory or maddeningly incomplete thoughts are characteristic of a new work in physics. The problem of how to characterise such innovative but not quite ordered thought is one which faces historians and philosophers alike.

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REFERENCES

GARBER, E. [1976]: 'Some Reactions to Planck's Law, 1900–1914', *Studies in History and Philosophy of Science*, **7**, pp. 89–126.

GOLDBERG, S. [1976]: 'Max Planck's Philosophy of Nature and His Elaboration of the Special Theory of Relativity', *Historical Studies in the Physical Sciences*, **7**, pp. 125–60.

HERMANN, A. [1971]: *The Genesis of the Quantum Theory*. Translated by C. W. Nash. Cambridge, Massachusetts: M.I.T. Press.

HIEBERT, E. N. [1968]: *The Conception of Thermodynamics in the Scientific Thought of Mach and Planck*. Wissenschaftlicher Bericht Nr. 5/68, Ernst Mach Institute (Freiburg i. Br., 1968).

JAMMER, M. [1966]: *The Conceptual Development of Quantum Mechanics*. New York: McGraw-Hill.

KANGRO, H. [1976]: *History of Planck's Radiation Law*. London: Taylor and Francis.

KLEIN, M. J. [1962]: 'Max Planck and the Beginnings of Quantum Theory', *Archive for History of Exact Sciences*, **1**, pp. 459–79.
KLEIN, M. J. [1963]: 'Planck, Entropy, and Quanta, 1901–1906', *The Natural Philosopher*, **1**, pp. 83–108.
KLEIN, M. J. [1966]: 'Thermodynamics and Quanta in Planck's Work', *Physics Today*, **19**, no. 11, pp. 23–32.
KLEIN, M. J. [1967]: 'Thermodynamics in Einstein's Thought', *Science*, **157**, pp. 509–16.
KLEIN, M. J. [1970]: *Paul Ehrenfest: Volume I: The Making of a Theoretical Physicist*. Amsterdam and London: North-Holland; New York: Elsevier.
KLEIN, M. J. [1979]: 'Essay Review' of *The Quantum Discontinuity*, *Isis*, **70**, pp. 430–4.
KUHN, T. S. [1971]: *The Copernican Revolution*. Cambridge, Massachusetts: Harvard University Press.
KUHN, T. S. [1973]: *The Structure of Scientific Revolutions*. International Encyclopedia of Unified Science **2**, Vol. 2. Chicago: The University of Chicago Press.
KUHN, T. S. [1977]: *The Essential Tension*. Chicago: The University of Chicago Press.
MACH, Ernst [1960]: *The Science of Mechanics*. La Salle: Open Court.
MILLER, A. I. [1976]: 'On Einstein, Light Quanta, Radiation and Relativity in 1905', *American Journal of Physics*, **44**, pp. 912–23.
PLANCK, M. [1900*a*]: 'Entropie und Temperatur strahlender Wärme', *Ann. d. Phys.*, **1**, pp. 719–37. Received 22 March 1900.
Planck, M. [1900*b*]: 'Über eine Verbesserung der Wien'schen Spektralgleichungen', *Verh. d. D. Phys. Ges.*, **2**, pp. 202–4. Presented 19 October 1900.
PLANCK, M. [1900*c*]: 'Zur Theorie des Gesetzes der Energieverteilung im Normalspectrum', *Verh. d. D. Phys. Ges.*, **2**, pp. 237–45. Presented 14 December 1900.
PLANCK, M. [1901*a*]: 'Über das Gesetz der Energieverteilung im Normalspectrum', *Ann. d. Phys.*, **4**, pp. 553–63. Received 7 January 1901.
PLANCK, M. [1901*b*]: 'Über die Elementarquanta der Materie und der Electrizität', *Ann. d. Phys.*, **4**, pp. 564–6. Received 9 January 1901.
PLANCK, M. [1906]: *Vorlesungen uber die Theorie der Wärmestrahlung*. Leipzig: Barth. Referred to in the preceding as *Lectures*.
ROSENFELD, L. [1936]: 'La Premiere phase de l'évolution de la théorie des quanta', *Osiris*, **2**, pp. 149–96.

A CAUTIOUS OVERVIEW OF BEHAVIOUR THERAPY*

A reviewer who imitated the carefully hedged style of *Behavior Therapy: Scientific, Philosophical, and Moral Foundations* could comment as follows: In general, it is probably true that most clinical psychologists who call themselves *behaviour therapists*, or who are sympathetic to many or at least to a significant number of the cluster of techniques professionally recognised as characteristic of behaviour therapy, will want to read this book, either in its entirety or in part. Since I do not intend to sustain this mandarin prose throughout my review, however, let me stipulate immediately that Edward Erwin's consistent posture is one of precarious balance, presenting arguments on both sides of nearly every issue he examines. Moreover, he frequently defines terms in an idiosyncratic way, and they shift in meaning at different stages of his argument. Consequently his protean theses tend to slither out of reach at just the point where the reader thinks he has grasped them. Erwin's bibliography covers eighteen pages of references; his method is to analyse and summarise their findings on five major