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Physics in Perspective

Planck, the Quantum, and the Historians

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In late 1900, the German theoretical physicist Max Planck derived an expression for the spectrum of black-body radiation. That derivation was the first step in the introduction of quantum concepts into physics. But how did Planck think about his result in the early years of the twentieth century? Did he assume that his derivation was consistent with the continuous energies inherent in Maxwellian electrodynamics and Newtonian mechanics? Or did he see the beginnings, however tentative and uncertain, of the quantum revolution to come? Historians of physics have debated this question for over twenty years. In this article, I review that debate and, at the same time, present Planck's achievement in its historical context.

Key words: Max Planck; Ludwig Boltzmann; Martin J. Klein; Thomas S. Kuhn; Olivier Darrigol; Allan A. Needell; quantum; black-body; resonator; entropy; second law of thermodynamics.

Introduction

Hot objects radiate heat and light, and the character of that heat and light changes with temperature. As an iron bar is heated, for example, it first glows dimly with a reddish light, and as its temperature increases, it glows more and more brightly, with light that becomes first yellowish, and then bluish-white. Such thermal radiation was studied widely throughout the second half of the nineteenth century from both theoretical and experimental perspectives. From mid-century on, scientists such as John Tyndall, Gustav Kirchhoff, Balfour Stewart, and Josef Stefan made significant contributions. On the basis of very general thermodynamic arguments, Ludwig Boltzmann derived Stefan's T^4 law, which states that any object emits thermal radiation (power per unit area) proportional to the fourth power of its absolute temperature T.[1] By the end of the century, physicists knew that thermal radiation was electromagnetic in character and had a good understanding of its spectrum – how the intensity of thermal radiation (variously called black radiation and black-body radiation) depended on wavelength or frequency.[2] And theorists, among them the German physicist Max Planck (figure 1), worked to develop mathematical relationships that could describe these experimental results. In late 1900 and early 1901, Planck published a series of papers that not

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only gave a successful mathematical description of the experimental black-body data, but in the process took the first steps on the path to twentieth-century quantum physics.

What did Planck do in 1900? Conventional wisdom holds that he quantized harmonic oscillators (or resonators, to use his term) in equilibrium with electromagnetic radiation in a cavity, at some fixed temperature. But physicists interested in the history of their discipline have learned to beware of such conventional wisdom, with good reason.[3] If we turn to the historians for guidance, however, we find little consensus. To be sure, most would agree that the unqualified statement that ''Planck quantized the oscillators'' can be misleading; on any interpretation, Planck's understanding of this phrase would have been quite different from our own. Nevertheless, many historians, following the lead of Martin J. Klein, do assert

Fig. 1. Max Planck (1858–1947), as he appeared around 1900. Credit: American Institute of Physics, Emilio Segrè Visual Archives, W. F. Meggers Collection.

that, however tentatively and uncertainly, Planck – with his finite "energy elements" – did introduce something very new into physics in 1900, and almost certainly knew that he had done so.[4]

This situation changed in 1978 with the publication of Thomas S. Kuhn's *Black-Body Theory and the Quantum Discontinuity*.[5] Kuhn gave a highly detailed account of Planck's work and the initial reaction to it. But in the process, he argued that Planck could not possibly have intended such a far-reaching step. In Kuhn's words:

> 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 . ... [6]

In Kuhn's scenario, the prospect of discontinuous energies did not appear until 1905 and 1906, in the work of the young and little-known physicists Paul Ehrenfest and Albert Einstein. And even then, according to Kuhn, Planck did not take the idea seriously until 1908.[7] Another historian of physics, Olivier Darrigol, has reached similar conclusions, though in part for different reasons, on the basis of his own detailed analysis of Planck's work.[8] Others have maintained the older view. And from the standpoint of Allan Needell, who also has written extensively on Planck, even to put the issue in these terms is to ask the wrong question: Our goal should be to understand Planck on his own terms, rather than focus too exclusively on a question that would have had little meaning in 1900.[9]

All of these works have led to a much more detailed understanding of Planck, and even to a considerable measure of agreement. Nevertheless, on the central question – how did Planck think about his derivation in 1900? – no consensus has emerged.[10] Moreover, this uncertainty is even spreading to physics textbooks, at least among texts whose authors are interested in the history of the subject.[11] Although several lines of argument have figured in this controversy, one in particular stands out. To derive his new radiation law late in 1900, Planck distributed finite, discrete "energy elements," as he called them, among a large number of resonators in equilibrium with electromagnetic radiation – a scheme that to a modern reader sounds very much like discontinuous, quantized energy levels. In doing so, Planck employed a technique developed by Boltzmann in 1877. But Boltzmann had applied that technique to a gas only as a simplified and unphysical illustration, one that he followed immediately with a more realistic calculation that assumed continuous molecular energies. Kuhn and Darrigol both argue that Planck, without saying so, really had this second approach in mind, and so was likewise assuming continuous resonator energies when he applied Boltzmann's theory to his own problem.

In this article, I will review these conflicting arguments, and at the same time present Planck's achievement in context, in the light of recent historical scholarship. I find it difficult to argue with Needell's point of view, cited above; but I will also maintain that the specific arguments that Planck could not possibly have been thinking in terms of discontinuous energies in these early years are hard to defend. Presented in historical context, Planck's thinking is considerably more complex and ambiguous – and in my own view, considerably more interesting.

If we are to understand Planck's achievement and the ensuing controversy among historians, we must first set the scene: What led Planck to write his famous papers of December 1900 and January 1901, and what did he do in those papers? In the following sections, I will first summarize the development of Planck's work in thermodynamics and thermal radiation through early 1900, in which Planck deployed his "absolute" version of the second law of thermodynamics to investigate the nature of black-body radiation, hoping that each would shed light on the other. That research program seemed at first to succeed; but in the fall of 1900, it foundered on the rocks of improved experimental results. At that point, Planck turned to Boltzmann; and so in the next section, I will outline Boltzmann's 1877 theory that relates entropy and probability – a theory that played a central role in Planck's derivation of his new radiation law late in 1900.

I will then review Planck's derivation, where we will see how he introduced his energy elements, and what he said about them. After a brief detour into the law of energy equipartition, I will present several excerpts from Planck's correspondence in 1905 and thereafter, in which he refers to his new energy elements. At that point, we can finally inquire into what is at issue in this dispute among the historians, and see what we can conclude about how Planck thought about his "energy elements" in the early years of the twentieth century. As we develop these ideas, some recourse to the mathematics of Planck's and Boltzmann's theories will be unavoidable. I hope that readers who are not mathematically inclined will nevertheless find it possible to "read through" the equations, and still gain an intuitive understanding of what Planck did and why historians of physics have found it so difficult to agree on its significance.

Planck, the Second Law, and Black-body Radiation

Throughout his career, Max Planck's research centered on the second law of thermodynamics. His Ph.D. thesis of 1879, and virtually all of his research through the mid-1890s, emphasized such topics as the distinction between reversible and irreversible processes, the importance of the entropy principle, and the advantages of thermodynamic potentials over Carnot cycles. He applied these ideas to the emerging discipline of physical chemistry, and indeed, roughly one-third of his early papers (through the mid-1890s) were published in the *Zeitschrift für physikalische Chemie*, a journal founded by J. H. van't Hoff and Wilhelm Ostwald in 1887.[12] And he devoted his second book, published in 1893, entirely to physical chemistry.[13]

Until about 1914, Planck took the second law to have absolute validity. In sharp contrast to figures like James Clerk Maxwell, Josiah Willard Gibbs, and Ludwig Boltzmann, Planck thought it entirely impossible that there could be any exceptions,* however unlikely, to the law of entropy increase.[14] Perhaps for this reason, and perhaps also because of the immense fruitfulness he found in thermodynamics, Planck was at best indifferent to and at times skeptical of atomic hypotheses, at least in the years before 1900.[15] He nevertheless had a solid grounding in kinetic theory, initially through his editing of Gustav Kirchhoff's thermodynamics text, which contains a concluding section on that topic.[16] The publication of this text in 1894 led to an exchange with Boltzmann on the validity of one of Kirchhoff's derivations, and surely led Planck deeper into the subject.[17] Before 1900, Planck had made little use of kinetic theory in his own research, and it is not clear how deeply he had thought about it, or how much he had read. But certainly, by 1900, Planck was not unacquainted with kinetic theory.

In 1895, in a shift in emphasis that was more apparent than real, Planck turned his attention to black-body radiation. During the 1890s, this topic had continued to arouse widespread interest among German physicists – including, as we shall see, Planck's friend and colleague Wilhelm Wien. Experimental measurements of the spectral distribution improved rapidly during this decade, stimulated in part by the need for practically useful improvements in temperature measurement and the absolute temperature scale. Much of this activity took place at the Physikalisch-Technische Reichsanstalt (PTR)** in Charlottenburg, close by Planck in Berlin. Wien, whose theoretical contributions we will encounter below, also had a hand in the experiments. A great many others did as well. I will mention only the work of Otto Lummer and Ernst Pringsheim, and of Heinrich Rubens and Ferdinand Kurlbaum, in 1899–1901.[18] All four were at the PTR, and thus Planck was able to follow their experiments at close hand. In his *Scientific Autobiography*, he even recounted an occasion in October 1900 when Rubens visited him one morning to report on the latest developments.[19]

Planck's first foray into this arena in 1895 involved the analysis of a harmonic oscillator, or "resonator" (a linear oscillating electric dipole) in an electromagnetic field, taking as his model a calculation Heinrich Hertz published in 1889.[20] This

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* Physicists and historians alike often assume that Planck's attitude towards the second law changed around 1900, when he adopted Boltzmann's probabilistic mathematics and, it is usually said, Boltzmann's probabilistic viewpoint as well. But recent work by Needell, and independently a few years later by Darrigol, has shown conclusively, and surprisingly, that Planck maintained his absolute view for many more years. Thus for Boltzmann (and also for Maxwell and Gibbs), spontaneous decreases in entropy – for example, all of the air in a room spontaneously collecting in one corner – are no more than highly improbable. Planck by contrast regarded such processes as entirely forbidden by the second law and, for example, criticized Gibbs for suggesting otherwise (ref. 72). It is beyond the scope of this article to present the evidence for Planck's viewpoint – see refs. 14 and 24 for detailed treatments. In addition, readers will find a nice "snapshot" of Planck's views, showing at firsthand this aspect of his thinking, in the first three of a series of eight lectures that Planck delivered at Columbia University in New York in the spring of 1909, recently reissued by Dover Press (ref. 49). Planck's continuing interest in physical chemistry is also evident there.

** The PTR, founded in 1887, might best be described as an early example of a national laboratory concerned with integrating pure science with the technological needs of German industry (ref. 18). As such, it served as a model for the National Physical Laboratory in England and the National Bureau of Standards (now the National Institute of Standards and Technology) in the United States, both founded a little over a decade later.

work, presented to the Prussian Academy of Sciences in March 1895 and February 1896, was followed by a five-part series of papers extending from 1897 to 1899 and titled "On Irreversible Radiation Processes."[21] In the course of this work, Planck continued to develop his idea of a linear resonator that is small with respect to the wavelength of electromagnetic radiation in the vicinity of its resonant frequency (and thus is effectively a point source). An oscillating current, of course, emits electromagnetic radiation, and Planck used this notion to introduce the idea of "radiation damping" or "conservative damping," in which a resonator loses energy not to dissipative processes, but to the electromagnetic field with which it is in equilibrium. Moreover, both Kirchhoff and Stewart had shown that the nature of thermal radiation is independent of the material with which it is in equilibrium. Planck's idealized resonator was thus a particularly convenient choice.

Planck had originally hoped to show that the interaction of such a resonator as it came into equilibrium with an electromagnetic field was irreversible, so that using only Maxwell's laws, one would be led to an increase in a suitably defined electromagnetic entropy. This hope was dashed in another exchange with Boltzmann, who argued that Maxwell's laws were (as we would say today) invariant under time reversal, and consequently, could not possibly lead to irreversible processes, any more than could Newton's laws. Forced to agree, Planck introduced an auxiliary hypothesis of "natural radiation"* as an electromagnetic analogy to "molecular disorder," a concept that Boltzmann had introduced in the 1890s to clarify the implications of his H-theorem.[22]

Planck summarized this work in a long paper, likewise titled "On Irreversible Radiation Processes," that was submitted to the *Annalen der Physik* in November 1899, and published early in 1900.[23] In the introduction to this paper, he emphasized that his hypothesis of natural radiation is independent of and in addition to Maxwell's laws, and he made an explicit comparison to Boltzmann's concept of "molecular disorder." But in contrast to Boltzmann, Planck took the second law as absolute, and, as both Allan Needell and Olivier Darrigol have emphasized, introduced "natural radiation" to ensure that for any process whatever, violations of the second law of thermodynamics were not merely improbable but forbidden. Both argue that Planck increasingly found in his resonators the key to the disorder that lay behind natural radiation.[24] Needell, in particular, has called attention to an address Planck delivered in 1899, in which he observed that in considering thermal radiation, one could divide space into "microscopic" and "macroscopic" regions. Regarding the former, he suggested that "thermal radiation most probably arises from certain oscillations that take place within molecules or ions" – by implication,

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* Planck's concept of natural radiation is similar to the notion of incoherent light. Since his resonators are damped, they respond not to a single frequency but to a narrow range of frequencies, which he described in terms of Fourier components. The assumption of natural radiation implies that these Fourier components, or "partial waves" as he often called them, are independent and vary at random; this condition in turn places constraints on how one calculates the average resonator energy U. For Planck's own treatment, see for example his "Strahlungsvorgänge" (ref. 23); the first section contains a good qualitative description, and the ensuing sections develop the detailed theory. For descriptions, see especially Sommerfeld, *Statistical Mechanics* (ref. 22), and also Longair, *Concepts* (ref. 10), Kuhn, *Quantum Discontinuity* (ref. 5), and Darrigol, *c-Numbers* (ref. 8).

not yet understood. He expressed the hope – never to be realized – that "further development of Maxwell's ideas" might lead to an electromagnetic theory of heat that dispenses with the need for "special new hypotheses" in explaining the relationship between these microscopic and macroscopic regions.[25] As early as 1899, Planck seems to have suspected that new and interesting physics was likely to result from a better understanding of his (microscopic) resonators and their relation to "natural radiation" – possibly one of the "special new hypotheses" to which he alluded.

By November 1899, when he submitted this paper to the *Annalen*, Planck had established the following results:

- Through an analysis of the interaction of his conservatively damped resonator with a radiation field, Planck showed that

  $$u_\nu = \frac{8\pi\nu^2}{c^3}\,U, \tag{1}$$

  where u_ν is the radiation density* in the frequency interval ν to $\nu + d\nu$, U is the time-averaged energy of a resonator of frequency ν, and c is the speed of light. Planck found it encouraging that this result did not depend on the damping constant or other detailed properties of the resonator.
- Planck *defined* the "electromagnetic entropy" S of a *single* resonator of frequency ν and average energy U as

  $$S = -\frac{U}{a\nu}\log\frac{U}{eb\nu}, \tag{2}$$

  where a and b are both "universal positive constants," and e is the base of natural logarithms. At this point, his overall strategy becomes clear. Using *both* Maxwell's laws *and* the hypothesis of natural radiation, he had derived Eq. (1). Then, using Eq. (2) and the thermodynamic definition of the absolute temperature T,

  $$\frac{1}{T} = \frac{dS}{dU}, \tag{3}$$

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* The radiation density (electromagnetic energy per unit volume in a narrow frequency range) is essentially the quantity plotted in figure 2, although it is given there in terms of wavelength instead of frequency. It thus is a measure of how the intensity of the radiation varies with frequency or wavelength. Planck was the first to write the energy density and related quantities in terms of the frequency ν instead of the wavelength λ, a convention that I follow for the most part. Both forms are still widely encountered both among historians and in physics texts. Note that the Stefan-Boltzmann law places a constraint on the radiation density: the integral $\int u_\nu\,d\nu$ over all possible frequencies must be proportional to T^4. A similar result must hold if the radiation density is expressed in terms of wavelength instead of frequency. More intuitively, the areas under the curves in figure 2 must likewise vary as T^4. A second constraint is given by Wien's displacement law, described below.

he readily found an expression for U, the average energy of a resonator. That result, combined with Eq. (1), led immediately to the radiation density of the electromagnetic field,

$$u_\nu = \frac{8\pi a}{c^3}\nu^3 e^{-b\nu/T}, \tag{4}$$

where a and b are the same "universal constants" introduced in Eq. (2).

In fact, this result had been proposed previously by Wilhelm Wien in 1896[26] – it is still referred to as Wien's law – and until late 1899 or early 1900, seemed to be in good agreement with experiment. (As Klein and others have argued, it seems likely that Planck in fact arrived at his expression for entropy, Eq. (2), by starting with Eq. (4) and reasoning in reverse. Planck himself seems to hint as much.[27]) Thus, Planck had *not* found an improved equation for the energy density u_ν; he had instead given a new and more fundamental derivation of Wien's law, based on the second law of thermodynamics.

Planck argued that the electromagnetic entropy could be identified with the thermodynamic entropy – the assumption of natural radiation leads to disorder, which in turn underlies irreversible processes. He went on to show that the total entropy (resonator and field combined) was a maximum for an equilibrium state. He finished by arguing strongly, though as it turned out incorrectly, that Wien's law and, by extension, his own expression for the entropy of a resonator were necessary consequences of the second law.

- Planck concluded by calculating numerical values for the "universal constants" a and b in Eqs. (2) and (4) – they are, of course, the precursors of the constants k and h (now known as Boltzmann's and Planck's constants) that he introduced at the end of 1900. He used these two constants, together with the speed of light and the gravitational constant, to propose a now-famous system of natural units for length, mass, time, and temperature. He set great store by these natural units, saying that they "necessarily retain their significance for all times and for all cultures, even alien and non-human ones. ... "

In the meantime, the experimentalists at the PTR continued to refine their own measurements. In consequence, the ink was barely dry on Planck's paper before word of possible deviations from Wien's law (figure 2) reached him, along with possible alternatives to Eq. (4). Planck's response was a second paper,[28] submitted to the *Annalen* in March 1900. Rather than simply defining the entropy S, he showed that it must satisfy the general condition, based on the second law of thermodynamics, that $d^2S/dU^2 = -f(U)$, where $f(U)$ is a positive function.

To find $f(U)$, Planck considered a system of n resonators, and made the seemingly plausible assumption that the entropy of the system is functionally related to the total energy of the system in the same way that entropy and energy are related for a single resonator. He then imagined this system displaced from equilibrium with the radiation field, and obtained the entropy change of the entire system as a function of $f(U)$. This line of argument led directly to the result

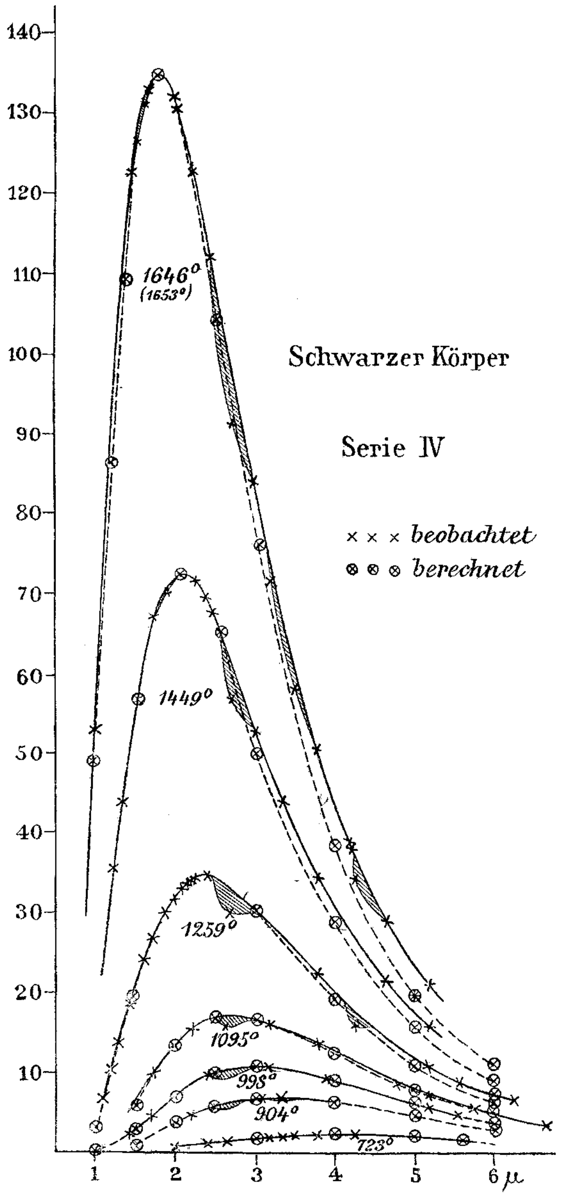

Fig. 2. Graph of black-body radiation for a range of temperatures. Wavelength is plotted on the horizontal axis, and radiation intensity on the vertical. The data are those of Lummer and Pringsheim from November 1899, which were the first to suggest a possible deviation from Wien's law. It was not at first clear whether the deviations between the solid curve representing the data, and the dashed curve calculated from Wien's law, might represent systematic errors in the experiment. Note that at these temperatures, the measurements lie well into the infrared region of the spectrum (that is, all wavelengths are longer than the 0.7 μ that marks the upper limit of visible light). *Source:* O. Lummer and E. Pringsheim, "1. Die Vertheilung der Energie im Spectrum des schwarzen Körpers und des blanken Platins; 2. Temperaturbestimmung fester glühender Körper," *Verhandlungen der Deutschen Physikalischen Gesellschaft* **1** (1899), 215–235, on p. 217.

$f(U) \propto 1/U$.[29] Integrating twice and using Wien's displacement law* immediately yields Eq. (4). Planck seemed all the more confident that he was on the right track. As he wryly observed, "my view of the significance of [this result] has become even stronger, even if the foundation on which it rests has in part shifted somewhat." Ironically, it would turn out, only a few months later, that his new derivation failed at just this point.

In his long introduction to this article, Planck summarized the foundations of his theory, and gave a careful explanation of why he thought it sensible to talk about the entropy of a single resonator, even though one cannot speak of the entropy of a single gas molecule. He observed that in contrast to a single gas molecule, which contains only a vanishingly small part of the total energy,**

> in radiation theory the energy of a single resonator is of the same order of magnitude as that of free radiation in a volume very large compared to the dimensions of a resonator. Accordingly, the stationary radiation of a resonator with specific period, situated in a stationary radiation field, cannot be described by a uniform elementary process, for example a simple sine wave with constant amplitude and phase – for then the energy of the oscillation must be freely transformable into work, and one could not define entropy – but on the contrary it consists of a superposition of very many small individual oscillations with nearly equal periods and constant amplitudes and phases, or, what amounts to the same thing mathematically, a single oscillation with constant finite amplitude but irregularly varying phase. In this case one can speak of the disorder, and hence also of an entropy and a temperature, of the resonator.[30]

It is worth pointing out that with the publication of these two papers early in 1900, Planck was in possession of all but one of the tools, conceptual and mathematical, that he used later that year to find a new and more accurate expression for the radiation density. They include his hypothesis of natural radiation and its close connection to the entropy of a single resonator; the relation between radiation density and average oscillator energy U, Eq. (1); the use of the thermodynamic equation for absolute temperature, Eq. (3), to find U from the entropy; the displacement law in the form $S = F(U/\nu)$; and not least, his condition

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* Not to be confused with Wien's law, Eq. (4). Wien's displacement law, a very general constraint on u_ν that Wien derived in 1893, states that the energy density of electromagnetic radiation in equilibrium within a cavity must take the form $u_\nu = \nu^3 f(\nu/T)$, where f is an arbitrary function of the single variable ν/T. Intuitively, it implies that at the maxima of the curves in figure 2, the quantity $\nu_{\max}/T$ (or in terms of the wavelength λ, the quantity $\lambda_{\max}T$) is a constant – that is, the peak of the curve shifts to higher frequencies (shorter wavelengths) as the temperature of the radiating object increases, just the behavior one expects as an iron bar is heated. Planck showed implicitly in his paper "Entropie und Temperatur" (ref. 28), pp. 732–733 and *PAV* (ref. 19), Vol. I, pp. 681–682, and explicitly in January 1901, that the displacement law, together with Eq. (1), implies that the entropy of a resonator must take the general form $S = F(U/\nu)$: Eq. (1) and the displacement law imply that $U/\nu = g(\nu/T)$, where g is some unknown function. Using $1/T = \partial S/\partial U$ and integrating, it follows at once that S must be a function of the single variable U/ν.

** Except as noted, translations are my own.

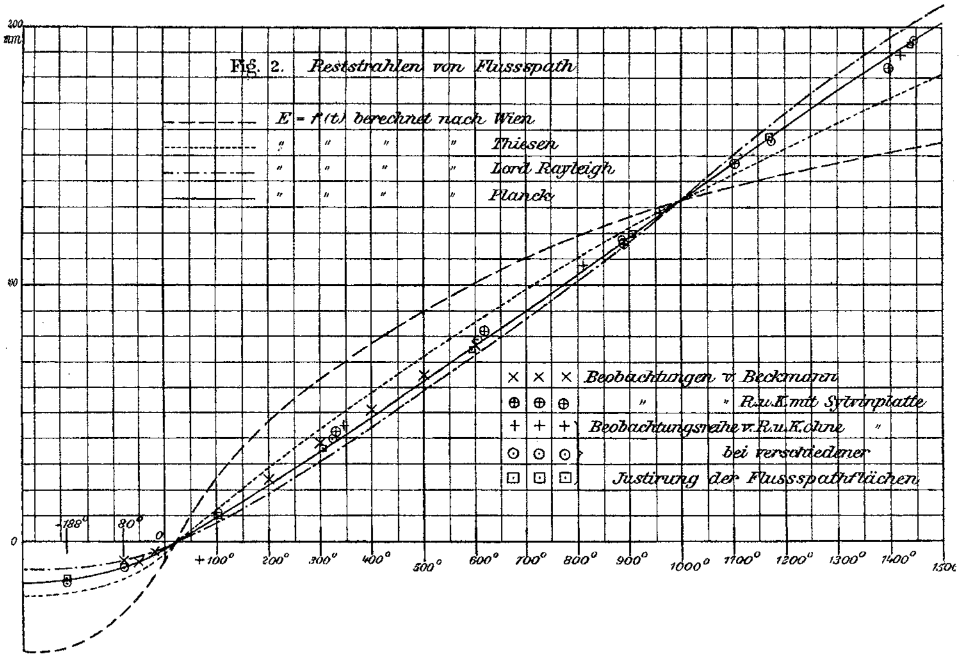

Fig. 3. Data of Rubens and Kurlbaum from the fall of 1900. The plot shows radiation intensity on the vertical axis, and temperature on the horizontal, at a *single* wavelength. Note that this wavelength, 24 μ, extends considerably farther into infrared than the data shown in figure 2. Note also that Wien's law clearly fails, but Planck's law describes the data very well. *Source:* H. Rubens and F. Kurlbaum, "Anwendung der Methode der Reststrahlen zur Prüfung des Strahlungsgesetzes," *Annalen der Physik* **4** (1901), 649–666, on p. 659.

that the second derivative, d^2S/dU^2, be negative. As the story unfolds, we will see how he deployed these tools, one after another, to find the equation for the radiation density u_ν with which we are familiar today.

The first step came in the fall of 1900, when the latest infrared measurements from the PTR experimentalists showed unmistakable deviations from Wien's law (figure 3).[31] Planck, who early in 1900 must have thought the problem solved, was forced to reconsider. In a short paper delivered to the German Physical Society in October 1900,[32] he noted that the assumptions involved in his derivation of $f(U)$ in March apparently did not hold, and observed that this circumstance was "not easily understandable" – perhaps an early indication for him of just how unusual his resonators were to become. He again used the second-derivative condition for the entropy that he had introduced earlier that year; but this time, instead of setting that derivative proportional to $-1/U$, he used the limiting behaviors at long and short wavelengths to write the equation in the form $d^2S/dU^2 = \alpha/U(\beta + U)$, where α and β are constants.[33] Integration, together with Wien's displacement law – the same technique he had used earlier in the year – yielded a new spectral distribution law. Planck for once expressed it in terms of wavelength, presumably for ease of comparison with the new infrared measurements.* It soon became apparent that this new law agreed well with experiment, as it has done ever since.

Nevertheless, as Planck later put it, his result had merely the standing of a "fortunate guess."[34] Planck, who only a few months earlier thought that he had a solid derivation of a unique result, now found himself in possession of a *new* law, in good agreement with observation, but entirely lacking in theoretical justification. In this way he was led to Boltzmann's 1877 combinatorial definition of entropy. In a letter to the PTR experimentalist Otto Lummer on October 26, 1900, Planck already talked about Boltzmann's theory of combinatorials; and a few weeks later, in a letter to Wien, he said that he now had a theory to support his new result.[35] To understand that new theory, we must first describe what Boltzmann had done in 1877.

Boltzmann's 1877 Combinatorial Definition of Entropy

In 1866, the Austrian theoretical physicist Ludwig Boltzmann published the first of what was to be a long series of fundamental papers on what we now call statistical mechanics.[36] In 1877, responding to a criticism from his friend Josef Loschmidt, he extended that work by demonstrating the fundamentally statistical nature of the second law of thermodynamics. In the process, he derived his now-famous relationship between the entropy of a system and the probability of finding it in some particular state.[37]

Boltzmann began by considering an illustrative if physically unrealistic model consisting of n gas molecules: Each molecule could have only the discrete kinetic energies $0, \varepsilon, 2\varepsilon, \ldots p\varepsilon$, and the total kinetic energy of the system is fixed at some integer multiple of ε. Boltzmann described his method in the following terms:

> I will seek out all combinations that are possible by the distribution of $p+1$ kinetic energies among n molecules, and then show how many of these combinations correspond to each state distribution [*Zustandsverteilung*].[38]

Boltzmann called each combination a "complexion" (today, it is usually called a microstate), and assumed that each complexion is equally probable. The discrete kinetic energies permit the use of combinatorials to find the number of complexions corresponding to a given macroscopic state (or "state distribution"). In turn, the number of complexions is proportional to the probability of finding the gas in that state. The final step is to relate the entropy to the logarithm of that probability.

Even before writing down the equations, Boltzmann gave a careful description of his approach and worked out a numerical example for a small system of 7 molecules with a total fixed energy of 7ε. That example helps to understand both his approach, and the very different way in which Planck later used it. It is shown in table 1:[39]

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* This result, $E_\lambda = C\lambda^{-5}/(e^{c/\lambda T} - 1)$, where c and C are both constants and λ is the wavelength, is equivalent to Eq. (11) below, which is written in terms of frequency instead of wavelength.

In this example, there are 15 possible "state distributions," or macrostates, numbered in the first column. The second column gives one of the possible complexions corresponding to that state. Thus, in the first state, six molecules have no energy, and the seventh an energy 7ε, as shown in the second column. But any of the seven molecules can have that energy, and so there are seven complexions, as shown in the third column (headed **B**). In the second state, five molecules have no energy, one has an energy ε, and the last one an energy 6ε. It takes only a moment to convince oneself that there are 42 ways in which the energy can be distributed to achieve this state.* Note that the total number of complexions for all fifteen states (the sum of the numbers in the third column of table 1) is 1716. Note also that the most probable state is number 10, with 420 complexions – many more than the next most probable, as Boltzmann pointed out.

Boltzmann next showed that in general, for a gas of n molecules, the number of complexions corresponding to the state characterized by w_0 molecules with zero energy, w_1 with energy ε, w_2 with energy 2ε, and so on, is given by

$$\mathbf{B} = \frac{n!}{w_0!w_1!w_2!\ldots}, \tag{5}$$

and noted without proof that the total number of complexions J is given by

$$J = \binom{\lambda + n - 1}{\lambda} = \frac{(\lambda + n - 1)!}{(n-1)!\lambda!}, \tag{6}$$

Table 1. Boltzmann's example from 1877 showing how discrete energies are distributed among molecules to form equally probable complexions. Here, 7 molecules share, in all possible ways, a total energy of 7ε, as explained in the text. Typically, many complexions are consistent with a single macroscopic state. Boltzmann equated the most probable state – that is, the state with the largest number of complexions (here number 10) – with the state corresponding to thermal equilibrium.

		B			**B**
1.	0000007	7	9.	0001114	140
2.	0000016	42	10.	0001123	420
3.	0000025	42	11.	0001222	140
4.	0000034	42	12.	0011113	105
5.	0000115	105	13.	0011122	210
6.	0000124	210	14.	0111112	42
7.	0000133	105	15.	1111111	1
8.	0000223	105			

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* For example, imagine seven columns of a spreadsheet, corresponding to the seven molecules. Each row of the spreadsheet represents a single complexion. In the first six rows, place a 1 (for 1 energy unit – a phrase Boltzmann does not use) in the first column. The molecule with 6 energy units can be in any of the other six columns – making six possible complexions. Then, in the next six rows, place a 1 in the second column; again, the molecule with 6 energy units can be in any of the other six columns, for another six complexions. And so on. Note, of course, that Eq. (5), with $n = 7$, $w_0 = 5$, $w_1 = w_6 = 1$, and $w_2 = w_3 = w_4 = w_5 = 0$, yields the same result – a total of $\mathbf{B} = 42$ complexions.

Fig. 4. Ludwig Boltzmann (1844–1906), as he appeared in 1875. Credit: American Institute of Physics, Emilio Segrè Visual Archives, Segrè Collection.

where λ is defined by the condition that the fixed total energy of the system is $\lambda\varepsilon$. (Boltzmann used only the first of these two equivalent forms.) He introduced J to define the probability of a state as the number of complexions for that state $\mathbf{B}$, divided by the total number of complexions J. To find the most probable state, Boltzmann maximized the logarithm of $\mathbf{B}$ subject to the constraints that the number of molecules and the total energy of the system are fixed. He developed a complex and lengthy scheme for calculating the quantities w_0, w_1, w_2 … corresponding to the most probable state.

Having used this simple model to explain his method, Boltzmann (figure 4) went on to consider the more realistic case in which molecular kinetic energies are continuous. First, he partitioned energy space into ranges, or cells, so that a given molecule has an energy lying within a range (0 to ε, ε to 2ε, 2ε to 3ε, and so on). Thus, instead of distributing discrete energies among molecules, one distributes molecules among energy cells, using the same combinatoric equations as before. Boltzmann then showed that this method does not quite work – if complexions are to be equally probable, it is necessary to choose cells representing small, equal regions of what we today would call phase space, rather than equal energy intervals.* He made these cells small enough to justify the conversion from discrete sums to integrals, and showed that the most probable state does indeed correspond to the Maxwell-Boltzmann distribution.** He further showed that one can relate the standard expression for the entropy of an ideal gas to the logarithm of the probability of the most probable state.[40] More generally, he had established, quantitatively, the statistical nature of the second law.

We do not know when Planck first encountered Boltzmann's 1877 paper – in contrast to his H-theorem, it was not widely known at the turn of the last century. Kirchhoff's thermodynamics text, which Planck had edited, cites several of Boltzmann's papers from this period, though not this one. (Kirchhoff's book also gives a solid introduction to the phase-space concepts that Boltzmann used; thus, these ideas would have been familiar to Planck in 1900.[41]) Boltzmann briefly described his 1877 paper in the first volume of his *Lectures on Gas Theory*, published in 1896.[42] Planck cited that work in one of his early 1900 *Annalen* papers; it is possible that he learned of the 1877 paper from Boltzmann's book, if he had not come across it earlier.

Boltzmann's discrete energies, introduced as an unphysical but easily understood model, were to reappear in 1900 in the guise of Planck's "energy elements" – the last of the tools he would need for his famous papers of December 1900 and January 1901. Moreover, the question of whether Planck also intended, without

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* By phase space, one means a multidimensional space in which the axes represent the Cartesian components of position and momentum for a molecule or a system of molecules. Both Boltzmann and Planck sometimes used velocities instead of momenta. In 1877, Boltzmann justified his choice of small cells in phase space through an appeal to a restricted form of Liouville's theorem (he does not use that name) that applies only to an ideal gas; see Sommerfeld, *Statistical Mechanics* (ref. 22), Chapter 4, for a clear discussion. As Sommerfeld points out, the association of small cells in phase space with equal probabilities is "in the last resort" a postulate, to be "decided in the light of experience." Nevertheless, Liouville's theorem makes this choice highly plausible. And Boltzmann's 1877 treatment likewise makes it highly implausible that energy units, or cells in energy space, are an appropriate choice. By 1906, Planck had seen a more general version of Liouville's theorem applied to ensembles moving through phase space, much as it is used today, both in the second volume (1898) of Boltzmann's *Lectures* (ref. 42) and in Gibbs's 1902 book (ref. 72); he may well have seen the former by 1900. Although Planck does not say how he thought about these matters in December 1900 and January 1901, and does not make explicit reference to them even in his 1906 *Lectures on the Theory of Heat Radiation* (ref. 48), we will see below that he understood Boltzmann's choice. He likewise appeared struck by his inability to justify the equiprobable nature of his own complexions, which as we shall see, were based on his "energy elements," without reference to phase space. See also Kuhn, *Quantum Discontinuity* (ref. 5), pp. 120–123.

** If one considers the logarithm of Eq. (5), then omitting constants and using Stirling's approximation, it is straightforward to show that $\log \mathbf{B} = -\Sigma\, w_k \log w_k$. It is this sum that goes over to an integral when cells in phase space become arbitrarily small. The question of how Boltzmann took the limit can be a little tricky, since cells are formally infinitesimal, but must nevertheless be big enough to hold large numbers of molecules; see for example Sommerfeld, *Statistical Mechanics* (ref. 22), § 28, p. 216 of the translation, and Darrigol, *c-Numbers* (ref. 8), p. 65.

saying so, to adopt Boltzmann's more realistic strategy of distributing molecules with continuous energies among cells is at the heart of the dispute among historians. Note, however, that in adopting Boltzmann's formalism, Planck did *not* adopt his probabilistic viewpoint, in which violations of the second law of thermodynamics are no more than highly improbable. For Planck, the second law continued to have absolute validity, a position he maintained until around 1914. The number of complexions became for him less a probability, although he used that word, than a quantitative measure of the disorder in a single resonator, inherent in his hypothesis of natural radiation.

Planck's 1900 and 1901 Derivations of his Radiation Law

In December 1900, Planck presented a derivation of his new spectral distribution law at a meeting of the German Physical Society,[43] and in January 1901 he submitted the same work spread over two papers* to the *Annalen*.[44] The December 1900 and January 1901 presentations have much in common, but one central point – the derivation itself – is handled differently. This circumstance, as we shall see, plays a role in the ensuing controversy among historians.

The discussion of "disorder" and its relation to entropy is much the same in the two presentations. In both, Planck observed that "entropy means disorder." He extended his analysis earlier in 1900 to associate disorder with irregular variations in *both* the amplitude and phase of a single resonator.** This association in turn is closely connected to his idea of natural radiation – all ideas he had worked with for some time. What was new was his use of Boltzmann's methods to seek a quantitative measure of that disorder – and here, a key step was his assumption that the time-averaged energy of a *single* resonator is equivalent to an average of the instantaneous values of the energy of a large number of resonators.

Thus, in December 1900, Planck cited Boltzmann, "especially" his 1877 paper, and said that by introducing probability considerations, he could derive an expression for the entropy of a resonator. He argued that "the entropy of a system of resonators with given energy is proportional to the logarithm of the total number of possible complexions." He then broke this statement up into two separate theorems:

- The entropy is proportional to the logarithm of the probability of a state; and
- The probability of any state is proportional to the number of corresponding complexions. Further, all complexions are equally probable.

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* The first of Planck's January 1901 *Annalen* papers, "Normalspectrum" (ref. 44), contained his derivation and most of the discussion; the second, much shorter paper, "Elementarquanta" (ref. 44), followed immediately after the first, and explored the implications of one of Planck's new constants.

** Planck introduced this extension without comment. In March 1900, he had related disorder only to random changes in the phase of a resonator (see above). At the risk of being overly speculative, one might wonder if his use of the instantaneous values of the energy of an ensemble of N resonators (see below) to calculate the time-averaged energy U of a single resonator led him to make this change. It may even be related to the failure of his March derivation showing that $f(U) \propto -1/U$; in this connection, see Stehle, *Order* (ref. 10), pp. 112–114.

The first theorem, he said, is no more than a definition. The second he described as the "core of the whole theory," but added that "in the last resort, its proof can only be given empirically."

Of the two versions of Planck's derivation, the first, given in December, is no more than a sketch. Planck presented an outline for one possible approach, but then said that a simpler and "more general" second method, which he did not give, achieves his goal "much more directly." The first of the January *Annalen* papers described this "more general" method.

In his December 1900 sketch, Planck considered a large number of resonators, N with frequency ν, N' with frequency ν', and so on, with a total energy $E + E' + \cdots = E_0$. He then turned to the first group, and noted that if E is continuously divisible, energy can be distributed among the N resonators in an infinite number of ways. To avoid this dilemma, in what he described as "the most essential point of the whole calculation," he supposed E to consist of P "energy elements" ε of size $h\nu$, where h is a "constant of nature" to which he assigned the numerical value of 6.55×10^{-27} erg-sec. The number of energy elements P is thus the ratio E/ε, in which connection Planck said: "If the ratio thus calculated is not an integer, we take for P an integer in the neighborhood." He did not repeat this caveat, either in his January 1901 *Annalen* treatment or in later accounts.

Planck next introduced Boltzmann's notion of a complexion, speaking, in language reminiscent of Boltzmann's, of the "distribution of P energy elements over the N resonators." He provided a numerical example for 10 resonators and 100 energy elements, and noted that "from the theory of permutations" the total number of complexions is given by

$$\frac{N(N+1)(N+2)\cdots(N+P-1)}{1\,.\,2\,.\,3\ldots P} = \frac{(N+P-1)!}{(N-1)!P!}\,. \tag{7}$$

The form in which he wrote this expression suggests that he had found it in a text on combinatorial theory;* he is also likely to have seen it in Boltzmann's 1877 paper where, as already noted, Boltzmann presented it without proof, as the total number of complexions for all states.

Planck then envisioned repeating this procedure for all other sets of oscillators at different frequencies, and then multiplying the individual complexion numbers to obtain the total number of complexions. Again following Boltzmann, he noted that for one particular distribution of the total energy E_0 among different frequencies, the number of complexions will be larger than any other, and envisioned finding it "if necessary by trial." Although in this December 1900 sketch, Planck's combinatorial starting point was different from Boltzmann's, his overall strategy was similar: Seek out the state with the maximum number of complexions, corresponding to the equilibrium distribution.

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* The left-hand side of Eq. (7) is sometimes found in mathematics texts, where it both hints at the mathematical meaning and suggests a calculational scheme. See for example William Allen Whitworth, *Choice and Chance* (New York: Hafner, 1959, reprint of 1901 5th edition), p. 63. In 1900, Whitworth's book was in its 4th edition, and might even have been available to Planck. See also Kuhn, *Quantum Discontinuity* (ref. 5), p. 283.

The rest of the paper is a highly abbreviated version of his January 1901 treatment, described below. Planck never actually carried out the derivation he had sketched in December 1900, nor did he ever return to it, apart from one very brief reference later in 1901.[45] But as we shall see, Kuhn has argued that this sketch is nevertheless central for understanding Planck's second and much more complete derivation.

Planck began his January 1901 treatment with a general discussion of the method that in the previous month he had described as simpler and more general. Having outlined the approach for a single resonator – find the entropy S and use it to find the average oscillator energy U – he observed: "The normal energy distribution is just the one for which the radiation densities of all different oscillation frequencies have the same temperature."

Planck had made a similar statement early in 1900. It is nevertheless tempting to suppose that in repeating it in January 1901, he had realized that the complex maximization procedure he had envisioned the previous month was unnecessary.[46] In any event, he began in much the same way, noting the equivalence of time and ensemble averages, repeating that entropy implies disorder, and explaining in a little more detail why one can speak of the entropy of a single resonator. He wrote the total energy and total entropy of his N resonators as $U_N = NU$ and $S_N = NS$ respectively, and this time citing only Boltzmann's 1877 paper, defined that total entropy as $S_N = k \log W + \text{const.}$ – the first time this now-famous equation had appeared in print (figure 5). Planck called W the probability that "the N resonators taken together have an energy U_N." He noted again the necessity of considering U_N as a whole number of finite energy elements ε, $U_N = P\varepsilon$, though this time he did not immediately set ε equal to $h\nu$.

At this point, the conceptual difference between the two derivations appears. In December, as we have seen, Planck had considered resonators at all frequencies, and had sketched a way of finding a *subset* of complexions that corresponds to the most probable state. Conceptually, this procedure was identical to Boltzmann's, however much the details of the two calculations differed.

In January, Planck's method was strikingly different. To be sure, it started off in the same way. He wrote down the same complexion example as he had in December, and stated Eq. (7) in exactly the same form, this time setting it equal to **R**. He considered resonators at a single frequency, and used Eq. (7) [equivalent to Boltzmann's J in Eq. (6)] to find the total number of complexions at that frequency. So far, the method is the same as in December. But now came the difference. Planck no longer considered other frequencies. He no longer used Eq. (7) as a first step in finding the subset of complexions that corresponds to the most probable state. Indeed, there is no maximization at all, nor any mention of a most probable state. Instead, with very little comment or explanation, Planck simply took Eq. (7) – that is, all possible ways of allocating P energy elements among N resonators at a single frequency – as his combinatorial starting point, and went on from there. It is worth repeating that Boltzmann's method was not widely known in 1900, and it is unlikely that many of Planck's readers would have noticed the change in emphasis, or wondered about the reasons for it. Nevertheless, Planck's starting point is conceptually very different from Boltzmann's, and his reasons for introducing it are not at all transparent – a theme we will revisit below.

Planck continued by using Stirling's approximation ($M! \cong M^M$ for large M, in the form Planck used it) to show that for large N and P, Eq. (7) reduces to

$$\mathbf{R} = \frac{(N+P)^{N+P}}{N^N P^P}, \tag{8}$$

the quantity that he now set equal to the probability W. The rest is manipulation, in which Planck deployed the same tools he had developed early in 1900 to find the entropy and average energy of a resonator. He had already stated that $U_N = NU = P\varepsilon$; and since $S_N = k \log \mathbf{R}$ (dropping the arbitrary constant), it follows in a few lines that the entropy of a single resonator, $S = S_N/N$, is given by

$$S = k\left\{\left(1+\frac{U}{\varepsilon}\right)\log\left(1+\frac{U}{\varepsilon}\right) - \frac{U}{\varepsilon}\log\frac{U}{\varepsilon}\right\}. \tag{9}$$

Fig. 5. Ludwig Boltzmann memorial grave in the Vienna Central Cemetery. The tombstone, dedicated in 1933, carries the famous relationship between entropy and probability developed by Boltzmann in 1877 and first written down in this form by Planck in 1901. Obscured on the right are the names of Boltzmann's son and daughter-in-law, Arthur Boltzmann (1881–1952) and Paula Boltzmann (1891–1977), who along with Boltzmann's wife, Henriette Boltzmann (1854–1938), and grandson, Ludwig Boltzmann (1923–1943), are also buried here. Credit: Daniel Weselka.

Planck next derived Wien's displacement law in the form $S = S(U/\nu)$. To satisfy that condition, Planck had to set his energy element equal to $h\nu$, whereupon Eq. (9) immediately becomes

$$S = k\left\{\left(1 + \frac{U}{h\nu}\right)\log\left(1 + \frac{U}{h\nu}\right) - \frac{U}{h\nu}\log\frac{U}{h\nu}\right\}. \tag{10}$$

At this point, he noted that h and k are "universal constants." Then, he used the thermodynamic definition of absolute temperature, $1/T = dS/dU$, to show that the average resonator energy U is given by $U = h\nu/(e^{h\nu/kT} - 1)$. This result, together with Eq. (1), yields the radiation density,

$$u_\nu = \frac{8\pi h\nu^3}{c^3}\frac{1}{e^{h\nu/kT} - 1}\,. \tag{11}$$

Planck concluded his first January 1901 paper by calculating the numerical values of h and k. He continued that discussion in a second, much shorter paper that followed immediately thereafter in the *Annalen*. Here he discussed only the constant k (now called Boltzmann's constant), and argued that it should take the same value for both Boltzmann's ideal gas, and his own resonators. Then he showed that k can be related to Avogadro's number (as we call it today – Planck called it Loschmidt's number), as well as to the electron charge (which he called the elementary quantum (or unit) of electricity), and to what he called the Boltzmann-Drude constant, $\frac{3}{2}k$, the "average kinetic energy of an atom" at unit temperature. As Martin J. Klein has emphasized, Planck attributed the greatest importance to these constants – he says nothing about the physical significance of his energy elements, but his discussion of the new constants is not the least bit tentative. Nevertheless, there is an interesting distinction: In both December 1900 and January 1901 he said very little about h, other than to identify it as a universal constant; by contrast, he had a great deal to say about k. In this respect, his new constants represent a considerable advance over the related constants a and b in Wien's law, Eq. (4) – those earlier constants did not admit of an obvious physical interpretation. But his new constant k clearly did. One should *not* infer that he thought h less important – among other things, he showed implicitly that the Stefan-Boltzmann constant depends on both h and k. And later in 1901, he showed the relation of both new constants to the natural system of units he had introduced in 1899 and 1900.[47] One may, however, reasonably suspect that he was puzzled about the physical meaning of h and its relation to his energy elements, and was not prepared to speculate about such matters.

Planck's treatment leaves a number of questions unanswered. How can one justify his combinatorial starting point, so different from Boltzmann's? Why should Planck's complexions, which represent distributions of energy elements, be equally probable? How are they related to Boltzmann's complexions, which assume small cells of equal probability in phase space? Perhaps most important, what for Planck was the significance of his energy elements and his new universal constant h? Historians have indulged in a great deal of conjecture on these questions; but Planck himself doesn't say. He returned to this derivation in his 1906 *Lectures on* *the Theory of Heat Radiation*,* where he gave a detailed treatment of his entire theory of resonators and natural radiation.[48] And it appears once more in his Columbia Lectures, a series of lectures he delivered at Columbia University in the spring of 1909[49] – by which time all historians agree that he had accepted the possibility of quantization. But even in these later treatments, he said remarkably little about the interpretation of his energy elements.

Some physicists and historians have suggested that Planck, being new to kinetic theory in 1900, did not fully understand Boltzmann's approach. This point of view can be overstated. As we have seen, he in fact had been introduced to kinetic theory some years earlier, and as we shall see shortly, even had a better understanding of equipartition than is often assumed. More evidence may be found in a paper he wrote for a Festschrift in 1901, and reprinted in the *Annalen* in 1902.[50] The purpose of that paper was to establish in more detail the universal nature of the constant k, showing that it applies to both gases and thermal radiation. But in the process, Planck gave a careful summary of Boltzmann's 1877 calculation for a gas, in which he showed how Boltzmann had found the *subset* of complexions that corresponds to the most probable state, and thus to thermal equilibrium. He understood how Boltzmann had gone from the equivalent of energy elements to ranges, or cells, in the process of converting sums to integrals. Planck also understood that these cells, or "elementary domains" as he called them, must be defined in phase space, not energy space, if complexions are to be equally probable (though he did not discuss this point explicitly). Thus, he surely understood how different his own derivation was from Boltzmann's. He nevertheless said nothing about those differences, or about what if any relationship might exist between Boltzmann's arbitrarily small cells in phase space, and his own finite energy elements. His inability to say why his own complexions were equally probable must have seemed especially mysterious.

Planck and Equipartition

The equipartition theorem states that, on average, the energy of a molecule (or one of Planck's resonators) is uniformly distributed over all possible modes of motion; or a little more precisely, that any separable, quadratic momentum or position term in the expression for the energy will result in a contribution to the average thermal energy equal to $\frac{1}{2}\,kT$. For example, a gas molecule considered as a point has three such terms (corresponding to the kinetic energy in each of the three independent spatial directions), and so has an average energy of $\frac{3}{2}\,kT$. A

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* The 1906 edition of Planck's *Lectures* was never translated into English, and should not be confused with the very different second edition of 1913, which is widely available in translation. By 1913, Planck's theory had changed substantially. Both the German first edition and the translation of the second are available in an edition published in 1988 by the American Institute of Physics (ref. 48). The translations from Planck's 1906 *Lectures* given here are in part my own, but in part guided by those of Kuhn, Darrigol, Needell, and some unpublished notes kindly made available to me by Roger H. Stuewer. My own rough translation of a handful of the central sections of the 1906 edition is available on my web site, http://www.employees.csbsju.edu/cgearhart.

resonator has two such terms, one each for its potential and kinetic energies, and so should have an average energy of kT. By Eq. (1), therefore, the radiation density should be proportional to $\nu^2 T$, as Lord Rayleigh argued from a different perspective in a short paper published in June 1900.[51] In fact, this result is not such a bad description of the data at sufficiently long wavelengths (see figure 3), though it is clearly absurd in other respects – it increases without limit at short wavelengths, a result Paul Ehrenfest later dubbed the "ultraviolet catastrophe."

It has always seemed surprising that Planck in his 1900–1901 papers made no mention of the equipartition law. (References hereafter to Planck's work in "1900–1901" refer collectively to the December 1900 paper and the two papers from January 1901.) He certainly knew of it by December 1900. Kirchhoff's thermodynamics text, which he had edited, contains a careful discussion of equipartition that even cites one of Boltzmann's papers.[52] And of course, Boltzmann's 1896–1898 *Lectures* treats equipartition. Planck's reference in January 1901 to the "Boltzmann-Drude constant," noted above, provides additional evidence: Paul Drude's electron theory of metals relies in a central way on equipartition, and thus Planck's reference to Drude's work here likewise suggests an acquaintance with this law.[53] These facts lend credibility to Planck's statement in 1910 that he had known that letting h go to zero would result in "Jeans's formula" – that is, the equipartition result, but also the long-wavelength limit of Planck's new formula – "long before Jeans supplied a rigorous proof."[54] (Planck's allusion is probably to a 1905 paper of James Jeans.[55]) Planck's statement can even be read as hinting that in 1900 he already knew of Rayleigh's paper.

There were many reasons to be skeptical of the equipartition law, which as both Boltzmann and Maxwell had emphasized, could not be reconciled with the specific heats of gases.* Planck in any case had long been suspicious of kinetic theory. Moreover, the equipartition law was invariably presented in the context of gas theory, so it is possible that at first, Planck did not see its relevance to his resonators. But once he knew of Rayleigh's paper, he could scarcely have missed the connection; and as Martin J. Klein has persuasively argued, Planck almost certainly knew of that paper late in 1900, but either did not understand its significance or doubted its applicability.[56] The evidence presented here suggests the latter.

Planck's Letters and Other Writings

Planck returned to his energy elements in his 1906 *Lectures*, a work I shall discuss below. But he also referred to them in a series of letters and other documents dating

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* The long-standing myth, often seen in older physics texts, that in the failure of equipartition, Planck thought himself faced in 1900 with a challenge to "classical physics," remains just that – an entirely unhistorical myth, however useful it may be pedagogically. The notion of "classical physics" is itself dubious, suggesting as it does a settled and well-established physical theory with few apparent difficulties. In 1900, as at most other times, physicists were faced with many seemingly intractable puzzles. The uncertain status of equipartition was one among many.

from 1905 and thereafter. Kuhn cites many of these documents and finds room in them for his interpretation. Nevertheless, it seems to me that all of them can be read as suggesting Planck took his energy elements seriously; and he was often more open in discussing their implications in his correspondence than in print.[57] Notably, nowhere in these passages does he allude to continuous resonator energies. Here are some prominent examples:

- letter from Planck to Paul Ehrenfest, July 6, 1905:
  > Resonator theory (including the hypothesis of natural radiation) does not suffice to derive the law of energy distribution in the normal spectrum, and the introduction of the *finite* energy quantum $\varepsilon = h\nu$ is an additional hypothesis, foreign to resonator theory itself.[58]
- letter from Planck to Wien, March 2, 1907:
  > Your measurement of the radiation emitted from the individual molecules naturally interests me a great deal. However, shouldn't the circumstance that this radiation amounts to considerably less than the elementary quantum of energy be explained by that fact that not all molecules emit uniformly, but, to the contrary, in general quite a few molecules do not emit at all while others emit either one full energy quantum or several energy quanta. The entire theory of the radiation entropy and the elementary quantum is based on the unequal and random distribution of energy among the individual molecules.[59]
- letter from Planck to Einstein, July 6, 1907:
  > … Does the absolute vacuum (the free aether) possess any atomistic properties? … [You] seem to answer the question in the affirmative, while I would answer it, at least in line with my present view, in the negative. For I do not seek the meaning of the quantum of action (light quantum) in the vacuum but at the sites of absorption and emission, and assume that the processes in the vacuum are described *exactly* by Maxwell's equations.[60]
- From a 1907 paper on special relativity:
  Planck showed that the action integral is a relativistic invariant, and remarked that his new constant h (which he had by then christened the "action element") has the units of action (energy multiplied by time), raising the possibility that "each and every change in nature corresponds to a definite number of action elements completely independent of the choice of coordinate system."[61]
- letter from Planck to H. A. Lorentz, October 7, 1908:
  > In sum, I might therefore say, I make two assumptions:
  > 1. The energy of the resonator at a given instant is $gh\nu$ (g a whole number or zero);
  > 2. the energy emitted and absorbed by a resonator during an interval containing many billion oscillations (and thus also the average energy of a resonator) is the same as it would be if the usual pendulum equation applied.
  >
  > These two assumptions do not seem to me to be incompatible. I have indicated these thoughts in a note to § 109 of my lectures on thermal radiation.[62]

  Kuhn cites this letter as the first occasion on which Planck makes explicit reference to quantized energies, and maintains that it represents a "clear departure" from his earlier views. Others, myself included, see it only as a more detailed statement of a reasonably consistent, if developing, viewpoint. Note especially his reference to a footnote in his 1906 *Lectures*;* he referred to it again in a 1910 paper we shall consider later, as well as in his *Lectures*, to express a position he maintained consistently: Maxwell's equations remain an entirely adequate description of electromagnetic radiation in a vacuum, but new and interesting physics was likely to emerge from a better understanding of his resonators.

- letter from Planck to the American physicist Robert W. Wood, October 7, 1931: In this letter, Planck responded to Wood's request for a description of the "considerations which had led me to propose the hypothesis of energy quanta." Planck spoke of his work as an "act of desperation," and said:

  > I also knew the formula that expresses the energy distribution in the normal spectrum. A theoretical interpretation therefore *had* to be found at any cost, no matter how high. It was clear to me that classical physics could offer no solution to this problem, and would have meant that all energy would eventually transfer from matter to radiation. … This approach was opened to me by maintaining the two laws of thermodynamics. The two laws, it seems to me, must be upheld under all circumstances. For the rest, I was ready to sacrifice every one of my previous convictions about physical laws. … [One] finds that the continuous loss of energy into radiation can be prevented by assuming that energy is forced at the outset to remain together in certain quanta. This was purely a formal assumption and I really did not give it much thought except that no matter what the cost, I must bring about a positive result.[63]

One should be careful of taking at face value recollections written thirty years after the event. Planck seems to say, for example, that his work had been motivated by a perceived inconsistency between experiment and the equipartition law (the context of his allusion to "classical physics"). I have argued above (in contrast to some historians) that he unquestionably understood this law in 1900. Nevertheless, there were many reasons to distrust the equipartition law in 1900, and there is nothing to suggest that it played a role in Planck's thought. Moreover, Planck did not say which aspect of his work represented an "act of desperation." His adoption of Boltzmann's combinatorial methods? His inability to explain why his complexions were equally probable? The energy elements themselves, or their importance in deriving his new constant h? All are possible; none is clear-cut or obvious. What does come through is Planck's sense – at least as he recalled it in 1931 – that he had taken a bold, even desperate step.

Arguments for Continuous Resonator Energies

Planck's use of Boltzmann's combinatorial ideas and his introduction of finite "energy elements" in 1900–1901 suggest to a modern reader something like

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* In § 109 of his *Lectures* (ref. 48), Planck said in a footnote that his derivation of the differential equation for a resonator "is always valid, if only sufficiently large values of T [the time over which an average is taken] come into consideration. For smaller values of T, one might possibly consider, in the place of the simple linear differential equation [considered here], another oscillation law that is even better adapted to the processes in nature."

quantized energy levels. Nevertheless, then and for many years thereafter, Planck was extraordinarily reticent in suggesting any physical interpretation of these energy elements and of his new constant *h*. And in later retrospective accounts, Planck stated that he had not suspected at first the far-reaching implications of his energy elements.[64] Yet he did realize from the beginning the importance of his new constants. This ambiguity is reflected in virtually all historical accounts. Thus, Martin J. Klein speaks of Planck's "quantizing the oscillators," but also emphasizes repeatedly how uncertain and ambiguous Planck was, in both his original papers and his later accounts.

Thomas S. Kuhn and Olivier Darrigol go a step further when they argue that, in effect, there is no ambiguity – Planck could not possibly have understood his results as suggesting that the energies of his resonators might be discontinuous. And in an important contribution to this debate, Allan Needell has argued persuasively that in focusing on whether Planck "quantized the oscillators," we are asking the wrong question: As he puts it, "The question of whether Planck introduced discontinuity in 1900 is not a major issue." We should instead be concentrating on Planck's view of the laws of thermodynamics, and his increasing suspicion following his introduction of "natural radiation" that one could make a division between a macroscopic realm, in which Maxwell's laws were rigorously obeyed, and a microscopic one, in which the behavior of the resonators could not be explained solely on the basis of Maxwell's laws and Hamiltonian dynamics.[65] Needell and Darrigol show considerable areas of agreement, and all of this work has led to a much better understanding of Planck. Nevertheless, the question of how Planck thought about his energy elements remains in dispute; for that reason, it is of interest to examine the basis of Kuhn's and Darrigol's arguments.

Energy elements or energy cells?

In his 1900–1901 papers, his 1906 *Lectures*, and his 1909 Columbia Lectures, Planck spoke consistently of "energy elements," and in doing so, seemed to be following Boltzmann. But as we have seen, although Boltzmann first used the equivalent of energy elements, he later replaced them with a more realistic calculation. In this second approach, he assumed molecules could take on continuous energies, and distributed them among "cells" – small ranges, first in energy space and then in phase space. Kuhn and Darrigol are convinced that Planck, without saying so, really had Boltzmann's more realistic calculation in mind. They also argue that Planck's viewpoint in 1900–1901 had not changed substantially by 1906 – when two passages in the *Lectures* mention energy cells; therefore, he must have had cells in mind in December 1900 and January 1901.[66] Thus, after reviewing Planck's distribution of discrete energy elements over resonators, Kuhn tells us:

> If this part of his presentation is taken literally, then his resonators can acquire only an integral number of energy elements $h\nu$, and they are therefore quantized … .

> Fortunately for the consistency of Planck's thought, these passages need not be read literally. When Planck wrote them in late 1900 and early 1901, he was carefully following Boltzmann's 1877 paper.[67]

After a brief review of Boltzmann's two approaches outlined above, one involving "energy elements," the other energy ranges or cells, Kuhn continues:

> 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. In his *Lectures* the substitution is explicit.[68]

Or as Darrigol puts it:

> What was a fiction to Boltzmann must also have been a fiction to Planck. In the real case, the energy of a resonator … can vary continuously, and energy cells must replace energy levels. Planck tolerated the fiction only because it did not affect the combinatorics. … [69]

Planck, it turns out, said nothing of the sort, either in 1900–1901 or 1906. We therefore have two questions: What did Planck say in 1906 that might justify this interpretation? And how solid is the evidence that Planck's viewpoint did not change materially between late 1900 and 1906?

Planck said in a 1910 letter that "for ten years, nothing in physics has so incessantly stimulated, agitated, and excited me as these new action elements."[70] His 1906 *Lectures* reveals a deepening of his understanding of kinetic theory as well.[71] His frequent use of the term "statistical mechanics" in his *Lectures* reflects two reviews he published, in 1903 and 1904, of J. Willard Gibbs's *Elementary Principles in Statistical Mechanics*, the book that gave kinetic theory this new name.[72] And whatever his understanding of equipartition in 1900, by 1906 he understood it very well. Three substantial sections of his *Lectures* (§§ 154, 165, and 166) cite not only Lord Rayleigh's paper of 1900, but additional papers by Rayleigh and by James Jeans in 1905. These sections give a detailed presentation of equipartition as Rayleigh and Jeans had applied it to electromagnetic radiation, and a discussion of the implications for his own theory. In particular, Planck clearly understood that the average energy U for his resonator should be kT if equipartition is correct, and that the same result follows if the size of the energy element $\varepsilon = h\nu$ shrinks to zero.

In 1906, Planck preceded his introduction of "energy elements" with a careful review of Boltzmann's 1877 derivation for an ideal gas (§§ 138–140), along the same lines as in his Festshrift paper of 1901. Then, in § 148, he turned to his system of N resonators, and in an introductory paragraph immediately preceding his derivation, he seemed to strike a new note when he spoke of

> … the number of resonators, which hold a definite quantity of energy (better: which lie in a definite "energy domain"). … [73]

Kuhn and Darrigol both argue that this passage, and a second one to be discussed below, signal a firm commitment to continuous resonator energies partitioned among Boltzmann-like cells – one, they say, that Planck must also have held in 1900–1901. I will take issue with both conclusions, and argue, by contrast, that even in 1906, his position was both uncertain and ambiguous. Moreover, I will suggest that it is far from obvious that Planck's viewpoint in 1906 was identical with what he had thought in December 1900 – he had learned a great deal about his resonators in the intervening years.

Planck's derivations of the entropy and energy of a resonator follow immediately after his parenthetical remark above. Nevertheless, those derivations were much as they had been in January 1901 – he still distributed P energy elements among his N resonators. Repeatedly, and unambiguously, he spoke of the number of energy elements allotted to a given resonator. He took the same combinatorial starting point as in January 1901, and much as he had done then, derived his expression for the entropy of a single resonator, Eq. (10). Again, he showed that Wien's displacement law requires that the energy elements have the fixed size $\varepsilon = h\,\nu$.

In the next two sections of his 1906 *Lectures*, however, Planck set off in a new direction, not found in 1900–1901. He first showed that his new universal constant h has the units of action (the product of energy and time), and for that reason he named it the elementary action quantum or action element (*elementares Wirkungsquantum* or *Wirkungselement*). He observed that:

> There can be no doubt that the constant h plays a definite role at an emission center of the elementary oscillation process. … [The] thermodynamics of radiation will have arrived at an entirely satisfactory conclusion only when the constant h is understood in its full universal significance.[74]

Planck suspected that such an understanding would have far-reaching consequences. He cited here an earlier section (§ 109, the same one he would cite in 1908, and again in 1910 – see footnote page 193), in which he had pointed to the potential need for "another oscillation law, that is even better adapted to the processes in nature."

The following section (§ 150) again suggests his deepened understanding of statistical mechanics. In this section, in which we find a second allusion to continuous energies, Planck offered a new interpretation of his action element. In a passage that cites both Gibbs's new book and the second volume of Boltzmann's *Gas Theory*, he considered a two-dimensional phase space whose axes are momentum and position:

> Let us consider … the coordinates of a point of the state-plane [*Zustandsebene*], and ask first for the magnitude of the probability that the energy of a resonator lies between the values U and $U + \Delta U$. This probability is measured by the size of the elementary area [*Flächenstück*] … which is bounded by the curves $U =$ const and $U + \Delta U =$ const.[75]

He showed that a resonator with constant energy describes an ellipse in that space, and that the area bounded by two such ellipses separated in energy by some ΔU is $\Delta U/\nu$, as shown in figure 6. Consequently if one equates ΔU with the

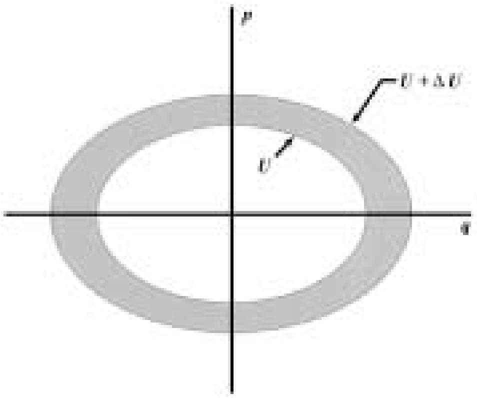

Fig. 6. Phase-space plot of momentum p *vs*. coordinate q for a simple harmonic oscillator. The two curves are ellipses and represent paths of constant energy. If $\Delta U = h\nu$, then the area of the shaded annulus is Planck's constant h.

energy element $h\nu$, then the area of the elliptical annulus is just the new universal constant h – that is, $\Delta U/\nu = h$. Planck noted that now he can set $\varepsilon = h\nu$ in his combinatorial derivation without invoking Wien's displacement law, and that:

> At the same time the elementary quantum of action h appears in a new significance, namely as the size of an elementary domain in the state-plane of a resonator.[76]

With this result, Planck had found, as he had not in 1900–1901, a way of relating his energy elements to cells in phase space, and so giving a clearer understanding of why his complexions are equally probable. He was nevertheless surprisingly reticent in discussing this point, saying only, in addition to the passages quoted above, that his calculation would permit a

> correct measurement of the "elementary domain" of the state-area whose size the calculation of the complexions will certainly be based upon, and consequently in the last analysis the measure furnished for the comparison of the probability of different states.[77]

Was that reticence related to the remarkable nature of these cells? Earlier in his *Lectures*, in his review of Boltzmann's 1877 derivation for an ideal gas, Planck spoke of cells in phase space, or in his phrase, "elementary domains," much as Boltzmann had done in 1877. But for Boltzmann, these domains had to be made arbitrarily small – and this step seemed essential in identifying the equilibrium state with the Maxwell-Boltzmann distribution for a gas. No such step is possible for Planck's finite phase-space domains of size h that are associated with his resonators – their size is fixed numerically by the need to agree with experiment. Several sections later, in § 166 – the discussion of equipartition – Planck made clear just how far-reaching this distinction becomes:

> I am therefore of the opinion that this difficulty [equipartition] has arisen only through an unjustified application of the theorem of equal energy distribution to all independent state variables. In fact, for the justification of this theorem, the assumption is essential that the distribution of states for all possible systems with given total energy is "ergodic" [here he cites Boltzmann's *Gas Theory*],* or, briefly expressed, that the probability that the state of a system lies in a definite small "elementary domain" (§ 150) is simply proportional to the size of that domain, if the latter is taken as ever so small [*noch so klein*]. *But this assumption is not satisfied by stationary radiant energy; for the elementary domains cannot be taken as arbitrarily small*, *but on the contrary their size is finite*, *determined by the value of the elementary action quantum h* [my italics]. Only if one could take the action element *h* as infinitely small, would one arrive at the law of equal energy distribution.[78]

The sections he cited from Volume 2 of Boltzmann's *Gas Theory* (§§ 35 and 36) come toward the end of a chapter in which Boltzmann gave a careful treatment of Liouville's theorem, and applied it to a discussion of ensembles of mechanical systems. He employed it much as we do today in textbook treatments of classical statistical mechanics – to show that a small element in phase space behaves like an incompressible fluid, preserving volume as it evolves in time. The point, of course, is that if the volume of a small element in phase space is constant in time, its use as a domain (or cell) of equal probability becomes more plausible. In the course of Boltzmann's discussion, it is apparent that these phase-space elements must be arbitrarily small, so that they may be treated mathematically as infinitesimals. Earlier, in 1877, Boltzmann had given a similar discussion, using there a more restricted form of Liouville's theorem.[79] In citing these sections of the *Gas Theory*, Planck appeared to understand that his finite action elements – however interpreted – have no place in Boltzmann's formalism, in ways that go well beyond equipartition.** Not surprisingly, Planck concluded this section by emphasizing again the need for a physical interpretation:

> Naturally, the action element *h* must receive a direct electrodynamic meaning; but of what sort remains for the present an open question.[80]

What are we to make of all this? It is of course possible that Kuhn and Darrigol are correct: Planck might be employing energy elements, instead of Boltzmann's cells, as a matter of convenience. But Planck nowhere *says* any such thing. To

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* Boltzmann (whom Planck is quoting) defined an "ergode" as an ensemble of systems all having the same energy, and distributed uniformly in phase space. It is similar to what Gibbs called a microcanonical ensemble. Thus, "ergodic" refers only to the ergode's uniform distribution in phase space. See the translator's note in Boltzmann's *Lectures* (ref. 42), p. 297.

** Darrigol argues that if Planck had not been guided by his notion of the entropy of a single resonator, he would have followed Boltzmann's derivation more closely, and would therefore have seen the necessity of letting his cell size, and therefore *h*, go to zero; see *c-Numbers* (ref. 8), p. 71, and "Meaning," (ref. 8), p. 19. This argument seems hard to follow in view of the foregoing; Planck had a good understanding of the differences between his and Boltzmann's derivations.

accept their picture thus demands a high price: We must assume that Planck said one thing, and meant another. Moreover, even if Planck had been thinking in terms of cells, we must also assume that he did not see the implications of the very different derivation – distributing discrete energy elements among resonators – that he in fact had given.

To be sure, it is not a trivial matter to derive Eq. (10), Planck's expression for the entropy of a resonator, on the assumption that resonators with continuous energies are distributed among finite cells in phase space – irrespective of whether one uses Planck's combinatorial starting point, or Boltzmann's. For either method, resonator energies will not be constant within a given cell; and the density of resonators can vary significantly from one cell to the next. In Boltzmann's derivation for an ideal gas, the phase-space cells become arbitrarily small. Hence these questions never arose, and Boltzmann never mentioned them. But they would have presented problems for Planck. Kuhn argues that in 1900–1901 and again in 1906, Planck had simply missed these points.[81] Kuhn thus asks us to make a string of assumptions: Not only had Planck spoken of energy elements when he really had cells in mind; he had also made what is, at least to a modern reader, a fairly elementary mistake, by not taking into account these variations in resonator energy within cells, and in resonator density from one cell to another. Might one not equally well assume that, in fact, he had noticed these problems, and for that reason, adopted energy elements instead of cells?

It is instructive to compare this 1906 treatment with one Planck adopted some years later, when in a very different derivation, he *did* make central use of phase-space cells. In 1913, in the second edition of his *Lectures*, Planck had found a way around these problems, based on a *new* theory he had started to formulate in 1911 – one that *did* assume continuous energies. In this new version, Planck had to argue that resonators are distributed uniformly within a given cell, in which case the average cell energy is the arithmetic mean of the energies of the bounding ellipses. Planck also showed in much greater detail, and, it seems to me, with much more confidence, how both his and Boltzmann's phase-space cells define what he now called "elementary domains of equal probability." This path eventually led him to a new derivation of the entropy S using Eq. (7), but his justification and interpretation of it were very different: This time, for example, there is no explicit mention of energy elements.[82]

However, there is no hint of any such approach in 1906, much less in 1900–1901. Even in 1906, Planck mentioned the possibility of continuous resonator energies only briefly, almost as an aside. His discussion of the significance of his new phase-space cells was at best terse. Most notably, he made no attempt to incorporate continuous energies partitioned among finite cells into the combinatorial derivation of the entropy and average energy of a resonator – as we have seen, he spoke unequivocally of a resonator having a definite number of energy elements. In short, Planck by no means settled on a single picture – he goes back and forth, as if blithely indifferent to the concerns that have plagued a generation of historians.

Both Kuhn and Darrigol imply that the equivalence of Boltzmann's two methods – energy elements on the one hand, and continuous energies partitioned among cells on the other – would have been so obvious to Planck that he did not so much as mention it.[83] I am suggesting, by contrast, that in 1906 the finite size of Planck's cells, in sharp contrast to Boltzmann's arbitrarily small phase-space domains, made this equivalence far from obvious. And in 1900–1901, Planck did not have these phase-space arguments at hand, so that the transition from energy elements to equiprobable cells surely would have seemed even more mysterious.

Let me propose another interpretation that has the advantage of taking Planck at his word. In 1906, Planck knew that his finite energy elements at some level might imply quantized resonator energies – he said as much, just before his parenthetical expression, quoted above, in which he alluded to energy domains. But he also knew that these energy elements are related to finite "elementary domains" in phase space. In this connection, he briefly mentioned continuous resonator energies, and judging from his parenthetical remark, found this interpretation attractive, however undeveloped it remained. But he did not settle firmly on any one picture; and he went out of his way to point out, repeatedly, the need for a *physical* interpretation. Apparently, neither his energy elements, nor his new insights into phase space, as yet qualified as such.

Planck knew that in the finite size of h, he had discovered something of fundamental importance. How to interpret the behavior of his idealized resonators was another matter, and Planck was clearly circumspect in even approaching this question. He had a result but not a theory – much less a dynamical theory – and he knew it. But in 1906, Planck did understand that he had come across something that was fundamentally new, and irreconcilable with physics as it had hitherto existed. I am not suggesting that Planck was inconsistent, or did not understand the implications of what he was doing. Rather, I am suggesting that Planck knew quite well what the possibilities were, but was not prepared even to speculate about them in print.

This interpretation finds support in Planck's 1909 Columbia Lectures. By that time, historians agree that he was taking quantized energies seriously, and as we have seen, he spoke openly of them in his correspondence. Nevertheless, in 1909, his treatment is almost identical to that of 1906. His phase-space derivation of the action element comes first this time, with the same reference to the probability that the energy of a resonator lies between the values U and $U + \Delta U$. His derivation of S, based on combinatorials and energy elements, follows immediately, and in much the same form it took in 1901 and 1906 – energy elements distributed among resonators. Moreover, Planck was still, in print, extremely reticent in talking about his energy elements:

> [The] theory would still require an extension as regards a certain point; for in it the physical meaning of the universal constant h remains quite unexplained … . [Several physicists] even believe that the propagation of electromagnetic waves in a pure vacuum does not occur precisely in accordance with the Maxwellian field equations, but in definite quanta $h\nu$. I am of the opinion, on the other hand, that at present it is not necessary to proceed in so revolutionary a manner and that one may come successfully through by seeking the significance of the energy quantum $h\nu$ solely in the mutual actions with which the resonators influence one another.[84]

What does this analysis tell us about Planck's viewpoint in 1900–1901? First, the argument that his viewpoint did not change between 1901 and 1906 seems difficult to support. In December 1900 and January 1901, Planck almost certainly did not have the insights into phase space and "elementary domains" that he showed five years later. It would have been a formidable challenge to develop a theory on the basis of finite, equal *energy* cells, and there is no evidence that such an approach ever occurred to him. And in any case, his point of view in 1906 seems far more ambiguous than Kuhn and Darrigol would have us believe, even more so in light of his correspondence. Above all, it seems to me that the finite energy elements are hard to get around. In December 1900, he described them as the "most essential point" of his calculation. Moreover, Planck summarized his January 1901 derivation on at least two later occasions, in 1911 and 1943; both times he spoke unambiguously of energy elements distributed among resonators.[85] Had he been thinking of cells, why would he not have said so?

Of course, both in 1900–1901 and in 1906, Planck said nothing about what his energy elements meant physically, and may well have supposed that they had only a formal significance. But certainly he understood their close relation to his new "universal constant" h. What did he *really* think? Planck doesn't say; and perhaps we could do worse than to take his silence seriously. It is hardly surprising that, faced with such an extraordinary result, Planck should have been circumspect in speculating about the possible implications of his energy elements.

Derivation of Equation (1)

Kuhn notes that the derivation of Eq. (1), $u_\nu = (8\pi\nu^2/c^3)U$, which relates the radiation density u_ν to the time-averaged resonator energy U, assumes that the resonators can exchange energy continuously with the radiation field. He argues that Planck would have found the inconsistency between that derivation and the implications of energy quanta difficult to accept. But Planck never mentions any such inconsistency. Therefore, according to Kuhn, he must have been assuming continuous resonator energies.[86]

Earlier historians had noted this apparent contradiction, and so had Planck's contemporaries, among them Albert Einstein, Peter Debye, and H. A. Lorentz.[87] The question here is less whether Planck would have noticed this inconsistency, than how seriously he would have regarded it. Planck himself spoke to this point in a 1910 paper in which, after pointing out the need to account for "discontinuity" in the oscillators, he said:

> But through this modification, will not the chief result of the prevailing electrodynamic theory be invalidated? [Planck was referring to Eq. (1).] … That by no means needs to be the case. For the quantities … which this equation contains are defined as time averages, extending over an enormously large number of oscillation periods. …
>
> Without committing myself too much to details, I may still note the following: in hydrodynamics and in the theory of elasticity, matter will almost always be

> assumed as arranged continuously in space, and no one finds in these considerations … a contradiction with the generally accepted atomic structure of bodies. … I am well aware that this analogy is by no means complete, but it can hardly be entirely dismissed.[88]

Planck went on to say that just as hydrodynamics and atomic theory are each valid in their separate realms, so his model for a resonator from the late 1890s, which assumed continuous energies, remained adequate for treating the *average* resonator energy, even though it could not describe the detailed microscopic behavior. As he put it, that Eq. (1) "is entirely independent of the damping constant σ of an oscillator [implies] … a general relation, independent of the details of the oscillation process." (As far as I know, this 1910 *Annalen* paper is the first occasion on which Planck referred explicitly in print to resonator quantization as a physical reality.)

Immediately preceding this passage, Planck noted that he had expressed a similar reservation regarding the resonators in § 109 of his 1906 *Lectures*, the same one he had cited in his 1908 letter to Lorentz (see footnote page 193). Moreover, in his 1909 Columbia Lectures, when everyone agrees that he was taking seriously the notion of quantized oscillators, Planck allowed Eq. (1) to stand without comment.[89] Planck's attitude here – that Maxwell's laws hold in one domain, but may require modification in another – seems entirely consistent with his viewpoint throughout this period.* For another example, see his 1908 letter to Lorentz, quoted above. It therefore seems unlikely that in 1900–1901, or in 1906, Planck would have been overly concerned with any apparent inconsistency between Eq. (1) and the implications of his discrete energy elements.

Planck's Combinatorial Starting Point

In 1900–1901, and again in both 1906 and 1909, Planck takes Eq. (7),

$$\frac{(N+P-1)!}{(N-1)!P!}, \tag{7}$$

as his combinatorial starting point, where N is the number of resonators and P is the number of energy elements. This expression, we recall, gives the *total* number of complexions, and stands in sharp contrast to Boltzmann's procedure: Boltzmann had picked out a *subset* of complexions corresponding to a given macroscopic state, and by a maximization procedure found the particular subset corresponding to the most probable (and hence, the equilibrium) state.

Historians have widely and plausibly assumed that Planck arrived at Eq. (7) by working backwards from his October 1900 extrapolation;[90] and as we have seen, he

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* Even when Planck finally did revise this derivation in the second edition of his *Lectures*, in 1913, he suggested that any "inconsistency was greatly reduced" by the sorts of considerations he gave in 1910. See Planck, *Vorlesungen* (2nd ed.) (ref. 48), p. 6 of the translation.

probably saw its significance both in standard textbook accounts and in Boltzmann's 1877 paper. But how did he justify its use? This question is interesting in its own right, and arguably has found a satisfactory answer only recently, with the work of Needell and Darrigol. Moreover, both Kuhn and Darrigol, for different reasons, think that Planck's use of Eq. (7) strengthens the argument that Planck was thinking exclusively in terms of continuous resonator energies both in 1900–1901, and in 1906.

We might begin by asking why Planck's method works at all. It turns out that for large N, the number of complexions corresponding to the equilibrium distribution is enormously larger than for any other state, and consequently, Eq. (5) – Boltzmann's subset – is to all intents and purposes equivalent to Eq. (7). Hence there is no need for Boltzmann's maximization procedure. Planck explained this point clearly in the second edition of his *Lectures*, published in 1913, where he said: "This number [of complexions corresponding to equilibrium] is … enormously large compared with the number of complexions of all states deviating from equilibrium," and added that: "The total number of all possible complexions may be calculated much more readily and directly than the number … referring to the state of equilibrium only."[91] It is tempting to suppose that Planck understood this point in December 1900, and it is at least possible that he did. He could have learned it from his reading of Boltzmann, and in the 1906 *Lectures*, he made the same point himself, though in a different context.[92]

There is, however, an important distinction that casts considerable doubt on any such interpretation. In 1913, Planck was considering a system of N resonators, and like Boltzmann, was seeking the subset of complexions corresponding to thermal equilibrium. In 1900–1901 and 1906, he was considering a *single* resonator, for which the disorder inherent in the rapid, random variations in amplitude and phase implied an entropy. Both Needell and Darrigol provide cogent discussions of the implications for Planck's use of Eq. (7), and Darrigol believes those implications argue against quantization. To introduce the argument, let us turn to Planck's 1906 *Lectures*. In § 147, he says, in laying the ground for his combinatorial starting point, that:

> For if the radiation field is stationary, then the physical state of the entire system is determined by the energy. … [Hence] the specification of the total energy U_N of the N resonators suffices for the determination of the state. … For the condition that the radiation field is stationary does not signify here a special case among many others, but on the contrary belongs with the necessary assumptions; otherwise one could no longer identify the quotient U_N/N, as we have done, with the time average of the energy of a single resonator.[93]

Planck continued this train of thought at the beginning of the next section, just before he introduced Eq. (7):

> We can proceed here in full analogy with gas molecules, if only we bear in mind that the given state of the resonator system does not, as there, admit of a single distribution law, but on the contrary a large number of different ones, since the number of resonators that possess a definite amount of energy (better: which fall

> in a definite "energy domain") is not prescribed, but on the contrary can vary. If we consider all possible kinds of energy distribution laws and calculate for each one the corresponding number of complexions, just as with gas molecules, then we obtain by addition of all of the so-obtained complexions,* the sought probability W of the given physical state.[94]

Needell and Darrigol both argue that Planck considered his combinatorial definition of entropy as a quantitative measure of the disorder that he found in the rapid random variation in the amplitude and phase of a *single* resonator.[95] Darrigol goes on to argue that under these circumstances, a state is characterized only by the total energy, and not by a particular distribution such as Boltzmann specifies in Eq. (5). These features stand out clearly in the passages quoted above. The key idea is that for a single resonator in equilibrium with a radiation field, all states of the resonator – not just a subset – are equilibrium states. Both analyses are insightful and informative, and have gone far to clarify our understanding of Planck's combinatorial starting point.

To understand this argument more clearly, consider the following distinction: In 1877, Boltzmann was considering a gas of N molecules with fixed total energy. Each complexion, then, represents a system of N molecules in a particular microstate. Thus, Eq. (5) represents the number of copies of the system of N molecules in a particular macroscopic state, and Eq. (7) the total number of copies for all possible states (an ensemble, in modern language).

By contrast, Planck was considering a *single* resonator, already in equilibrium with a radiation field, and using a collection of N copies of that single resonator to find its entropy and average energy. It is as if one were to apply Boltzmann's method, not to a system of N molecules, but to a single gas molecule in equilibrium with a heat bath. Over time, the speed of such a molecule ranges from very slow to very rapid, as it collides with other gas molecules. Its average energy is approximately equal to kT; but at any instant, it can have a *much* smaller or *much* larger energy, and still be in equilibrium.

Similarly, as one of Planck's resonators interacts with a radiation field, that resonator can take on very small or very large energies – such rapid, random fluctuations are at the heart of Planck's concept of natural radiation. To find the average quantities he needed to calculate the radiation density, Planck employed a conceptual collection of resonators with fixed total energy U_N. He used the number of possible complexions for that collection to find the entropy and the time-averaged energy of a single resonator that he assumed is always in equilibrium with the field. Hence, as Planck states in the passages quoted above, Boltzmann's subset does not apply; *all* complexions represent equilibrium states, and their number is given by Eq. (7). Thus, Planck is *not* treating an ensemble of systems of N resonators, all with fixed total energy, and seeking an equilibrium distribution, in

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* In other words, the number of complexions for a single "energy distribution," Eq. (5), summed over all possible distributions, yields the total number of complexions, given by Eqs. (6) and (7).

analogy to Boltzmann's treatment of a gas. He is considering a very different problem, for which Eq. (7) is indeed the appropriate starting point.*

Planck's method is surprising to a modern reader, for whom Boltzmann's 1877 theory is not a natural one to apply to a single molecule or resonator. Today, for such a system, more direct methods are available (for physicists, Gibbs's canonical ensemble readily comes to mind). Indeed, Boltzmann had already used such a method in 1871, deriving the Maxwell-Boltzmann distribution for a gas by considering a *single* gas molecule in equilibrium with a reservoir. Einstein used a similar scheme in 1902.[96] Planck did not have this method available to him, however, and so – at least in 1906 – seems to have been applying a scheme that Boltzmann had developed for a system of N molecules to a system consisting of a single resonator.

Planck was surely thinking along these lines in 1906. Was he thinking along these same lines in December 1900 and January 1901? It is entirely possible, and even likely. He does say in December 1900 that his assumption, that "the probability of any state is proportional to the number of corresponding complexions,"

> … can also be understood as a more detailed definition of the hypothesis of natural radiation … . This hypothesis I have expressed before only in the form that the energy of the radiation is completely "randomly" distributed over the various partial vibrations present in the radiation.[97]

It is less likely, but also possible that he understood that Eq. (5) (Boltzmann's subset) and Eq. (7) give essentially the same number of complexions, as noted above. It is even possible that Planck had found Eq. (7) only by reverse reasoning, did not fully understand its basis, but thought it intuitively plausible and knew that it led to a correct result. One can outline the possibilities, but it is well to be cautious in assuming that any one of them must be correct.

Darrigol argues that Planck's choice of Eq. (7) as a starting point makes it more likely that he "could not possibly have understood the introduction of energy elements as a discrete selection of the admissible energy values of a resonator." His argument goes as follows: Planck had good reasons for his choice of Eq. (7). He could readily interpret that equation in terms of "energy elements," but found it inconvenient (for reasons that are not clearly stated) to do so in terms of energy cells. Consequently Planck used energy elements for convenience, even though he had cells in mind.[98] But as we have seen, Planck never said anything along these lines; and indeed, as I have argued above, the introduction of cells, whether in energy space or phase space, and whether using Planck's formalism or Boltzmann's, would not have been a trivial undertaking for Planck either in 1900–1901 or in 1906.

Kuhn argues for the same conclusion, but from an entirely different point of view. He argues that Planck's December 1900 sketch (which, we recall, envisions picking a *subset* of complexions, and finding the most probable state) "provided the

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* In using the total number of complexions, of course, Planck was not really calculating a probability at all; what he called a probability was in fact, as he also says, a quantitative measure of the disorder inherent in the random fluctuations in the amplitude and phase of resonator oscillations. Note also that Planck's "absolute" view of the second law is consistent with his use of Boltzmann's technique, since none of Planck's complexions correspond to non-equilibrium states. From a modern perspective, Planck was – without having the conceptual framework to express it – setting the entropy proportional to the logarithm of the number of accessible microscopic states, much as one might do today.

conceptual basis for his alternate proof" of January 1901. At one point, he even refers to the latter as a "drastic shortcut."[99]

It must be said that Kuhn's reconstruction was the first to take seriously the differences between Planck's December 1900 and January 1901 derivations, and to suggest how Planck might have justified his combinatorial starting point. Kuhn also understood that the 1901 derivation assumes that *all* complexions represent the equilibrium state (and therefore are counted by Eq. (7)), although he offers no explanation other than the one given above.[100] There are, however, serious difficulties with Kuhn's argument: First, Planck never again referred to his December 1900 sketch, and indeed, at the beginning of his 1901 paper, may even have suggested that any such approach is unnecessary (see above). In January 1901, and again in 1906 and 1909, Planck started with Eq. (7) even when he considered only a collection of resonators all at the same frequency, and never so much as hinted at anything resembling Kuhn's reconstruction. Moreover, the two derivations appear to be conceptually distinct: The December 1900 sketch, as we have seen, assumes finding a maximum subset; the full derivation of January 1901 emphatically does not. It therefore seems to me that the two approaches are in fact independent, having in common only the same combinatorial starting point – one that Planck probably reached by reverse reasoning in any case. Kuhn also argues, particularly in his 1984 retrospective, that his interpretation is another argument against quantization; but in fact that argument seems to rely on his insistence that Planck really had energy cells in mind, instead of energy elements.[101]

Other Arguments

Darrigol makes two additional arguments in support of his thesis that Planck could have been thinking only in terms of continuous resonator energies in 1900–1901. First, he points out that in introducing the energy elements in December 1900, Planck says: "If the ratio $[E/\varepsilon]$ thus calculated is not an integer, we take for P an integer in the neighborhood." Darrigol argues that this statement "by itself shows that … the energy of a single resonator … was not thought to be restricted to multiples of ε."[102] Planck's words certainly reflect the provisional, and perhaps only formal significance that he attached to his energy elements – he is not giving a physical argument or anything approaching one. But might not his words also suggest that he understood the possible implications, however little physical significance he attached to them? Perhaps more to the point, Planck seems to have had second thoughts about this caveat – he never again repeated it, not even in his January 1901 *Annalen* paper, only a month later.

To understand Darrigol's second argument, recall that in both December 1900 and January 1901, Planck had commented on his assumption that all complexions are equally probable; in the January 1901 *Annalen* paper, he said that:

> Whether this assumption really proves to be true in nature can in the end only be verified through experience. But conversely, if experience should decide in its favor, from the validity of this hypothesis, it may be possible to draw wider

> conclusions about the special nature of resonator oscillations, namely about the character of the "indifferent and comparably sized primary domains [*Spielräume*]," in the expression of J. v. Kries.[103]

Darrigol suggests that the reference to Kries implies that Planck could not have been thinking about discontinuous quantities, since "by *Spielräume*, Kries meant domains of equiprobability for probability distributions of *continuous* variables."[104] It is clear from this passage that, as we have already seen, Planck was thinking about his assumption of equally probable complexions; whether more far-reaching conclusions can be drawn from Planck's brief reference to Kries is, perhaps, less certain.

Conclusion

Over the last forty years, historical studies of Planck's contributions to black-body radiation have resulted in a substantial measure of agreement – an agreement that is easily obscured by the continuing debate over the quantum. The importance of this work for our understanding of Planck is, it seems to me, independent of the authors' views on this still-controversial topic. We have a good understanding of Planck's perspective on the second law and how he used it, his concept of natural radiation, and eventually Boltzmann's methods to complete his description of black-body radiation. In the process, he increasingly came to realize that he could not explain the behavior of his resonators in terms of Maxwell's laws and Hamiltonian mechanics. I find it hard to argue with Needell's contention that this progression – in all likelihood a gradual one – is more important to our understanding of Planck than the effort to pin down exactly what Planck thought about his energy elements in 1900.

What did Planck do in 1900? Certainly he distributed finite energy elements among resonators; and those energy elements were, in his words, the "most essential point" of a derivation that resulted not only in good agreement with experiment, but in two new "universal constants" to which he attached the greatest importance. It is apparent that in 1906, and quite possibly in 1900–1901, he strongly suspected that he could not explain his new constant h on the basis of established physical law. In his 1906 *Lectures* he said as much, pointing out repeatedly the need for a physical interpretation, and hinting in § 109 at the need for "another oscillation law that is even better adapted to the processes in nature." In December 1900 and January 1901, he likewise said nothing about the physical significance of either his energy elements or h, in sharp and almost pointed contrast to his detailed discussion of k. Did his silence imply, as one might infer from his 1931 letter to Robert W. Wood, that initially he did not think too much about the implications of the energy elements, or thought them only a mathematical formality, albeit one that was closely tied to his new constant h? Or had he already begun to wonder about their possible physical implications, but thought it imprudent even to speculate about them? We don't know. We know only that he was silent, and at least in print, maintained that silence for nearly a decade.

In any event, the specific arguments that suggest that Planck must have been thinking only in terms of continuous resonator energies until well after 1906 do not hold up under close examination. The most robust of these arguments is that he was really thinking in terms of energy cells, not energy elements. And even that argument seems unpersuasive to me, as I have argued in detail above. Planck speaks only of energy elements in 1900–1901. His retrospective accounts of his January 1901 derivation also speak unequivocally of energy elements distributed over resonators; he never so much as hints that he had something else in mind.

Moreover, it is risky to infer Planck's thinking in 1900–1901 from what he says in his 1906 *Lectures*. Between 1900 and 1906, he had learned a good deal of statistical mechanics, and had had a good deal of time to reflect on his derivation. It does not seem plausible that his outlook did not evolve during those years, particularly in view of his new finite cells in phase space. And in 1906, what was that outlook? In 1906 Planck did indeed allude briefly and approvingly to the possibility of continuous energies. But they play no role whatever in his derivation, which was still unambiguously in terms of energy elements. He repeatedly emphasized the lack of, and need for, a physical interpretation. In his correspondence from 1905 on – in which he was much more forthcoming than in print – he recognized the possible significance of his new constant h, and he balked only at the idea of a quantized electromagnetic field. Thus, even in 1906, Planck had by no means settled on an unequivocal scheme involving phase-space cells and continuous energies, as he did in the second edition of his *Lectures*, in 1913.

What did Planck *really* think about these energy elements, in 1900–1901 or for that matter, in 1906? As I have repeatedly suggested, he does not say. Perhaps Einstein, as in so many other ways, got it right in 1906 when in a review of Planck's *Lectures* in the *Beiblätter* to the *Annalen der Physik*, he wrote:

> The author repeatedly points to the necessity of introducing this universal constant h and emphasizes the importance of a physical interpretation (not given in the book) of the latter.[105]

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References

1 For this early history, see Stephen G. Brush, *The Kind of Motion We Call Heat*, 2 Vols. (New York: North Holland, 1976).

2 Hans Kangro, *Vorgeschichte des Plankschen Strahlungsgesetzes* (Wiesbaden: Steiner, 1970), transl. R. E. W. Maddison, *Early History of Planck's Radiation Law* (London: Taylor and Francis, 1976). This book can be hard to find, but it contains an outstanding description of experimental developments, as well as a detailed account of Planck's work from 1894 on, and a valuable bibliography.

3 For a recent discussion of such issues, see Roger H. Stuewer, "History and Physics," *Science & Education* **7** (1998), 13–30, and *History as Myth and Muse* (Amsterdam: University of Amsterdam Press, 1998). See also Stephen G. Brush, "Should the History of Science Be Rated X?" *Science* **18** (1974), 1164–1172, and Martin J. Klein, "The use and abuse of historical teaching in physics," in Stephen G. Brush and Allen L. King, eds., *History in the Teaching of Physics* (Hanover, NH: University of New England Press, 1972), pp. 12–27.

4 Martin J. Klein has written on this topic in a series of publications: "Max Planck and the Beginnings of Quantum Theory," *Archive for History of Exact Sciences* **1** (1962), 459–479; "Planck, Entropy, and Quanta, 1901–1906," *Natural Philosopher* **1** (1963), 83–108; "Einstein's First Paper on Quanta," *Natural Philosopher* **2** (1963), 59–86; "Thermodynamics and Quanta in Planck's Work," *Physics Today* **19** (November 1966), 23–32, reprinted in Spencer R. Weart and Melba Phillips, eds., *History of Physics: Readings from Physics Today*, Number Two (New York: American Institute of Physics, 1985), pp. 294–302; *Paul Ehrenfest: Vol. 1, The Making of a Theoretical Physicist* (New York: North Holland, 1970); and "The Beginnings of Quantum Theory," in C. Weiner, ed., *History of Twentieth Century Physics* (New York: Academic Press, 1977), pp. 1–39.

5 Thomas S. Kuhn, *Black-Body Theory and the Quantum Discontinuity, 1894–1912* (Oxford: Oxford University Press, 1978). Kuhn's bibliography is an indispensable reference. In addition, see Thomas S. Kuhn, "Revisiting Planck," *Historical Studies in the Physical Sciences* **14** (1984), 231–252. This work is included as an appendix in the 1987 paperback printing of Kuhn's book.

6 Kuhn, *Quantum Discontinuity* (ref. 5), p. 126.

7 *Ibid*., ch. V–VII. For earlier discussions of Einstein and Ehrenfest, see Klein, "Einstein's First Paper" and Klein, *Ehrenfest*, ch. 10 (ref. 4).

8 Olivier Darrigol, "Statistics and combinatorics in early quantum theory," *Historical Studies in the Physical Sciences* **19** (1988), 17–80; *From c-Numbers to q-Numbers* (Berkeley: University of California Press, 1992); and "The Historians' Disagreement over the Meaning of Planck's Quantum," Preprint 150 on the web site of the Max Planck Institute for the History of Science, http://www.mpiwg-berlin.mpg.de and *Centaurus* **43** (2001), 219–239. One can disagree with some of the conclusions; it is in this last work, it is nevertheless a thorough and thoughtful summary.

9 Allan A. Needell, "Irreversibility and the Failure of Classical Dynamics: Max Planck's Work on the Quantum Theory, 1900–1915," Ph.D. dissertation, Yale University, 1980. Needell's dissertation was never published, apart from a comparatively short Introduction to a recent printing of the 1906 and 1913 editions of Planck's *Lectures on the Theory of Heat Radiation* (ref. 48). It is nevertheless an essential reference for historians interested in Planck's work on the quantum. See also Needell's review of Kuhn, *Quantum Discontinuity* (ref. 5), in *Isis* **78** (1987), 604–605.

10 Other treatments of this history, apart from the works already cited, include Max Jammer, *The Conceptual Development of Quantum Mechanics* (New York: McGraw-Hill, 1966), chapter 1; Armin Hermann, *Frühgeschiche der Quantentheorie (1899–1913)* (Mosbach/Baden: Physik Verlag, 1969), transl. Claude W. Nash, *The Genesis of Quantum Theory (1899–1913)* (Cambridge, MA: MIT Press, 1971), ch. 1–3; Friedrich Hund, *Geschichte der Quantentheorie* (Manheim: Bibliographisches Institut, 1967); transl. Gordon Reece, *The History of Quantum Theory* (New York: Harper and Row, 1974); Elizabeth Garber, "Some Reactions to Planck's Law, 1900–1914," *Studies in the History and Philosophy of Science* **7** (1976), 89–126; and Jagdish Mehra and Helmut Rechenberg, *The Historical Development of Quantum Mechanics*, Vol. 1 (2 parts) (New York: Springer-Verlag, 1982), part 1, ch. 1. A good general biography of Planck is J. L. Heilbron, *The*

Dilemmas of an Upright Man: Max Planck as Spokesman for German Science (Berkeley: University of California Press, 1986). See also Hans Kangro, "Planck," *Dictionary of Scientific Biography* (New York: Scribner, 1970). Three books that place more emphasis on the physics, but from a historical perspective, are Martin Bailyn, *A Survey of Thermodynamics* (New York: American Institute of Physics, 1994); M. S. Longair, *Theoretical Concepts in Physics* (Cambridge and New York: Cambridge University Press, 1984), ch. 6–12; and Philip Stehle, *Order*, *Chaos*, *Order* (New York: Oxford University Press, 1994). Two useful reviews of the controversy are Martin J. Klein, Abner Shimony, and Trevor J. Pinch, "Paradigm Lost? A Review Symposium," *Isis* **70** (1979), 429–440; and Peter Galison, "Kuhn and the Quantum Controversy," *British Journal for the Philosophy of Science* **32** (1981), 71–84. My treatment relies heavily on these works, including those of Klein, Kuhn, Darrigol, and Needell previously cited.

11 See for example the very different treatments in Bernard H. Lavenda, *Statistical Physics* (New York: Wiley, 1991), ch. 2; Ralph Baierlein, *Thermal Physics* (Cambridge and New York: Cambridge University Press, 1999), p. 21; and George D. J. Phillies, *Elementary Lectures in Statistical Mechanics* (New York: Springer-Verlag, 2000), pp. 189, 202–205.

12 Planck's work in physical chemistry has not been extensively studied; but see Robert Scott Root-Bernstein, "The Ionists: Founding Physical Chemistry, 1872–1890," Ph.D. dissertation, Princeton University, 1980. For additional insight into the uneven reception of Planck's work among physical chemists, see Helge Kragh and Stephen J. Weininger, "Sooner silence than confusion: The tortuous entry of entropy into chemistry," *Historical Studies in the Physical and Biological Sciences* **27** (1996), 91–130.

13 Max Planck, *Grundriss der allgemeinen Thermochemie*. *Mit einem Anhang: Der Kern des zweiten Hauptsatzes der Wärmetheorie* (Breslau: E. Trewendt, 1893); also published in A. Ladenburg, ed., *Handwörterbuch der Chemie*, Elfter Band (Breslau: E. Trewendt, 1893), pp. 566–646.

14 Martin J. Klein first called attention to this aspect of Planck's work; see Klein, "Planck, Entropy, and Quanta" (ref. 4), pp. 97–99. For more recent discussions that present conclusive and detailed evidence, see Needell, "Irreversibility" (ref. 9), and "Introduction" (ref. 48), and all three works by Darrigol (ref. 8).

15 See for example the introduction to Max Planck, *Vorlesungen über Thermodynamik* (Leipzig: Von Veit, 1897), transl. Alexander Ogg, *Treatise on Thermodynamics*, 7th edition (London: Longmans, Green, 1927).

16 Gustav Kirchhoff, *Vorlesungen über die Theorie der Wärme*, ed. Max Planck (Leipzig: Teubner, 1894).

17 Kuhn, *Quantum Discontinuity* (ref. 5), pp. 61–63.

18 For full bibliography and accounts of this early history, see Kangro, *Radiation Law* (ref. 2); David Cahan, *An Institute for an Empire: The Physikalisch-Technische Reichsanstalt*, *1891–1918* (Cambridge and New York: Cambridge University Press, 1989), pp. 136–157, esp. p. 145; and Dieter Hoffmann, "On the Experimental Context of Planck's Foundation of Quantum Theory," Preprint 150 on the web site of the Max Planck Institute for the History of Science, http://www.mpiwg-berlin.mpg.de. See also Kuhn, *Quantum Discontinuity* (ref. 5), and Klein, "Max Planck" (ref. 4).

19 Max Planck, *Wissenschaftliche Selbstbiographie* (Leipzig: Barth, 1949), reprinted in Planck, *Physikalische Abhandlungen und Vorträge*, 3 Vols. (Braunschweig/Wiesbaden: Vieweg, 1958), Vol. 3, pp. 374–401 (hereafter cited as *PAV*), transl. Frank Gaynor, *Scientific Autobiography and Other Papers* (New York: Philosophical Library, 1949), p. 40 of the translation.

20 Planck, *PAV* (ref. 19), pp. 445–458, 466–492. Here and in the following note, see Kuhn, *Quantum Discontinuity* (ref. 5), Kangro, *Radiation Law* (ref. 2), Klein, *Ehrenfest* (ref. 4), and Darrigol, "Statistics" and *c-Numbers* (ref. 8) for full citations and discussion.

21 *Ibid*., pp. 493–600. Kangro, *Radiation Law* (ref. 2), gives an especially helpful discussion of the development of Planck's views during these years.

22 See for example Arnold Sommerfeld, *Vorlesung über Theoretische Physik*, Vol. 5 (Leipzig: Geest & Portig, 1952), transl. J. Kestin, *Thermodynamics and Statistical Mechanics* (New York: Academic Press, 1956). For Boltzmann's H-theorem and "molecular disorder," see Carlo Cercignani, *Ludwig Boltzmann*, *The Man Who Trusted Atoms* (Oxford: Oxford University Press, 1998); Martin J. Klein, "The Development of Boltzmann's Statistical Ideas," in E. D. G. Cohen and W. Thirring, eds., *The Boltzmann Equation: Theory and Applications*, Acta Physica Austraica Suppl.

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23 Max Planck, "Über irreversible Strahlungsvorgänge," *Annalen der Physik* **1** (1900), 69–122, reprinted in *PAV* (ref. 19), Vol. 1, pp. 614–667.

24 Needell, "Irreversibility" (ref. 9), was the first to set forth detailed arguments for Planck's interpretation of the second law, and his location of disorder in his resonators. Darrigol, in both "Statistics" and *c-Numbers* (ref. 8), developed many of these arguments independently, from a different perspective. See also ref. 14.

25 See the concluding paragraphs of Planck's 1899 address to the German Mathematical Union, in *PAV* (ref. 19), Vol. 1, pp. 610–613. For discussion, see Needell, "Irreversibility" (ref. 9), pp. 21–23, 38; on the more general issues, see also Needell, "Introduction" (ref. 48), and Darrigol, "Statistics" and *c-Numbers* (ref. 8).

26 Wilhelm Wien, "Über die Energievertheilung im Emissionsspectrum eines schwarzen Körpers," *Ann. Phys.* **58** (1896), 662–669. See also Kangro, *Radiation Law* (ref. 2).

27 Klein, "Max Planck" (ref. 4), p. 463. For the "hint," see Planck, "Strahlungsvorgänge" (ref. 23), p. 118, and *PAV* (ref. 19), Vol. 1, p. 663.

28 Max Planck, "Entropie und Temperatur strahlender Wärme," *Ann. Phys.* **1** (1900), 719–737, reprinted in *PAV* (ref. 19), Vol. 1, pp. 668–686.

29 Stehle, *Order* (ref. 10), pp. 112–114, and Kuhn, *Quantum Discontinuity* (ref. 5), pp. 95–97. See also Planck's later account in § 189 of his 1906 *Lectures* (ref. 48, below).

30 Planck, "Entropie und Temperatur" (ref. 28), p. 725; and *PAV* (ref. 19), Vol. 1, pp. 673–674.

31 See for example Kangro, *Radiation Law* (ref. 2), § 8.7, pp. 194–198 of the translation.

32 Max Planck, "Über eine Verbesserung der Wien'schen Spectralgleichung," *Verhandlungen der Deutschen Physikalischen Gesellschaft* **2** (1900), 202–204, reprinted in *PAV* (ref. 19), Vol. 1, pp. 687–689, transl. D. der Haar and Stephen G. Brush, *Planck's Original Papers in Quantum Physics* (German and English Edition), in Hans Kangro, ed. (London: Taylor and Francis, 1972).

33 Planck did not give this explanation in October 1900, but did note in § 189 of his 1906 *Lectures* (ref. 48, below) that he had originally found his new result in this way.

34 The phrase appears in Planck's Nobel Lecture: Max Planck, *Die Entstehung und bisherige Entwicklung der Quantentheorie* (Leipzig: Barth, 1920), reprinted in *PAV* (ref. 19), Vol. 3, pp. 121–136, transl. R. Jones and D. H. Williams, *A Survey of Physics* (London: Methuen, 1924, reprinted New York: Dover, 1993), p. 106 of the Dover translation.

35 Full citations for both letters, and an extensive excerpt from the first, are given in Christa Jungnickel and Russell McCormmach, *Intellectual Mastery of Nature*, 2 Vols. (Chicago: University of Chicago Press, 1986), Vol. 2, p. 262. This section (pp. 254–268) contains another good account of the close interaction between Planck and the experimentalists, and contemporary graphs comparing experimental results with the various theories.

36 Cercignani, *Boltzmann* (ref. 22); Klein, "Development" (ref. 22) and *Ehrenfest* (ref. 4), ch. 6; Kuhn, *Quantum Discontinuity* (ref. 5), ch. 2; and Giovanni Gallavotti, "Ergodicity, Ensembles, Irreversibility in Boltzmann and Beyond," *Journal of Statistical Physics* **78** (1995), 1571–1589, reprinted in Giovanni Gallavotti, *Statistical Mechanics* (New York: Springer-Verlag, 2000), pp. 36–44.

37 Ludwig Boltzmann, "Über die Beziehung zwischen dem zweiten Hauptsatze der mechanischen Wärmetheorie und der Wahrscheinlichkeitsrechnung respektive den Sätzen über das Wärmegleichgewicht," *Wiener Berichte* **76** (1877), 373–435, reprinted in *Wissenschaftlich Abhandlungen*, 3 Vols. (Leipzig: Barth, 1909), Vol. 2, pp. 164–223.

38 *Ibid.*, Vol. 2, p. 168. I use Boltzmann's notation in this section.

39 *Ibid.*, Vol. 2, p. 170.

40 For fuller discussions of Boltzmann's approach, see Cercignani, *Boltzmann* (ref. 22); Klein, "Development" (ref. 22); Klein, *Ehrenfest* (ref. 4), pp. 105–108; Kuhn, *Quantum Discontinuity* (ref. 5), ch. 2; and Darrigol, *c-Numbers* (ref. 8), pp. 62–66.

41 Kirchhoff, *Wärme* (ref. 16), pp. 137–149.

42 Ludwig Boltzmann, *Vorlesungen über Gastheorie*, 2 Vols. (Leipzig: Barth, 1896–1898), reprinted in Roman U. Sexl, ed., *Gesamtausgabe*, Band 1 (Braunschweig/Wiesbaden: Vieweg, 1981), transl.

Stephen G. Brush, *Lectures on Gas Theory* (Berkeley: University of California Press, 1964); reprinted (New York: Dover 1995). Boltzmann gives a brief discussion of and citation to his 1877 paper in Vol. 1, § 6. Planck cited this volume in Planck, "Strahlungsvorgänge" (ref. 23), p. 75, *PAV* (ref. 19), Vol. 1, p. 620.

43 Max Planck, "Zur Theorie des Gesetzes der Energieverteilung im Normalspectrum," *Verhandlungen der Deutschen Physikalischen Gesellschaft* **2** (1900), 237–245, reprinted in *PAV* (ref. 19), Vol. 1, pp. 698–706, transl. in Kangro, ed., *Original Papers* (ref. 32). My quotations from this paper follow the translation.

44 Max Planck, "Über das Gesetz der Energieverteilung im Normalspectrum," *Ann. Phys.* **4** (1901), 553–563, reprinted in *PAV* (ref. 19), Vol. 1, pp. 717–727; and "Über die Elementarquanta der Materie und der Electricität," *Ann. Phys.* **4** (1901), 564–566, reprinted in *PAV* Vol. 1, pp. 728–730.

45 Max Planck, "Über die Verteilung der Energie zwischen Aether und Materie," *Ann. Phys.* **4** (1902), 629–641, reprinted in *PAV* (ref. 19), Vol. 1, pp. 731–743. This paper was first published in a 1901 Festschrift for Johannes Bosscha; see Kuhn, *Quantum Discontinuity* (ref. 5), p. 342, for a full citation.

46 Planck, "Strahlungsvorgänge" (ref. 23), p. 115, *PAV* (ref. 19), Vol. 1, p. 660; and Planck, "Normalspectrum" (ref. 44), p. 554, *PAV* Vol. 1, p. 718. Darrigol makes a similar point, though without quoting Planck – that Planck's December 1900 sketch, if carried out, would only imply "that the resonators at various frequencies have the same temperature." See Darrigol, *c-Numbers* (ref. 8), p. 69, note 94.

47 Max Planck, "Über irreversible Strahlungsvorgänge. Nachtrag," *Ann. Phys.* **6** (1901), 818–831, reprinted in *PAV* (ref. 19), Vol. 1, pp. 744–757. This paper is primarily devoted to showing that the combined entropy of an oscillator and the field must always increase, much as he had done for the older entropy expression from which Wien's law resulted. For his 1899 and 1900 discussions of his natural units in the context of Wien's law, see refs. 21 and 23. Planck returned to his system of natural units again in his 1906 *Lectures* (Planck, *Vorlesungen*, ref. 48), § 159. See Klein, "Thermodynamics and Quanta" (ref. 4), pp. 297–298 of the AIP reprint; Kangro (ref. 2), pp. 142–147; and Kuhn, *Quantum Discontinuity* (ref. 5), pp. 110–113.

48 Max Planck, *Vorlesungen über die Theorie der Wärmestrahlung* (Leipzig: Barth, 1906); second edition (Leipzig: Barth, 1913). The second edition is also available in translation: *The Theory of Heat Radiation*, transl. Morton Masius (New York: Dover, 1959 and 1991). Both the first edition (1906) in the original German and the translation of the second edition (1913) are reprinted in Max Planck, *The Theory of Heat Radiation* (New York: American Institute of Physics, 1988), with an Introduction by Allan A. Needell.

49 Max Planck, *Acht Vorlesungen über theoretische Physik* (Leipzig: Hirzel, 1910), transl. A. P. Wills, *Eight Lectures on Theoretical Physics* (New York: Columbia University Press, 1915), reissued with an introduction by Peter Pesic (New York: Dover, 1998). These lectures were delivered at Columbia University in the spring of 1909, and I usually refer to them in the text as the Columbia Lectures.

50 Planck, "Verteilung" (ref. 45).

51 Lord Rayleigh, "Remarks upon the Law of Complete Radiation," *Philosophical Magazine* **49** (1900), 539–40. This short paper is reproduced in Longair, *Concepts* (ref. 10), pp. 208–209.

52 Kirchhoff, *Wärme* (ref. 16), pp. 152, 166–172.

53 For Drude's use of equipartition, and reasons that might have led Planck to be suspicious of it, see Walter Kaiser, "Electron Gas Theory of Metals," *Historical Studies in the Physical and Biological Sciences* **17** (1987), 271–297.

54 Max Planck, "Zur Theorie der Wärmestrahlung," *Ann. Phys.* **31** (1910), 758–768, p. 766; reprinted in *PAV* (ref. 19), Vol. 2, pp. 237–247, p. 245.

55 James Jeans, "A Comparison between Two Theories of Radiation," *Nature* **72** (1905), 293–294. Jeans gave several other more detailed treatments of equipartition in 1905 to which Planck might be referring – see for example the bibliography in Kuhn, *Quantum Discontinuity* (ref. 5). Planck's reference might even conceivably be to a 1909 paper that Planck cited in 1910 in a different connection, James Jeans, "The Motion of Electrons in Solids, Part II," *Philosophical Magazine* **18** (1909), 209–226; see Klein, *Ehrenfest* (ref. 4), p. 236–237, and Darrigol, "Statistics" (ref. 8), p. 63.

Jeans's 1909 paper also gives a proof of equipartition, but does not mention the new constant h; moreover, Planck had clearly been aware of this point in his 1906 *Lectures*.

56 Klein, "Max Planck," pp. 464–468, and Klein, *Ehrenfest* (ref. 4), pp. 234–237; see also pp. 108–112 for another discussion of the problems with equipartition. For Planck's own viewpoint a few years later, see Planck, *Eight Lectures* (ref. 49), at the end of the fourth lecture.

57 Kuhn, *Quantum Discontinuity* (ref. 5) gives an extensive discussion of this correspondence, especially in chs. 5, 6, and 8. Needell, "Irreversibility" (ref. 9), gives another detailed discussion, more in line with the interpretation given here; see esp. ch. 3.

58 Kuhn, *Quantum Discontinuity* (ref. 5), pp. 132, 288, gives a translation (which I have followed) and the German original of the entire letter. Kuhn, of course, offers his own interpretation of it. See also the discussion in Needell, "Irreversibility" (ref. 9), p. 90. The original is available in the microfilmed Archive for the History of Quantum Physics, at the Niels Bohr Library, AIP Center for the History of Physics, College Park, Maryland, the University of Minnesota, Minneapolis, and elsewhere.

59 See Needell, "Irreversibility" (ref. 9), pp. 90–92, for a discussion of this letter, and the German original. I have followed his translation. This letter is also discussed briefly in Kuhn, *Quantum Discontinuity* (ref. 5), pp. 201, 305–306, and in Kangro, *Radiation Law* (ref. 2), pp. 220–221 of the translation. The original is available in the microfilmed Archive for the History of Quantum Physics (ref. 58); see Kuhn, *Quantum Discontinuity* (ref. 5), p. 306, for a full citation.

60 *The Collected Papers of Albert Einstein* (Princeton, NJ: Princeton University Press, 1987–98), Vol. 5, Martin J. Klein, A. J. Kox, and Robert Schulman, eds., Document 47 (hereafter abbreviated as *CP*). I have followed the translation in the accompanying translation volume.

61 Max Planck, "Zur Dynamik bewegter Systeme," *Ann. Phys.* **26** (1908), 1–24; reprinted in Planck, *PAV* (ref. 19), Vol. 2, pp. 176–209. See Needell, "Irreversibility" (ref. 9), pp. 87–89 for a discussion of this paper, which was first presented to the Prussian Academy in June, 1907. I have followed his translation.

62 This letter is fully cited, and parts of it are quoted both in translation and in the original German in Kuhn, *Quantum Discontinuity* (ref. 5), pp. 197–198, 304. I have followed his translation. Needell, "Irreversibility" (ref. 9), pp. 93–108, gives a translation of a more extended though not fully overlapping portion of this letter, and argues, I think cogently, that contrary to Kuhn, this letter does not represent a sharp break in Planck's thought.

63 Letter from Max Planck to Robert W. Wood, Archive for the History of Quantum Physics, Microfilm 66, 5 (ref. 58). A translation of the entire letter, which I follow here, may be found in Hermann, *Genesis* (ref. 10), p. 23 of the translation; and also in Longair, *Concepts* (ref. 10), p. 222.

64 See Planck's Nobel address (ref. 34), pp. 108–109 of the translation; his *Scientific Autobiography* (ref. 19), pp. 43–45 of the translation; and a 1943 article written for the benefit of "physicists of a later generation": Max Planck, "Zur Geschichte der Auffindung des physikalischen Wirkungsquantums," *Naturwissenschaften* **31** (1943), 153–159, reprinted in *PAV* (ref. 19), Vol. 3, pp. 255–267, p. 267.

65 Needell, "Introduction" (ref. 48), p. xix; see also Needell, "Irreversibility" (ref. 9), p. 151.

66 Kuhn, *Quantum Discontinuity* (ref. 5), pp. 116–120; Kuhn, "Revisiting" (ref. 5), pp. 236–238; Darrigol, "Statistics" (ref. 8), pp. 55–56; and Darrigol, "Meaning" (ref. 8), p. 15.

67 Kuhn, *Quantum Discontinuity* (ref. 5), p. 128.

68 *Ibid*.

69 Darrigol, "Statistics" (ref. 8) p. 55.

70 Letter of Planck to Walther Nernst, included in the Archive for the History of Quantum Physics (ref. 58), Microfilm 58. This letter is mentioned in Kuhn, *Quantum Discontinuity* (ref. 5), pp. 134, 289.

71 See Klein, "Planck, Entropy, and Quanta" (ref. 4), esp. pp. 99–104, for an early discussion of some of the points made below.

72 Josiah Willard Gibbs, *Elementary Principles in Statistical Mechanics* (New York: Scribner's, 1902), transl. Ernst Zermelo (Planck's former student), *Elementare Grundlagen der statistiche Mechanik* (Leipzig: Barth, 1905). Planck's reviews appear in *Beiblätter zu den Annalen der Physik* **27** (1903), 748–753, and "Über die mechanische Bedeutung der Temperatur und der Entropie," in *Festschrift Ludwig Boltzmann* (Leipzig: Barth, 1904), 113–122, reprinted in Planck, *PAV* (ref. 19), Vol. 2, pp. 79–88. See Needell, "Irreversibility" (ref. 9), ch. 2, for a discussion of these reviews.

73 Planck, *Vorlesungen* (1st ed.) (ref. 48), p. 151.
74 *Ibid*., p. 154.
75 *Ibid*., p. 155.
76 *Ibid*., p. 156.
77 *Ibid*., p. 154. See also Kuhn, *Quantum Discontinuity* (ref. 5), pp. 120–123.
78 *Ibid*., pp. 178–179.
79 See Sommerfeld, *Statistical Mechanics* (ref. 22), Chapter 4, for a good discussion of the physics.
80 Planck, *Vorlesungen* (1st ed.) (ref. 48), p. 179.
81 Kuhn, *Quantum Discontinuity*, pp. 60, 134, and Kuhn, "Revisiting" (ref. 5), p. 233.
82 Planck, *Vorlesungen* (2nd ed.) (ref. 48), §§ 125–127 and 135–143, esp. § 142. For a discussion, see the last two chapters of Needell, "Irreversibility" (ref. 9), esp. pp. 238–249, and the much briefer discussions in Needell, "Introduction" (ref. 48); and Kuhn, *Quantum Discontinuity* (ref. 5), ch. 10. For another brief discussion, and a sketch of what Planck might have done if he *had* made central use of energy cells in 1906, see Res Jost, *Das Märchen vom Elfenbeinernen Turm* (Berlin: Springer-Verlag, 1995), pp. 75–77.
83 See, for example, Kuhn, *Quantum Discontinuity* (ref. 5), pp. 128–130; Darrigol, "Statistics," p. 55, and Darrigol, "Meaning," p. 15 (ref. 8).
84 Planck, *Eight Lectures* (ref. 49), Sixth Lecture, pp. 94–95 of the Dover edition of the translation, which I follow here.
85 The first is a 1911 paper where Planck introduced his new theory, in which resonator energies are continuous: Max Planck, "Eine neue Strahlungshypothese," *Verh. der Deutsch. Phys. Ges.* **13** (1911), 138–148, reprinted in Planck, *PAV* (ref. 19), Vol. 2, pp. 249–259; see also the discussion in Needell, "Irreversibility" (ref. 9), ch. 4. The second is Planck's 1943 *Naturwissenschaften* article written for the benefit of "physicists of a later generation" (ref. 64).
86 Kuhn, *Quantum Discontinuity* (ref. 5), p. 118; Kuhn, "Revisiting" (ref. 5), p. 236. Darrigol formerly took this position; see "Statistics," p. 55, and *c-Numbers*, pp. 72–73, but has recently abandoned it; see "Meaning," pp. 17–18 (ref. 8), based on Needell's and his own analysis of Planck's distinction between microscopic and macroscopic domains.
87 See for example Klein, "Einstein's First Paper" (ref. 4); and Jammer, *Conceptual Development* (ref. 10), pp. 26–27, for citations and discussion of Einstein's and Debye's work. See also the letter from Lorentz to Wien, June 6, 1908; a full citation, a translation, and the original German are given in Kuhn, *Quantum Discontinuity* (ref. 5), pp. 194, 302.
88 Planck, "Wärmestrahlung" (ref. 54).
89 Planck, *Eight Lectures* (ref. 49), Fifth Lecture and the introductory comments to the Sixth Lecture.
90 See for example Klein, "Max Planck" (ref. 4), p. 474. This suggestion was first made by Léon Rosenfeld, "La première phase de l'évolution de la théorie des quanta," *Osiris* **2** (1936), 149–196.
91 See Planck, *Vorlesungen* (2nd ed.) (ref. 48), § 141, p. 145 of the translation of the second edition. See also Bailyn, *Survey* (ref. 10), p. 533.
92 Planck, *Vorlesungen* (1st ed.) (ref. 48), § 131, where he made this point in connection with both "natural radiation" and gas theory. Boltzmann made the same point in his *Gas Theory* (ref. 42), Vol. 1, § 6 and Vol. 2, §§ 41, 87.
93 *Ibid*., p. 151.
94 *Ibid*., pp. 151–152.
95 Needell, "Irreversibility" (ref. 9), pp. 31–33, 171–181; Darrigol, *c-Numbers*, pp. 66–70, and "Meaning," p. 19 (ref. 8). Needell quotes part of this passage, and I have in part followed his translation. Darrigol also quotes part of this passage, but in a different connection; see Darrigol, "Statistics" (ref. 8), p. 55. Kuhn also mentions this aspect of Planck's thinking, but does not develop it; see, for example, Kuhn, *Quantum Discontinuity* (ref. 5), pp. 118–120.
96 For discussion and bibliography, see Clayton A. Gearhart, "Einstein before 1905: The early papers on statistical mechanics," *American Journal of Physics* **58** (1990), 468–480; Jürgen Renn, "Einstein's Controversy with Drude and the Origin of Statistical Mechanics," *Archive for History of Exact Sciences* **51** (1997), 315–354; and Gallavotti, "Ergodicity" (ref. 36).
97 Planck, "Theorie" (ref. 43).
98 Darrigol, *c-Numbers*, pp. 72–73, and "Meaning," pp. 15, 19 (ref. 8).

99 Kuhn, *Quantum Discontinuity* (ref. 5), pp. 102–110, esp. p. 109, and in more detail, Kuhn, "Revisiting" (ref. 5), pp. 239–243, esp. p. 241.

100 *Ibid.*, pp. 108–109.

101 Kuhn, "Revisiting" (ref. 5), esp. pp. 240–241.

102 Darrigol, *c-Numbers*, p. 73. See also Darrigol, "Statistics," p. 55, and Darrigol, "Meaning," pp. 10, 19 (ref. 8).

103 Planck, "Gesetz" (ref. 44), p. 558, *PAV* (ref. 19), Vol. 1, p. 722.

104 Darrigol, "Meaning" (ref. 8), p. 19. Planck's reference is to Johannes von Kries, *Die Principien der Warscheinlichkeitsrechnung*, *Eine Logische Untersuchung* (Freiburg: Akademische Verlagsbuchhandlung, 1886), p. 36. Kuhn also discusses this passage in *Quantum Discontinuity* (ref. 5), pp. 121–122. Kries's treatment is, as the subtitle suggests, "a logical investigation," and as such, is not heavily mathematical. See also Kangro, *Radiation Law* (ref. 2), pp. 219–220 of the translation.

105 Albert Einstein, Review of Max Planck, *Vorlesungen über die Theorie der Wärmestrahlung*, *Beiblätter zu den Annalen der Physik* **30** (1906), 764–766; reprinted in Einstein, *CP* (ref. 60), Vol. 2, John Stachel, ed., Doc. 37. I have followed the translation in the accompanying translation volume.

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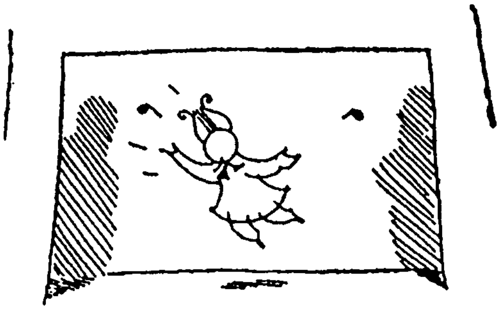

UNQUALIFIED QUALIFICATION

To a friend,
in case he misunderstand me

I grant you, I attacked your foe;
but this, I feel you ought to know,
is something you should not construe
to mean that I agree with you.

Piet Hein