Deriving the distribution law

Planck's discovery of equation (8) is likely to have encouraged him greatly, for it gave the problem of deriving his distribution law a far more concrete structure than it had previously possessed. But the problem itself remained. The combinatorial expression discovered by working backwards from his new distribution law is very different from the one Boltzmann had developed in deriving the equilibrium distribution of gas molecules. Planck must, therefore, still show that it is proportional to the probability appropriate to equilibrium radiation. Presumably that task called forth additional "strenuous work," but it was soon successfully concluded. By 14 December 1900, when Planck first described to the members of the German Physical Society the theoretical basis of the law he had presented to them two months before, he had in fact found two derivations, historically closely related but logically independent. In his December lecture, Planck outlined one and mentioned the existence of the other[21]; it was shortly made available in a paper received by the editors of the *Annalen der Physik* in early January 1901.[22]

As published, Planck's first derivation explicitly omitted one essential, though conceptually straightforward step. His second was complete, but it was presented in an extremely condensed form, one especially difficult to follow because, after introducing Boltzmann's relation between entropy and probability, Planck's derivations dealt with a distribution problem very different from Boltzmann's. As a result, until the appearance of his *Lectures on the Theory of Thermal Radiation* in 1906, many of Planck's contemporaries found his derivations extremely obscure, especially the second version, better known because published in the *Annalen*. Later historians have inherited their difficulties together with one still more severe. In 1910, H. A. Lorentz (1853–1928) derived Planck's law in a way that closely parallels Boltzmann's derivation of the distribution law for gases.[23] Planck adopted a similar method in the second edition of his *Lectures*, published in 1913, and it has been standard ever since. Under those circumstances the temptation to assimilate Planck's very different early derivations to the subsequently canonical Boltzmann-like form has proven irresistible. Since no such assimilation is possible, the few analysts of Planck's first quantum papers have concluded that he did not have at his command the probabilistic techniques on which his early derivations were based, and they have therefore dismissed his argument as hand-waving. More typical accounts simply paraphrase Planck's second proof, further condensing it in the process until even the possibility of comprehension is lost. Both approaches block, though in different ways, an understanding of the process by which the quantum entered physics. The one that treats Planck's argument as unproblematic, inevitably concludes that resistance to it was due exclusively to his introduction of the energy element $h\nu$. But the alternate approach, which dismisses Planck's derivation as incompetent, fails to identify the respects in which it departs not simply from Boltzmann's argument but from his approach. As a result, it joins the standard alternate approach in misrepresenting both the nature and the function of the energy element, Planck's central innovation. The integrity of Planck's combinatorial argument must therefore be restored before the nature of that innovation can be understood.

Recall, to begin with, the structure of the now standard derivation with which Planck's first formulations are regularly confounded: N resonators, all with the same frequency ν, are imagined, and the various ways in which a given total energy E may be distributed among them are examined.[24]† For that purpose the energy is imagined subdivided into P elements of size ε, so that $P\varepsilon = E$. A given distribution or state is then defined by a set of integers w_k, with $k = 0, 1, 2, \ldots, P$, and with w_k the number of resonators possessing k energy elements. Two distributions are distinct if they are described by different sets w_k. Any individual distribution can, however, be achieved in Z different ways, with

$$Z = \frac{N!}{w_0!\, w_1! \cdots w_p!}. \tag{9}$$

If one can show that all the ways of distributing the P indistinguishable energy elements over the N distinguishable resonators are equally probable (a problem to be considered in the next chapter), then Z is proportional to the probability W of the distribution specified by the w_k's. The proportionality factor can, furthermore, be neglected, for it appears only as an additive constant in the entropy, which is itself proportional to log W. The equilibrium distribution is therefore specified by the set of w_k's which maximizes log Z subject to the constraints

$$\sum_{k=0}^{P} w_k = N$$

and

$$\sum_{k=0}^{P} k w_k = P. \tag{10}$$

Clearly, this part of the standard proof of Planck's law is identical, both conceptually and mathematically, with the combinatorial proof developed by Boltzmann for gases.

In his two early derivation papers Planck's problem has a different structure, though it is described explicitly only in the first, his December lecture to the Physical Society. There, after a brief introduction explaining his reasons for using Boltzmann's relation between entropy and probability, Planck asks his audience to consider a reflecting enclosure that contains N resonators at frequency ν, N' at frequency ν', N'', at frequency ν'', and so on. The total energy of all these resonators is E_0, and it is distributed among them so that the set of N resonators at frequency ν has energy E, the set at ν' has energy E', and so on. Thereafter, Planck's problem is to compute the entropy of this particular distribution of the total energy E_0 over the $N + N' + N'' + \cdots$ resonators and, then, to discover its maximum with respect to the variation of the distribution of the total energy over frequency. That problem differs in two respects from the one considered in the now standard derivations that stem from Lorentz. First, sets of resonators at *different* frequencies are considered from the start. More important, the quantities to be varied in maximizing entropy or probability are simply the energies E, E', E'', etc., attributed to each frequency; the manner in which each of these energies is distributed over resonators at the corresponding frequency does not enter the argument; Planck has no need for parameters that correspond to the Boltzmann-Lorentz w_k's.

To compute the entropy of an arbitrary distribution Planck must introduce combinatorials, and for this purpose he follows Boltzmann in subdividing the energy continuum into elements of finite size. It is at this point that he introduces the further novelty that was soon to prove the most consequential of all. For his purpose, unlike Boltzmann's, the size of the energy elements ε, ε', ε'', etc., must be fixed and proportional to frequency.[25] Consideration of that vital step is the subject of the next chapter, but the passage in which Planck introduces it must be noted here, for it illustrates an aspect of his lecture that helped mislead readers about his intent.

> The distribution of energy over each type of resonator must now be considered, first the distribution of the energy E over the N resonators with frequency ν. If E is regarded as infinitely divisible, an infinite number of different distributions is possible. We, however, consider—and this is the essential point—E to be composed of a determinate number of equal finite parts and employ in their determination the natural constant $h = 6.55 \times 10^{-27}$ (erg × sec). This constant multiplied by the frequency, ν, of the resonator yields the energy element ε in ergs, and, dividing E by ε, we obtain the number, P, of energy elements to be distributed over the N resonators.[26]

Because Planck, here and for some time after, considers only the single set of resonators with frequency ν and because he later omits the computation of a maximum, which would have demanded explicit recourse to resonators at other frequencies, the difference between his argument and that of Lorentz is obscured.

Planck next defines a "complexion" (an expression, he points out, "used by Boltzmann for a *similar* concept"[27]) as a particular specification of the set of numbers k_i, which fixes the number of elements ε attributed to the various resonators in the set of N. (No other term was available, but Planck might better have reserved "complexion" for the distribution determined by the full set of numbers k_i, k_i', k_i'', etc.) The total number of complexions compatible with a distribution in which the N resonators of frequency ν possess energy $E(=P\varepsilon)$ is just $(N + P - 1)!/(N - 1)!\,P!$, i.e., the combinatorial expression first discovered by working backwards to equation (8). It is also, as it should be, the expression derivable by summing Lorentz's equation (9) over all values of the w_k's compatible with the constraints, equations (10). For Planck's problem, unlike Boltzmann's and Lorentz's, any set of the w_k's that satisfy these constraints corresponds to *the same distribution* of the total energy E_0.

From this point, Planck's path is straightforward. Having found the number of ways in which the energy E can be distributed over the N resonators at frequency ν, one must find the corresponding numbers for the N' resonators with energy E' and frequency ν', the N'' resonators with energy E'' and frequency ν'', etc. These numbers multiplied together yield "the total number, R, of possible complexions compatible with the provisionally selected [*versuchsweise vorgenomennen*] distribution of energy over all resonators."[28] To find the equilibrium distribution one then simply maximizes R or $\log R$ by varying the energies at the various frequencies subject to the constraint on total energy. Having found the equilibrium distribution R_0, one may, ignoring the additive entropy constant, write the equation for equilibrium resonator entropy as $S_0 = k \log R_0$, with k "a second natural constant" of value 1.346×10^{-16} erg/deg.[29] Temperature is then determined from the standard thermodynamic relation $\partial S_0/\partial E_0 = 1/T$, and the result manipulated to yield the distribution law.

Planck did not carry out these mathematical manipulations. Early in the sketch of his argument he had spoken of "seeking the [equilibrium] distribution, if need be with the aid of trial and error [*eventuell durch Probieren*]." After concluding his outline, he described the computations it would require as "obviously very roundabout [*freilich sehr umständlich*]." Rather than involve himself with any procedure so cumbersome, Planck mentioned the existence of "a more general, entirely straightforward means of computing the *normal* distribution which would result from the preceding steps and which follows immediately from their description."[30] The outcome of that computation he simply wrote down, reserving the description of the alternate method for the paper he submitted to the *Annalen* three weeks later.

Except for trial-and-error steps required to transform non-integral multiples of ε to integral multiples, the mathematical manipulations missing from Planck's paper can, in fact, be carried through straightforwardly. If Planck did not discover a quick way to do so, that is partly because, given an alternate, he had no occasion to work on the problem and probably also because he was following Boltzmann extremely closely. In the one place where Boltzmann had dealt with a mathematically closely related problem (the most probable distribution of molecules restricted to energies $0, \varepsilon, 2\varepsilon, \ldots$), he had employed an elaborate ad hoc method, which, including the last trial-and-error stages, occupied ten dense pages.[31] Adapting that argument to his own problem is presumably what Planck, with reason, wished to avoid. But the argument Planck omitted is nevertheless worth examining here, for doing so will both clarify his derivation-sketch and supply the background needed to understand his quite different alternate form. For that purpose, it is convenient to drop Planck's prime notation and write E_ν, N_ν, P_ν, and ε_ν for the energy, number of resonators, number of energy elements, and size of the energy element at each frequency. When Stirling's formula is then applied to equation (8), the entropy of any given distribution can be written, for large N_ν and P_ν,

$$S_{E_0} = k\sum_\nu \{(N_\nu + P_\nu)\log(N_\nu + P_\nu) - N_\nu \log N_\nu - P_\nu \log P_\nu\}. \qquad (11)$$

This is the basic formula for resonator entropy. Planck's sketch envisages maximizing it subject to the constraint

$$\sum_\nu P_\nu \varepsilon_\nu = \sum_\nu h\nu P_\nu = E_0.$$

That task can be carried out somewhat more straightforwardly than Planck may have realized, but it ordinarily yields non-integral values of the P_ν's, and these must be adjusted by trial and error. Here we may avoid that problem with the aid of a substitution that Planck uses for other reasons. If U_ν is the *average* energy of the N_ν resonators at frequency ν, then $N_\nu U_\nu = P_\nu \varepsilon_\nu$. For sufficiently large N_ν, the variations of U_ν are therefore effectively continuous as P_ν runs through successive integral values. Equation (11), rewritten as a function of the U_ν's and the arbitrary integral parameters N_ν, thus yields, after brief manipulation, an equation for entropy as a continuous function of the mean resonator energies,

$$S_{E_0} = k\sum_\nu N_\nu \left\{\left(1 + \frac{U_\nu}{\varepsilon_\nu}\right)\log\left(1 + \frac{U_\nu}{\varepsilon_\nu}\right) - \frac{U_\nu}{\varepsilon_\nu}\log\frac{U_\nu}{\varepsilon_\nu}\right\}. \qquad (12a)$$

With the insertion of Planck's special hypothesis, $\varepsilon = h\nu$, that equation becomes

$$S_{E_0} = k\sum_\nu N_\nu \left\{\left(1 + \frac{U_\nu}{h\nu}\right)\log\left(1 + \frac{U_\nu}{h\nu}\right) - \frac{U_\nu}{h\nu}\log\frac{U_\nu}{h\nu}\right\}. \qquad (12b)$$

These are now the formulas to be maximized, subject to the constraint on total energy,

$$E_0 = \sum_\nu N_\nu U_\nu. \qquad (13)$$

To find a maximum one sets $\delta(S_{E_0} - \mu E_0) = 0$, with μ a multiplier to be determined. Straightforward manipulation shows that the entropy will be maximum and the constraint satisfied only if the U_ν's are governed by

$$U_\nu = \frac{h\nu}{e^{\mu h\nu - 1}}.$$

The insertion of that result in equations (12b) and (13) yields formulas for the entropy and total energy at equilibrium as functions of μ. From those expressions, μ can be evaluated by applying the standard relation $\partial S_{E_0}/\partial E_0 = (\partial S_{E_0}/\partial\mu)/(\partial E_0/\partial\mu) = 1/T$. Straightforward manipulation yields $\mu = 1/kT$, and the equilibrium distribution becomes

$$U_\nu = \frac{h\nu}{e^{h\nu/kT} - 1}, \qquad (14)$$

just the form Planck seeks. One of its significant characteristics, he quickly notes, is that the corresponding distribution for the field, $u_\nu = (8\pi\nu^2/c^3)U_\nu$, satisfies the Wien displacement law.

Other aspects of the paper in which Planck presented his first derivation will concern us later, both at the end of this chapter and in the next, but the relation of the preceding argument to the better known derivation he prepared for the *Annalen* must be considered first. In the latter, Planck does not introduce the ad hoc, and correspondingly implausible, relation $\varepsilon = h\nu$ as an hypothesis. Instead, he supposes from the start that he is dealing with resonators already in equilibrium with the radiation field, and he enforces that condition at the appropriate point in his argument by introducing the displacement law, now a precondition rather than a consequence of his derivation. The counting of states and the justification of recourse to the combinatorial expression, equation (8), proceed exactly as before, since the relevant arguments apply to equilibrium as well as to more general distributions. Now, however, since equilibrium is presupposed, there is no place for further maximization. Instead, Planck calls upon the displacement law to specify the still missing elements in his expression for entropy. Both his distribution law and the mysterious formula $\varepsilon = h\nu$ emerge at once, the latter now as a consequence of the derivation.

In Planck's new proof, equation (11) continues to express the total entropy of all resonators at all frequencies. Since he is now dealing with the equilibrium case, however, he can ignore exchanges between resonators at different frequencies and consider only the expression for the equilibrium entropy S_{N_ν} of N_ν resonators at any frequency ν. By eq. (11) or by a direct count of complexions, it is given by

$$\begin{aligned} S_{N_\nu} &= k\{(N_\nu + P_\nu)\log(N_\nu + P_\nu) - N_\nu \log N_\nu - P_\nu \log P_\nu\} \\ &= kN_\nu\left\{\left(1 + \frac{U_\nu}{\varepsilon_\nu}\right)\log\left(1 + \frac{U_\nu}{\varepsilon_\nu}\right) - \frac{U_\nu}{\varepsilon_\nu}\log\frac{U_\nu}{\varepsilon_\nu}\right\}. \end{aligned}$$

Since, as Planck shows next,[32] the displacement law applied to resonators demands that $S = \phi(U/\nu)$, the preceding expression is compatible with an equilibrium distribution only if ε is proportional to ν. Imposing that condition in the form $\varepsilon = h\nu$, dropping the subscript ν, and dividing the preceding equation by N yields the entropy S of a single resonator at equilibrium:

$$S = k\left\{\left(1 + \frac{U}{h\nu}\right)\log\left(1 + \frac{U}{h\nu}\right) - \frac{U}{h\nu}\log\frac{U}{h\nu}\right\}. \qquad (15)$$

A final application of the thermodynamic relation $\partial S/\partial U = 1/T$ gives the distribution law in the form of equation (14). Rewritten for the density of radiant energy in the field, it becomes

$$u_\nu = \frac{8\pi h\nu^3}{c^3}\frac{1}{e^{h\nu/kT} - 1}. \qquad (16)$$

Except for the problems raised by the introduction of the relation, $\varepsilon = h\nu$, Planck's argument is, I think, unexceptionable. Unfortunately, however, his presentation of it was extremely condensed and his derivation correspondingly obscure. Writing for the *Annalen* in January 1901, Planck failed to describe the general problem—distributing given total energy E_0 over resonators at various frequencies—which he had considered in his December lecture and which provided the conceptual basis for his alternate proof. Instead, having postulated equilibrium, he immediately took up the problem of distributing energy $E(=NU = P\varepsilon)$ over N resonators at a single frequency, pointing out that there are just $(N + P - 1)!/(N - 1)!\,P!$ ways in which that can be done, from which point the argument continued as above. Under those circumstances it is not surprising that his contemporaries, especially those unfamiliar with his December lecture, found his presentation hard to follow. Nor is it difficult to understand why recent commentators, noticing that Planck deals explicitly only with resonators at a single frequency, have emphasized his apparent failure to introduce the Boltzmann–Lorentz count of complexions, equation (9), and to maximize the result by varying the w_k's. Planck's *Annalen* paper makes clear conceptual sense only when systematically juxtaposed with his December lecture, itself easily dismissed by virtue of its incompleteness.

During 1901 Planck published several more papers on his black-body theory, but none repeats, except by brief reference, either derivation of his radiation law. Thereafter, he published nothing further on the black-body problem until 1906, when the first edition of his *Lectures* appeared. In the *Lectures*, for reasons to be discussed in Chapter V, the first derivation is not even mentioned. Instead, Planck presents again the elements of his *Annalen* argument, but in a new order and with the previously missing explanatory comments supplied.[33] The displacement law is introduced before combinatorials, and Planck emphasizes both that its introduction restricts his treatment to the equilibrium case and that the restriction distinguishes his problem from Boltzmann's. Nevertheless, he describes how Boltzmann's method of counting complexions could be applied to his problem and points out why one would then have to sum over all possible Boltzmann distributions to obtain the number of complexions relevant to the problem he has in mind. Rather than perform the summation, he produces his own combinatorial form, equation (8), as its result. These and other additions to his argument suggest that, by the time he wrote his *Lectures*, Planck had recognized the problems his first formulation might present to readers. But they do not suggest that the conceptions that underlay the derivation had changed. A coherent derivation of Planck's law does not demand recourse to the Boltzmann–Lorentz count of complexions nor to explicit maximization.

The new status of the radiation constants

Planck closed his December lecture to the Physical Society by looking once more at the radiation-law constants and calling attention "to an important consequence of the theory [just] developed, one which makes possible a further test of its admissibility."[34] Nothing in his own earlier work or in that of his contemporaries had suggested that any such test might exist. Planck took its emergence especially seriously, as is indicated by the pattern of his publications relating to the new theory during 1901 and 1902. When he prepared an account of his second derivation for the *Annalen*, he reserved for a brief separate paper his remarks on the important new consequence of his theory, presumably hoping in this way to call special attention to it.[35] Later in the year, he described the new result at greater length in a contribution to a volume of papers honoring the Dutch physicist Johannes Bosscha. Though it added nothing of substance to the remarks he had reported in the *Annalen* earlier in the year, Planck republished it in that widely read journal during 1902.[36] With one exception, to be discussed briefly in the next chapter, these articles are the only ones on his new theory that Planck published anywhere between the beginning of 1901 and the appearance of his *Lectures* in 1906. Whether for its intrinsic importance or for its special evidential appeal, Planck emphasized the new consequence of his theory more strongly than the theory itself.

To discover the reason for Planck's special emphasis, return briefly to the Wien distribution law. When Planck first announced his success in deriving it, he also reported up-to-date values for the two constants it contained: $a = 4.818 \times 10^{-11}$ deg·sec; $b = 6.885 \times 10^{-27}$ erg·sec.[37] Simultaneously, he grew as nearly ecstatic as he ever could over the glimpse their determination offered of a natural system of units. Max Thiesen, in the paper discussed early in this chapter, extended the point. The appearance of two natural constants was not, he pointed

out, characteristic of Wien's distribution alone. The displacement law itself necessitated their presence in any satisfactory radiation distribution law. Such a law must, Thiesen emphasized, be expressible by a known form involving an arbitrary function Ψ of a single argument λT. One constant would be required to determine the amplitude of Ψ and another—"since Ψ cannot, as is easily seen, be a simple power [of its argument]"[38]—to render dimensionless the argument of the exponential term. Under these circumstances, there was nothing surprising either about the reappearance of two constants in the distribution law Planck announced in October or about the values he attributed to them when deriving the law in December: $k = 1.346 \times 10^{-16}$ erg/deg; $h = 6.55 \times 10^{-27}$ erg·sec. If Planck's law were to coincide with Wien's in the high-frequency limit, the value of h would have to be very nearly that of b, and h/k of a. That the reported values were only close, not identical, was due partly to Planck's use of Lummer and Pringsheim's latest data and partly to the difference in the behavior of the two distribution laws in the region to which the data applied.[39]

What was extremly surprising, however, was the new significance these constants gained by virtue of the role of k in Boltzmann's combinatorial definition of entropy. Boltzmann had not himself introduced any similar constant, but he had, for a perfect monatomic gas in equilibrium, compared the value of his permutability measure Ω with that of the thermodynamic entropy $\int dQ/T$.[40] Planck rewrote the relationships thus obtained in the form $S = \omega R \log Z_0$, where Z_0 is the equilibrium permutation number for the gas, R the universal gas constant, and ω the ratio of the weight of a molecule to that of a mole of the corresponding gas, the reciprocal of Avogadro's number. If that gas were in equilibrium with radiation, the total entropy of the system could, he then showed, be the sum of the entropy of its parts only if the radiation constant k were equal to ωR. Since R was well known, that relationship enabled him to compute ω and from it both Loschmidt's number and the electronic charge. The values he obtained were comparable with existing estimates and, if his method were sound, considerably more precisely determined. For Loschmidt's number he found 2.76×10^{19} molecules/cm^3, which he compared with a standard previous estimate of 2.1×10^{19}; its modern value is 2.69×10^{19}. For the electronic charge he found 4.69×10^{-10} esu, which he compared with recent estimates of 1.29×10^{-10} and 6.5×10^{-10}; its modern value is 4.803×10^{-10}.

Planck concluded his lecture by urging that his new values be tested by more direct means, but the experiments he sought were slow in appearing. Ernest Rutherford (1871–1937) is the only scientist known to have been drawn to "the general idea of a quantum of action" by the special accuracy of Planck's computations. Though his interest proved consequential (it enabled him "to view with equanimity and even to encourage Professor Bohr's bold application of the quantum theory to explain the origins of spectra"), it was apparently unique.[41†] By the time additional measurements of the electronic charge unequivocally demonstrated the accuracy of Planck's prediction, his theory had won widespread acceptance by other means.

Accuracy was not, however, what made the new consequence of Planck's theory especially impressive. Rather it was that, in this area, he had obtained any results at all. Without apparently having intended to do so, Planck had produced a concrete quantitative link between electromagnetic theory, on the one hand, and the properties of electrons and atoms, on the other. At the turn of the century the search for such links was central to perhaps the most active, exciting, and troublesome area of physics research, for the relationship between electrodynamics and mechanics had, for a generation, been growing increasingly problematic. Maxwell's theory, which was almost universally accepted at the close of the nineteenth century, offered no obvious place for the introduction of either matter or discrete charge. Many physicists still expected that the resulting gap—widened and deepened by the discovery of the electron in the 1890s—would be bridged by the design of an appropriate mechanical model of the ether. A mechanical ether would, by its nature, interact with ordinary molecules, and its displacements would constitute the electromagnetic field. But the discouraging outcome of strenuous efforts to design ether models had led other physicists to doubt that a mechanical theory would ever succeed. Led by H. A. Lorentz, a number of them hoped ultimately to reduce matter and mechanics to electrodynamics within what they increasingly referred to as "the electromagnetic view of nature."[42]

Though he published nothing about these issues until he became involved with the special theory of relativity after 1905, Planck was very much aware of them. From the beginning of his career, mechanics had been for him a model science. Since 1894 he had been concerned with electromagnetic theory as well, and by late 1898 he was exchanging long letters on that subject with Lorentz. Their main topic is the standard one of ether drag considered in relation to the Fizeau and Michelson-Morley experiments. Questions about the interaction between ether and matter emerge repeatedly, and Planck proves willing to contemplate such possibilities as the ether's being subject to gravitational attraction. His conviction, however, is that there is "no basis for attributing the properties of ponderable matter to the optical ether, for the latter differs from the former in its most essential characteristics."[43] How, then, account for their interaction?

Remarks like this one are by no means unique to Planck. His letters to Lorentz suggest the context within which his response to the unexpected discovery that experiments on radiation could yield constants relating to matter and charge must be viewed. Though the joint entry of the constant k into the divergent realms of mechanics and radiation provided no conceptual bridge between the two, it was a striking, concrete clue to the direction in which such a bridge might be sought. Since, furthermore, that clue involved the universal natural constants which Planck had so emphasized the year before, the special pleasure and conviction its discovery generated is not surprising. It suggested that Planck had found something more important and fundamental than a derivation of his own distribution law.

Planck had a son Erwin who was seven years old in late 1900 and who, late in life, at least twice reported a memorable walk he had taken with his father at about the turn of the century. On that occasion, the younger Planck said, his father had told him that he had just made the greatest discovery in physics since Newton. The details of that story may well be retrospective—Planck need only have said, for example, that he was on the track of such a discovery—but it is likely that some such conversation occurred.[44†] If it did, however, then the discovery to which Planck referred almost surely involved his unveiling the special characteristics of the constant k. No other event in his scientific career provides so firm a basis for the claim that his research had provided or might provide a previously inaccessible glimpse into nature's innermost workings. His attempt to explain irreversibility without special assumptions had been abandoned. His radiation law had yet to be severely tested. His derivation of that law remained firmly within the classical tradition, a point to be further explored in the next chapter. Until others intervened during 1906 (by which time Erwin was thirteen, a developmental interval he would likely have remembered), neither the law nor its derivation provided a basis for a claim to fundamental innovation. The joint role of the constant k at least promised such a result, and I am aware of no other aspect of Planck's work that did so.

V

THE FOUNDATIONS OF PLANCK'S RADIATION THEORY, 1901–1906

Planck's achievements in the scant four months from late September 1900 to early January 1901 presage a turning point in the development of physics. During the decade that followed its discovery, steadily improving experimental tests continued to support his distribution law.[1] Simultaneously, attempts to prove that law without recourse to the odd relation $\varepsilon = h\nu$ regularly proved fruitless, as did attempts to derive the fixed energy element from classical principles. Beginning in 1905, new analyses and new applications of Planck's theory increasingly restricted the ways in which the relation $\varepsilon = h\nu$ could be interpreted, making its incompatibility with classical theory more and more apparent. With the advantage of hindsight, it is clear that there could have been no turning back. Assimilation of the papers examined in the preceding chapter would require fundamental reconstruction of well-established theory.

The bases for such an evaluation were not, however, available in 1901. Nor had Planck's theory then taken a form that brought it into explicit conflict with older views. To see how Planck's turn-of-the-century achievements came to mark a turning point, one must therefore ask, first, how Planck and others interpreted what he had done in his derivation papers, and second, what happened to change their initial interpretations. Since reactions from others were rare and, with one possible exception, of little significance before 1905, most of this chapter is restricted to an examination of Planck's own understanding of his results during the period to the first publication of his *Lectures on the Theory of Thermal Radiation* in 1906. Prerequisite to that examination is the suppression of numerous associations that the first view of Planck's constant h and of the energy element $h\nu$ inevitably now bring to mind.

The continuity of Planck's theory, 1894–1906

Note, to begin with, the relation between Planck's first derivation papers and the classical theory of black-body radiation he had developed to an apparently successful conclusion between 1894 and late 1899. When, in the latter year, Planck summarized for the *Annalen* the outcome of his earlier research, he acknowledged that his theory was in one respect essentially incomplete: it lacked a proof of the uniqueness of the function he had "defined" as resonator entropy. In March 1900, with the Wien law in doubt, he attempted to bridge that gap with the argument that eventuated in equation (IV-5), $\partial^2 S/\partial U^2 = -\alpha/U$, from which the Wien law followed. Presenting his alternate distribution law in October, he emphasized that that argument was the only portion of his previous work that need now be set aside, thus reintroducing the gap he had apparently bridged in March. The new combinatorial derivation papers he presented in December and January provided a new means of bridging it, for their product was a unique entropy function $S(U)$ to which his earlier theory could again be applied. From 1901 through 1906 that is what Planck and most readers took the role of his combinatorial argument to be. It provided a substitute for the inadequate uniqueness proof of March 1900. Though Planck's radiation theory raised problems that would require solution, those problems did not seem to threaten the integrity of his own earlier work, much less of classical physics.

That Planck himself took this view of the novelties introduced in his December and January derivation papers is strongly indicated by the last of the articles he prepared for the *Annalen* in 1901. Its title was "On Irreversible Radiation Processes," to which he added in parentheses the word "Addendum"[2]; its opening reference was to the summary paper he had published in the same journal under the same title at the beginning of 1900; after an opening statement of purpose, both its formulas and paragraphs were numbered to make the new paper a direct extension and continuation of the old. Planck's introductory remarks remind readers that he had originally defined as entropy a function that led to the Wien law. His supposition that that function was unique had, he acknowledged, proved to be mistaken, but he nevertheless reiterated his conviction that an examination of "the most general possible radiation process" would be compatible with only one form of entropy function. That way of finding a unique form did not, he continued, seem feasible "in the present state of our knowledge," but to demonstrate irreversibility one need only show that a given candidate for entropy function changes irreversibly with time. At that point, Planck referred to the derivation paper he had sent to the *Annalen* in January for an entropy function that "seems to be compatible with the facts determined to date by experiment."[3] In the form appropriate to a single resonator, the function had been

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

and Planck at once proceeded to show (but now in the paragraphs numbered to follow his earlier paper, on the results of which he regularly called) that the corresponding total entropy S_t must satisfy $\mathrm{d}S_t/\mathrm{d}t \geq 0$. The entropy function he had derived from combinatorial techniques was thus fully assimilated to the radiation theory he had developed before the turn of the century.

Two aspects of Planck's subsequent publications on black-body theory make that assimilation especially impressive. First, there are none until 1906. Submitted in mid-October 1901, the paper just discussed is the last, or the last but one, that Planck prepared on this subject before the appearance of his *Lectures*. In a sense to be explored further below, Planck's insertion of his new entropy function into his older theory marked the successful conclusion of the research he had begun in 1894. He could and did then turn to other topics, though we shall see that the principal one he chose can be plausibly linked to what he saw as the puzzle his theory still posed. Second, when Planck did publish again on black-body theory, the volume he produced was essentially an expanded, self-contained, and much clarified version of material he had submitted to the *Annalen* between late 1899 and the end of 1901.

Of the five chapters in the *Lectures*, the first, "Fundamentals and Definitions," is an elementary account of black-body radiation and Kirchhoff's law. The second, "Consequences of Electrodynamics and Thermodynamics," opens with Maxwell's equations and from them, together with thermodynamics, derives radiation pressure together with the Stefan-Boltzmann and Wien displacement laws. With the latter written in the form $u = (\nu^3/c^3)F(T/\nu)$, where u is the density of radiant energy and F an unknown function, the chapter concludes by showing how entropy, temperature, and a few related quantities can be expressed in terms of the undetermined function F. So far, Planck has presented background material, appropriate to a student audience but not to his original research reports. Chapter III, "Emission and Absorption of Electromagnetic Waves by a Linear Oscillator," presents results Planck had developed for himself, all from a period prior to the derivation of his own distribution law. The concept of a damped resonator in interaction with the field is introduced and used to derive appropriate equilibrium equations, including the fundamental relationship $u = (8\pi\nu^2/c^3)U$. The displacement law is rewritten in the special form $S = H(U/\nu)$, which is thereafter repeatedly deployed. Combinatorials enter for the first and only time in the next chapter, "Entropy and Probability," where Planck presents an extended and much clarified version of his second combinatorial derivation, thus at last fixing the form of the unknown function F. With that function determined, Planck returns, in a closing chapter, "Irreversible Radiation Processes," to the one element of his pre-1900 theory not previously introduced. There he develops non-equilibrium versions of the electromagnetic equations of his third chapter, applies the concept of natural radiation to them, and emerges with a proof, like that submitted to the *Annalen* in October 1901, of his electromagnetic H-theorem: entropy can only increase with time.

The structure of Planck's argument will gain in significance when the first edition of the *Lectures* is compared, late in this book, with the very different revised editions of 1913 and 1921, but its central lesson should already be clear. As of 1906, when Planck published the first full and mature account of his theory of thermal radiation, that theory still included all the main elements developed in the research program he had pursued from 1894 through 1901. They entered his text, furthermore, in very nearly the order, and to serve precisely the functions for which they had initially been developed. Except for the new importance they lent to the radiation constants, the events of late 1900 had not visibly changed Planck's view of the nature of the theory he had developed in the preceding years.

More than autobiography and pride of authorship account for the central position of Planck's pre-combinatorial achievements in the *Lectures*. They are an integral part of a sustained and coherent argument; in both obvious and subtle ways Planck needed them. The recourse to combinatorials provided information only about the equilibrium distribution of *resonator* energy with frequency. Planck's concern, however, had been and remained with *radiation*. His resonators were imaginary entities, not susceptible to experimental investigation.

Their introduction was simply a device for bringing radiation to equilibrium, and it was justified, not by knowledge of the physical processes involved, but by Kirchhoff's law, which made the equilibrium field independent of the equilibrium-producing material. Before his combinatorial arguments could be put to their intended use or to any other, Planck would have to convert resonator energy to field energy by means of the proportionality factor, $8\pi\nu^2/c^3$, which he had derived from Maxwell's equations. Though the use of those equations and that factor would shortly seem an inconsistency in Planck's theory, sometimes a reason for rejecting it, Planck and most early readers saw no such problem. Introduced at the start of his book, Maxwell's equations remained basic throughout. In their absence Planck could not have treated the interaction of field and resonators, attributed a specified entropy to the field, or produced an electromagnetic H-theorem, the last presented in 1906 as the crowning achievement of his book, though dropped soon thereafter. The point is not simply that Planck needed to use Maxwell's equations, but that he was apparently unaware of the slightest awkwardness in doing so.

Planck's need for the concepts of classical electrodynamics was not, in any case, restricted to the points where his argument required that he produce quantitative links between the behavior of resonators and of the field. Those concepts play an indispensable qualitative role within his combinatorial argument as well. In gas theory, whether approached through the H-theorem or combinatorials, only a collection of similar particles is characterized by entropy; attributing a value of that thermodynamic function to a single particle is nonsense. The radiation problem, on the other hand, requires only a single resonator at each frequency; if several happen to be present, each one must be in equilibrium with the field. Planck's early research had dealt exclusively with the one-resonator case. When, in March 1900, he first considered a problem involving n resonators at the same frequency, Wien had promptly challenged him to show that the result he obtained could be applied to a single resonator as well.[4] That challenge was still relevant at the end of the year, when Planck's use of combinatorials required that he again consider the many-resonator case. By itself, the combinatorial argument led only to equation (IV-12) for the entropy of a collection of N resonators. To put that result to use, Planck had first to produce a formula for the entropy of a single resonator, equation (IV-15). For that step there was no precedent in Boltzmann's work or elsewhere. What could the entropy of a single resonator mean?

Planck addressed himself to the problem directly in the second paragraph of his December 1900 lecture to the Physical Society:

> Entropy signifies disorder, and this disorder I believed I was required to recognize in the irregularity with which the vibrations of a resonator change their amplitude and phase even in a stationary radiation field. . . . The constant energy of a stationary vibrating resonator is therefore to be conceived simply as a time average, or, *what comes to the same thing*, as the momentary average of the energy of a large number of identical resonators which are sufficiently separated in that stationary field so that they cannot reciprocally influence each other.[5]

The same analysis is repeated early in Planck's second derivation, where it immediately precedes the appropriate but misleading clause, "the entropy, S_N, is a consequence of the disorder with which the total energy U_N is distributed over the individual resonators."[6] It recurs in his *Lectures*, where it takes on additional significance because it is explained in terms of the independent components in the Fourier expansion of the amplitude of a damped resonator:

> It is, therefore, these numerous independent partial vibrations which play the role with respect to elementary disorder that is played in a gas by the numerous molecules in constant interpenetrating motion. Just as one may not speak of the finite entropy of a gas if. . . the velocity of all its molecules are in some way ordered, so a resonator possesses no finite entropy if its vibrations are simply periodic or follow some determinate law which regulates all details [of its motion]. . . . In short, for the thermal vibration of a resonator the disorder is temporal, while for the molecular motions of a gas it is spatial. For the computation of entropy, however, this difference proves less weighty than may at first appear; for it may be removed by a simple observation [the equivalence of spatial and temporal averages] which also marks an advance from the viewpoint of uniform treatment.[7]

Both those passages, but most explicitly the latter, indicate the extent to which the concepts underlying Planck's combinatorial theory remain, even in 1906, the ones with which he had begun his research more than a decade before. Interacting with an arbitrary field, the vibrations of a single damped resonator are still described by a Fourier series, which governs the continuous variation of its amplitude and phase with time. Under those circumstances, the time average of its energy can be calculated by known techniques, presumably those developed both in Planck's pre-1900 papers and again in the *Lectures*.[8†] Only because that average is the same as the average energy of a collection of N independent resonators at a single instant of time may the combinatorial definition of entropy be applied to the radiation problem at all. In the *Lectures*, as in the papers written six years before, Planck's conception of his theory remains classical.

Planck had recognized all these aspects of his theory when he presented his first combinatorial papers in 1900 and 1901. In the *Lectures*, however, he also noted a consequence he may not have seen at that time, and it explains what might otherwise seem oddities in his book. Immediately after the passage just quoted, Planck points out that, in the absence of equilibrium, the time average of the energy of a selected resonator need not, and generally will not, be the same as the space average of the energy of all resonators at the same frequency. Equilibrium is therefore a "necessary precondition" of his entire combinatorial approach. Combinatorial expressions representing non-equilibrium states (including those in his December 1900 Physical Society lecture) are therefore prohibited together with arguments that would demonstrate irreversibility by recourse to transitions from less to more probable states. Only after abandoning the initially classical basis of his theory would Planck reintroduce them. Simultaneously, he would drop the damping term in the resonator equation and eliminate his electromagnetic H-theorem from his text.

Natural radiation and equiprobable states

An additional illustration of the intimate interpenetration of the early electromagnetic and the subsequent combinatorial aspects of Planck's work is provided by the important use he continues to make of the concept of natural radiation. Originally introduced as a condition on the amplitudes and phases of permissible electromagnetic radiation, it is, from the start, basic also to his justification of his way of choosing and counting complexions. Early in his December lecture to the Physical Society, Planck remarked that to derive his distribution law "it is only necessary to give the hypothesis of 'natural radiation' which I introduced into electromagnetic theory, a somewhat extended interpretation."[9] What he had in mind begins to emerge together with other significant information towards the end of his paper where he discusses "the question of the necessity of the derivation [just] given." It rests, Planck says, upon a single proposition, which can be divided into two parts:

> 1. that the entropy of the system in a given state is proportional to the logarithm of the probability of that state, and 2. that the probability of any such state is proportional to the number of complexions which correspond to it, or in other words that any determinate complexion is just as probable as any other. The 1st proposition, applied to radiation processes, probably amounts to a definition of the probability of a state since for radiation one possesses no means for defining probability other than a determination of entropy. Here lies one of the decisive differences from the corresponding circumstances of gas theory. The 2nd proposition provides the core of the theory just developed, and its proof can in the final analysis only be provided by experiment. It can also be regarded as a more precise version of my hypothesis of natural radiation which I previously embodied only in the statement that the radiant energy must be completely "irregularly distributed" over the individual partial vibrations which constitute it.[10]

Planck's second paper on his derivation includes similar, though far more concise, remarks, which add one element essential to an understanding of their author's intent. After repeating that experiment will have to determine the legitimacy of the hypothesis that attributes equal probability to each complexion, Planck continued:

> Conversely, if experiment decides in favor of the hypothesis, it will be possible to draw further conclusions about the special nature of the resonator vibrations, that is, to use the words of J. v. Kries, about the character of "the indistinguishable elementary regions, comparable in their magnitudes" which enter into the [radiation] problem.[11]

To discover the viewpoint that underlies these passages, begin by noting that there is nothing new about using a definition to supply an hypothesis needed for the completion of statistical arguments. Boltzmann himself had introduced molecular disorder as the condition molecules must satisfy to permit a particular step in his derivation of the rate of collision between gas particles.[12] Planck had defined natural radiation in the same way, by means, that is, of a mathematical condition the Fourier components of the field must satisfy to permit the derivation of his H-theorem.[13] He cannot, under those circumstances, have been surprised by the discovery that his combinatorial argument demanded recourse to a similar device.

Nevertheless, as Planck also recognized, his new appeal to natural radiation did, in one respect, distinguish his argument from Boltzmann's. Though Boltzmann had required molecular disorder to derive the H-theorem for gases, no obviously similar hypothesis had been needed to complete his combinatorial derivation. Instead, Boltzmann had called, however intuitively and imperfectly, upon Liouville's theorem or on collision theory to justify the assertion that any molecule might, with equal probability, be found in equal volumes of phase or velocity space.[14] In his considerations of resonators in interaction with radiation, Planck could have recourse to no similar theorem. That is what he had in mind when, in the first of the quotations above, he wrote: "for radiation one possesses no means for defining probability other than a determination of entropy. Here lies one of the decisive differences from the corresponding circumstances of gas theory." In the absence of a substitute for Liouville's theorem, however, equiprobable configurations could not be specified a priori. The ultimate justification of any particular choice must inevitably be from experiment.

That point has a converse, which Planck makes explicit in the second of the passages quoted above. Experimental confirmation of the law he had deduced must supply information about "the special nature of the resonator vibrations." More precisely, experimental confirmation must provide information about the relative probability[15†] of the various possible sets of coefficients in the Fourier series that specifies the change of resonator configuration with time. This last characteristic is, of course, the one that renders Planck's "definition" of probability a refinement of his concept of natural radiation. An assertion about the relative probability of different sets of Fourier coefficients is an hypothesis about the relative frequency with which particular sorts of resonator motions occur in nature. It is thus an assertion of essentially the same sort as the one Planck had used to introduce natural radiation before.

Planck's original derivation papers, from which the preceding quotations are taken, were addressed, of course, to an audience familiar with one or more of the extended discussions of natural radiation he had presented since 1898. For an audience without that background, approaching the same papers now in search of quantum theory, his remarks on the subject are inevitably cryptic. But natural radiation also plays a major role in the chapter, "Entropy and Probability" in the *Lectures*, where Planck redeveloped his views at length from first principles. There, too, it emerges as a physical hypothesis about the distribution of microstates, and its role is to permit a definition of probability and thus a derivation of the second law. In the *Lectures*, furthermore, Planck extends to gases the argument he had developed five years before for radiation. No reference is there made to the possibility of deploying Liouville's theorem in the mechanical case nor to the corresponding "decisive difference" between gas and radiation theory. Instead, the two theories are developed in parallel; equiprobable distributions are in both cases specified by fiat; the justification supplied by natural radiation for the black-body case is supplied by molecular disorder for gases.[16†]

Planck's fourth chapter, "Entropy and Probability," opens by presenting the same paradox with which, in 1897, his famous five-part series "On Irreversible Radiation Processes" had begun:

> Since the electromagnetic field equations together with initial and boundary conditions unequivocally determine the temporal course of an electromagnetic process, considerations which [like probability] lie outside of the field equations would seem unjustified in principle and in any case dispensable. Either, that is, they lead to the same result as the field equations—in which case they are superfluous; or they lead to different results—in which case they are wrong.[17]

To escape from "this apparently ineradicable dilemma," Planck recapitulates the argument he had developed with his electromagnetic H-theorem in 1899. A full electrodynamic treatment of any problem requires the specification, as initial conditions, of the amplitudes and phases of all the Fourier components of the field. Experimental evidence does not, however, permit any specification so full. On the contrary, one finds that almost all possible choices of amplitude and phase lead to the same values for the quantities that can be determined by experiment. Only a minuscule fraction of the possible boundary conditions lead to other results, for example to continuous absorption of incident energy without reradiation or even to negative absorption.[18] Gas theory, Planck emphasizes, presents the same paradox and leads to the same sorts of exceptional cases, cases that would violate thermodynamics.

> As a result, unless one is willing to renounce the attempt to grasp thermodynamics mechanically or electrodynamically, only one possibility remains: the introduction of a special hypothesis which restricts the initial and boundary conditions so that the equations of mechanics or electrodynamics lead to unique results which agree with experiment.[19]

The required special hypothesis (Planck says that it "will entirely fulfill its purpose if it says only that these extraordinary cases . . . do not occur"[20]) is, of course, molecular disorder in mechanics or natural radiation in electrodynamics. If the applicable one is not obeyed, the second law will be violated, and the concepts of entropy and temperature lose their meaning.

All of this is exceedingly familiar. Boltzmann had said very nearly the same things in Volume I of the *Gas Theory*; Planck had said them exactly in papers published in 1898 and 1899. Both men had, however, then been providing the bases for an H-theorem, whereas Planck now aims to provide a basis for combinatorial derivations. Though cryptic, incomplete, and very likely incompletable, the passage in which he makes the attempt leaves no doubt about his intention. Simultaneously, it shows both how far he has come since the late 1890s and how close he remains to the position he had taken at that time.

> Which mechanical or electrodynamic quantities shall now, however, represent the entropy of a state? Obviously [!] the magnitude in question relates somehow to the "probability" of the state. For since elementary disorder and the lack of any control over individual microstates [*der Mangel jeglicher Einzelkontrolle*] is of the essence of entropy, only combinatorial or probabilistic considerations offer the needed entry point for the computation of its magnitude. *Even the hypothesis of elementary disorder is essentially a probabilistic hypothesis*, since from an immense number of equally possible cases it singles out a determinate number and declares these to be nonexistent in nature.[21]

That thought becomes clearer in the following pages, where Planck introduces Boltzmann's relation between the entropy of a physical system and the probability of the corresponding "state." To specify further the latter notion, Planck continues:

> By the "state" of a physical system at a given time, we understand the totality of all those independent magnitudes which uniquely determine the temporal course of processes taking place in the system, insofar as these are subject to measurement.... In the case of a gas consisting of invariable molecules, for example, the state is determined by the law of space and velocity distribution, that is, by the specification of the number of molecules with coordinates and velocity components lying within individual small "intervals" or "regions."... On the other hand [since we are concerned only with quantities accessible to observation], the characterization of a state does not require our providing additional details about the molecules within individual elementary regions. The hypothesis of elementary disorder supplies what is missing and ensures the uniqueness of the temporal process despite the mechanical indeterminacy [of the "initial conditions" supplied by specifying only the number of molecules in each small region].[22]

Like Boltzmann in 1896, but now more clearly, Planck has recognized the difference between the molar and the molecular specification of states. Like Boltzmann, furthermore, he is preserving the second law by prohibiting the occurrence of just those special "ordered" molecular (or resonator-and-field) configurations that would lead to its violation. For Planck, however, prohibiting those configurations has somehow become a means of fixing the relative probability of the states that remain. Under those circumstances, the criterion of an appropriate

choice of equiprobable states can only be that it yield experimentall observed regularities, first and foremost the second law of thermo dynamics: "The decision about which hypothesis [concerning th exclusion of certain specified initial conditions] to prefer can only b made by testing the result to which the hypothesis leads against th experimental theorems of thermodynamics."[23] In short, an hypo thesis governing the distribution of initial conditions within individua "intervals" or "regions" determines combinatorial probability an thus entropy. From the latter follows a unique energy distribution law and experiments designed to check it therefore also test the hypothetica restriction on initial conditions. Developed initially for radiation, tha analysis can be applied to equilibrium in gases as well.

Energy elements and energy discontinuity

As Planck's continuing emphasis on the close parallels between hi theory and Boltzmann's suggests, his view of the radiation problem i still, in the *Lectures* of 1906, fully classical. Though his apparently mor radical understanding of the energy element $h\nu$ remains to be discussed what has already been said rules out any version of a long-standin historiographic tradition. Both in his original derivation papers and far more clearly, in the *Lectures*, Planck's radiation theory is incompatible with the quantization of resonator energy. That theory doe require fixing the size of the small intervals into which the energ continuum is subdivided for purposes of combinatorial computation and the restriction to a fixed size does isolate the main respect in whic Planck's theory diverges from Boltzmann's. But the divergence doe not, as developed by Planck, make radiation theory less classical tha gas theory, for it does not of itself demand that the values of resonato energy be limited to a discrete set. On the contrary, as this chapter ha already shown, any such restriction would conflict both with the globa structure and with multiple details of Planck's argument.

In Planck's theory, resonator emission and absorption are governed in full by Maxwell's equations. Variations of resonator energy with time are determined by the same sorts of differential equations and described by the same sorts of Fourier series that Planck had used for these purposes before 1900. Planck's H-theorem of 1899, presented in the closing chapter of the *Lectures* as its crowning achievement, demands those equations and series too, and it had to be abandoned when Planck gave up continuity after 1906. Thus, though the structure of the energy continuum is fixed by the energy element $h\nu$, the motion of Planck's resonators remains continuous, both within the elements constituting that continuum and from one to the next. With a single misleading exception, to be considered below, nothing in Planck's published papers, known manuscripts, or autobiographical fragments suggest that the idea of restricting resonator energies to a discrete set of values had even occurred to him as a possibility until others forced it upon him during 1906 and the years following.[24] My point is not that Planck doubted the reality of quantization or that he regarded it as a formality to be eliminated during the further development of his theory. Rather, I am claiming that the concept of restricted resonator energy played no role in his thought until after the *Lectures* was written. Could Planck responsibly have remained silent about it in that work if the idea had even crossed his mind as relevant to the theory he there presented?

To this historiographic heresy, Planck's treatment of natural radiation and molecular disorder lends essential support. Those special hypotheses are what, in Planck's theory, restrict the permissible microdistribution of resonators within the energy intervals $h\nu$ and of molecules within the phase-space cells $d\omega$. If Planck had wished to do so, he could have used them to prohibit a resonator or molecule from occupying any part of the interior of these small regions. Resonators, if thus restricted to the endpoints of the intervals into which Planck divides the energy continuum, could only have energies $nh\nu$. But, putting aside the improbability that Planck would have failed to mention a restriction quite so strange and quite so unlike the apparently parallel molecular case, the text of the *Lectures* prohibits any possibility of that sort. In the *Lectures*, after admitting the existence of special initial conditions that must be prohibited because they would lead to unobserved phenomena, like negative absorption, Planck continues:

> If, however, one examines more carefully the infinity of different cases, corresponding to the different possible values of C_n and θ_n compatible with a given [observed] radiation intensity, and if one compares the results of different choices, one finds that a huge majority of such choices lead on the average to corresponding [experimental] results, while those choices which result in noticeable deviations are, by comparison, negligibly small in number.[25]

Planck is again following Boltzmann. Some initial conditions must be prohibited to ensure the validity of the second law, but their number is small compared with that of the admissible initial conditions. Forbidden states are therefore not numerous enough to occupy the

entire interior of a cell, restricting the admissible ones to its surfac
Excluding rare singular cases, Planck's resonators, like Boltzmann
molecules, are to be found anywhere within the small cells or energ
ranges required for combinatorial computations.

How then can anyone have found energy quantization in Planck
early discussions of his black-body law? Part of the answer is that th
first edition of the *Lectures*, still the only unambiguous source of hi
position, was, from 1913, rapidly displaced by a series of better-know
editions in which quantization does play a central role; for over sixt
years Planck's original version has been read only by an occasiona
historian. His first derivation papers—the earliest source of the innovations from which energy quantization arose—have been far mor
widely read. But they are extremely brief and, in some respect
obscure, so that readers aware of what happened next have been abl
to bridge the gap to later versions of black-body theory withou
realizing that it existed. More than obscurity however, is, responsibl
for the ease with which they have done so. Before the first appearanc
of the *Lectures*, Planck's first derivation papers were also misread
though not independently, by two of his contemporaries, men to whon
none of the preceding explanations can apply. Those misreadings hav
another source, and its existence helps also to account for what ha
occurred since Planck's time. Two technical aspects of his presentatio
do suggest energy quantization. The first is easily disposed of and i
not, in any case, likely to have affected his contemporaries.[26] Th
second is deep and will require careful explanation.

Though Planck does not, either in his derivation papers or th
Lectures, ever equate the energy of a single resonator with an integra
multiple of $h\nu$, he does repeatedly write expressions like $U_N = Ph\nu$
with P an integer. In such expressions, however, U_N is the tota
energy of N resonators. Restricting it to integral multiples of $h\nu$ doe
not impose any similar restriction on the energy of an individua
resonator, which may vary continuously. Indeed, Planck's subdivisio
of *total* energy into an integral number of equal finite elements is entirel
modeled on Boltzmann's. Though the size of the element employe
by the latter was not uniquely fixed, it could not continuously approach zero, for it was required to be large enough to contain man
molecules. If quantization is the subdivision of total energy into finit
parts, then Boltzmann is its author.

A second aspect of Planck's presentation raises more basic difficulties. In both his early derivation papers Planck described th problem to be solved en route to his combinatorial form as "the distribution of the P energy elements over the N resonators." Each distribution was for him a "complexion," and he illustrated what that term meant with an example in which 7 elements were attributed to the first resonator, 38 to the second, and so on to a total of 10 resonators and 100 elements. Introducing his combinatorial form next, Planck described it as "the number of all possible complexions."[27] 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. The passages in which these phrases and diagrams occur are the presumptive source of the traditional view, for which, in any case, they provide the only significant evidence.

Fortunately for the consistency of Planck's thought, these passages need not be read literally, and important sections of the *Lectures* show that they should not be. When Planck wrote them in late 1900 and early 1901, he was carefully following Boltzmann's 1877 paper. In that paper, described in Chapter II, Boltzmann had twice illustrated combinatorial derivations by distributing molecules over the subdivided energy continuum. In the first case, the energy of individual molecules was restricted to values $0, \varepsilon, 2\varepsilon, 3\varepsilon, \ldots$; in the second, molecules were described as lying in the range 0 to ε, ε to 2ε, 2ε to 3ε, and so on. Both cases led to the same combinatorial expression and, for large N and P, to the same distribution law. The two appear to be interchangeable, and Planck clearly thought that they were. As a result, he felt justified in simplifying his combinatorial derivation by describing a discrete energy spectrum when the physical situation he had in mind called for a continuum. In his *Lectures* the substitution is explicit.

Planck's presentation in the *Lectures* closely parallels the ones he had provided five years before. The meaning of "complexion" is again illustrated with a diagram that assigns an integral number of energy elements to each resonator. His combinatorial expression is described as providing "the number of individual orderings or complexions compatible with the distribution of the energy U_N over the N resonators." These and other echoes of his original papers strongly suggest that Planck had not in the interim changed his mind about the essentials of his theory. Nevertheless, either because he was taking special care or because he had discovered that his earlier way of putting his point could be misunderstood, he did pause long enough for an essential clarification. A few lines after the phrase just quoted, he spoke of "the number of resonators with energy of a given magnitude" and then added at once, "(better: which lie within a given 'energy region')."[28] Only the omission of some equivalent parenthetical clause from his early papers makes it so difficult to discover what Planck had in mind.

A second passage in the *Lectures* reinforces the point, perhaps definitively. In it Planck shows how to compute "the number of complexions corresponding to a given state directly from the electromagnetic state of an individual resonator rather than from its energy, which is always a compounded quantity."[29] The result—an aside in this first presentation but later of great significance for the development of quantum theory—is a phase-space description of the equiprobable regions accessible to a resonator. To achieve it, Planck first rewrites equation (1-7) for resonator energy in terms of the resonator moment f and its conjugate momentum g:

$$U = \tfrac{1}{2}Kf^2 + \tfrac{1}{2}\frac{g^2}{L}.$$

Curves of constant energy are then readily shown to be ellipses of area U/ν, so that the equiprobable regions previously specified by equal energy increments $h\nu$ become elliptical rings of area h in the phase plane. Even before deriving that result, Planck had indicated the use he would make of it: "We conceive f and g as coordinates of a point in the phase plane [*Zustandsebene*] and inquire about the magnitude of the probability that the energy of a resonator lies *between* the values U and $U + \Delta U$."[30] When ΔU is later set equal to $h\nu$, resonators continue to lie within, not simply on the boundaries between, the elliptical rings thus formed.

These passages are, I suspect, decisive by themselves, but they do pose a puzzle. Why, if Planck conceived his resonators as lying within energy intervals $nh\nu \leqq U < (n+1)h\nu$, did he use a vocabulary that apparently restricted resonator energy to integral multiples of $h\nu$? That question, too, has an answer, one that depends upon an often overlooked difference between Planck's and Boltzmann's methods of determining equiprobable complexions. Both men began by dividing the energy continuum into P elements of size ε. Boltzmann's next step was to distribute molecules at random over the energy continuum, immediately labeling each one with the number specifying the energy on which, or the interval into which, it had fallen. Planck, on the other hand, separates the P individual elements of the divided continuum and distributes them at random over the N resonators. Only after the distribution process has been completed does he label each resonator with the number of elements it has received. Though the two methods are readily shown to be equivalent, they are at first glance clearly distinct.[31]

Planck could, of course, have used Boltzmann's method to derive his combinatorial form, a fact he notes in the *Lectures*. But, as he points out in the same place, the process would involve summing over all Boltzmann distributions compatible with the given energy and would be correspondingly cumbersome. His own method allows him "to go more quickly and conveniently . . . to the same goal"[32] and is therefore to be preferred. For the use of that shortcut, however, Planck paid an unnoticed price. Boltzmann's method of distribution could be used to place molecules either within energy intervals or at the boundaries between them. Planck's method, in the absence of explicit further specification like that given in the *Lectures*, could only leave each resonator with an integral number of the whole energy units, which had been distributed to it one at a time.

The quantum of action and its presumptive source

Despite its generally classical nature, Planck's statistical radiation theory did differ from Boltzmann's gas theory in one central respect. For Boltzmann, the subdivision of the energy continuum was a mathematical device,[33] and the size of the element employed to introduce it did not matter. For Planck, that subdivision was a physical necessity, and the size of the element was fixed by the relation $\varepsilon = h\nu$. Early in his December 1900 lecture to the Physical Society he had described that relation as "the essential point" of his theory.[34] Thereafter, until the appearance of the *Lectures*, he employed it without special comment, and even in 1906 his remarks on that relation were extremely brief. Immediately after introducing the relationship $\varepsilon = h\nu$, Planck continued:

> An immediately striking feature of this result is the entry of a new universal constant h of which the dimensions are a product of energy and time. It marks an essential difference from the expression for the entropy of a gas. In the latter, the magnitude of an elementary region which we call $d\omega$ disappears from the final result since its only effect is on the physically meaningless additive constant. . . . The thermodynamics of radiation will therefore not be brought to an entirely satisfactory conclusion until the full and universal significance of the constant h is understood. I should like to label it the "quantum of action" or the "element of action" because it has the same dimensions as the quantity to which the Principle of Least Action owes its name.[35]

Implicit in that passage is a subtle but extremely important change of emphasis. Though the difference between gas theory and radiation theory is the physical role played in the latter by a particular choice of cell size, what requires explanation is not the necessity for a fixed size but the "significance of the constant h," which determines its magnitude. If only h were understood, then fixed cell size might be seen to follow from it or even to be a misleading interpretation of some more fundamental aspect of radiation phenomena. For Planck, apparently, h has now become the "quantum of action," a phrase that, unlike its suggested alternate, "element of action," is henceforth standard usage in his writings on radiation theory. More important, it is standard also throughout his autobiographical writings, for it is this constant, and not a restriction on resonator energy or on continuous motion, that Planck regularly identifies as the novelty he introduced into physics. The quantum of action proved "cumbersome and refractory," he notes, when confronted by his efforts to assimilate it classically. Ultimately, it called forth "a break with classical physics far more radical than I had initially dreamt of."[36]

Doubtless the brevity of Planck's remarks in the *Lectures* was dictated by this interaction of circumstances with character. He had little to say about the quantum of action, and he was not one to speculate or waste words in scientific papers. But he did believe more needed to be said—"The thermodynamics of radiation will therefore not be brought to an entirely satisfactory state until the full and universal significance of the constant h is understood"—and he thought he knew the area in which work must be done to supply what his theory still lacked. The sentence replaced by ellipsis in the last long quotation reads: "Though contemporary theory offers no point of entry for its exploration, there can scarcely be a doubt that the constant h plays some role in the elementary oscillatory process at the center of emission." Attached to that sentence is a footnote calling attention to an earlier passage in which Planck had emphasized that his formulas for radiated energy apply only when radiation intensity is measured over periods T of sufficient length. "For smaller values of T," he had continued, "the simple linear equation [governing resonator vibration] may possibly need to be replaced by one better suited to natural phenomena."[37]

Those passages suggest that Planck expected, or at least hoped, that the puzzle posed by his theory would be solved by research on the microscopic detail of the emission process, thus by electron theory.

That suggestion is, in turn, fully confirmed by a letter Planck had written to the young physicist Paul Ehrenfest (1880–1933) during the summer before the *Lectures* appeared. Its importance and the use to be made of it in Chapter VI justify its reproduction in full:

> Grunewald, 6. July 1905
>
> Honored Herr Dr!
>
> In response to your valuable letter of the first of this month, I will gladly give you my opinion about the question you have raised. First of all, I agree entirely with your principal point. 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.
>
> But perhaps it is not out of the question to make progress in the following way. If one assumes that resonator oscillations are produced by the motion of electrons, then a new element enters the theory in any case. Because the charge of the electron is proportional to div E, E cannot be increased by m^2 throughout the field unless the charge of the electron grows in the ratio $1:m^2$. Therefore, if the charges of electrons are constant, the process you describe [—] $E' = m^2E$, $H' = m^2H$, $f' = m^2f$ [—] is impossible.
>
> It seems to me not impossible that this assumption (the existence of an elementary quantum of electricity [the charge e]) offers a bridge to the existence of an elementary energetic quantum h, particularly since h has the same dimensions as e^2/c (e, elementary quantity of electricity in electrostatic units; c, velocity of light). But I am in no position to offer a definite opinion in this matter.
>
> Your most humble
> M. Planck[38]

Planck's localization of the problem of the constant h in electron theory, especially his view of its relation to the quantum of electricity e, was immensely plausible. Thiesen had emphasized in 1900 that any theory of black-body radiation would require two constants.[39] One of those used by Planck, the constant k, was related to the choice of a temperature unit and could be understood in terms of the mechanical theory of heat. To anticipate that the source of h would be radiation theory was more than reasonable. The only constant in Maxwell's equations is, however, the velocity of light, and it is dimensionally unsuited to the purpose. What remained was the charge of the recently discovered electron, at the time central to the most active and exciting area of physics. That the quantum of electricity might solve the puzzles of black-body theory was not, in any case, an idea that originated with Planck; indeed, he may well have borrowed it.

As early as 1900 the leading expert on electron theory, H. A. Lorentz, had argued that Kirchhoff's law could only be understood in terms of some property common to all matter. "In all probability," he added, "the likeness in question consists in the equality of the small charged particles or *electrons*, in whose motions modern theory seeks the origin of the vibrations in the aether."[40] The paper in which he announced that idea is also the source of the sorts of dimensional arguments to which the second paragraph of Planck's letter to Ehrenfest refers. (As will be seen in Chapter VI, that letter is part of a correspondence about issues that Ehrenfest was soon to explore in an important paper.) In 1903 Lorentz returned to electron theory to derive the form of Kirchhoff's universal function appropriate to long wavelengths, and in 1905 James Jeans used similar arguments to explain the universal character of the Wien displacement law, an explanation he required since he did not believe that black-body radiation was in an equilibrium state.[41] To these considerations Planck could add one other, the converse of the great achievement he had first announced at the end of 1900. At that time he had computed the value of e from experimental values of h and k. What, then, could be more natural than to explain the conceptual mysteries of h (k was in some sense understood) in terms of the electronic charge to which he had himself demonstrated its astonishing relation?

Electron theory, the field to which Planck was thus relating the problem of h, was characterized by numerous other unsolved problems, and great progress was being made with them. Since Planck himself had not as yet done research in this active area, he could have been forgiven if he had left the problem of explaining the quantum of action to electron theorists. In practice, however, he seems not to have done so. Though he published nothing explicitly concerned with black-body theory between 1901 and the appearance of his *Lectures* in 1906, the main topic of his research in the intervening years was the electromagnetic theory of optical dispersion, on which, from 1902, he produced a number of substantial papers. That topic did involve him for the first time with electron theory, and it seems probable that he took it up hoping that it would provide "a point of entry [to the]. . . elementary oscillatory process at the center of emission" and thus provide information about the constant h. The likelihood of that hypothesis is reinforced by Planck's later insistence that he had been deeply engaged with the problem of the quantum of action throughout the years when it seems otherwise to be virtually absent from both his published work and his correspondence.[42]† In a letter of 1910 to the physical chemist

Walther Nernst (1864–1941), Planck wrote: "I can say without exaggeration that for ten years, without interruption, nothing in physics has so stimulated, agitated, and excited me as these quanta of action."[43]

Needless to say, Planck's hopes for electron theory were frustrated, and in retrospect it is apparent that they could not in principle have been fulfilled within classical physics. There is, as Einstein would point out in a paper to be discussed in Chapter VII, a fundamental error in Planck's derivation. Though the energy continuum or phase space may be subdivided for purposes of computing combinatorial probabilities, the cells employed must be small enough so that varying the position of resonators or molecules within them produces no observable change in the physical state of the system being considered. For Planck's problem, that condition demands $h\nu \ll kT$, and it is not everywhere fulfilled in the frequency and temperature ranges explored by relevant black-body measurements. When it is not fulfilled, Planck's computation of probability takes account of only some of the states (those at or very near cell boundaries) available to his resonators. Noting this fact, Einstein would conclude that Planck's version of radiation theory demands, in effect, a redefinition of probability. Nothing of that sort could have been forthcoming from electron theory, though quantum theory has, in a sense, since supplied it.

Obvious in retrospect, Planck's mistake was, as we shall see in Part Two, everywhere overlooked for some time. Help in understanding its obscurity, particularly to Planck, may be provided by a last return to Boltzmann. The function of the condition $h\nu \ll kT$ is to ensure that the distribution function—Planck's $U(\nu, T)$ or Boltzmann's $f(u, v, w)$—does not vary significantly between neighboring cells. The necessity for such a condition is, however, a key point missed by Boltzmann when he discussed the transition from sums to integrals in his combinatorial paper of 1877.[44] For Boltzmann, as we have seen, that transition was a mathematical step, and its legitimacy did not depend on the physical condition of the gas. At a similar point in his own argument, Planck may once again have been following Boltzmann's lead.

Planck's early readers, 1900–1906[45]

Whatever Planck's own view of the extent to which his theory broke with the classical tradition, his work was not received as radical by most of his early readers. Partly for that reason, however, there were not many of them. Until after 1906, as before 1900, black-body theory

remained an esoteric specialty. Nevertheless, Planck's papers on the subject were known. Reviewed during 1901 and 1902 in the standard British and German abstracting journals, they were there treated simply as further developing the line of research on which he had been reporting since 1895.[46] The only sign, probably without significance, of the recognition of something special about his most recent work is the extra but not unprecedented length of the German abstract of his 1901 article for the *Archives Néerlandaises*, in which values for the atomic constants were derived. Lorentz, too, cited Planck's new law and his first attempt to derive it in the paper, read at the end of 1900, in which he related electron theory to black-body laws. But he there simply coupled Planck's work with Wien's and discussed neither.

During the next five years, references to Planck's theory accumulated slowly in both the British and the German literature, but their character was somewhat different in the two countries. The German references were predominantly reportorial rather than analytic, and most of them appeared in books rather than articles. (There were also a number of articles dealing with the experimental adequacy of Planck's law.) The second volume of the famous *Handbook of Spectroscopy*, published in 1902 by Heinrich Kayser (1853–1904), includes many passages referring to Planck's work in the years 1897–1901. One of them sketches the derivation of the Wien law given in Planck's summary article for the *Annalen* in early 1900, adding that "in a more recent article Planck seeks a firmer foundation for his chosen expression for entropy."[47] Ten pages later, Kayser returns to the matter, reporting that: "For his derivation of the Wien law Planck chose an especially simple form of the entropy function. Since that law was not entirely [*durchweg*] confirmed, he tried a less simple expression and arrived at [his new] radiation formula."[48] That formula Kayser had previously characterized in a sentence that singled out Planck as the man who, "starting from electromagnetic theory, has apparently found the true radiation law."[49]

Two years later, in 1904, Planck's work was again mentioned in a widely circulated book: the two-volume treatise, *Thermodynamics*, by Woldemar Voigt (1850–1919). Most of its closing chapter deals with such standard topics as Kirchhoff's law and the displacement law; distribution laws occupy only a two-page section, which begins by mentioning Wien's attempt and its experimental inadequacy. Then Voigt continues: "By a most noteworthy combination of probability considerations with the theory of the emission of waves by electric resonators, M. Planck has arrived at a formula which satisfies experiment in the entire region that has been [experimentally] investigated."[50] Planck's law is then presented, and the experimental determination of the two constants is discussed. In a thirty-two–page chapter, "Thermodynamics of Radiation," nothing more is said about Planck's work.

During 1905, Planck's law was mentioned also by Albert Einstein (1879–1955) in a famous paper to be discussed in Chapter VII. But he appealed to it simply as the best experimental formula currently available, and he made use of it only at high frequencies where it becomes identical with the Wien law. Finally, a somewhat fuller sketch of Planck's new theory, drawn largely from the recently published *Lectures* was included in the second edition of the standard textbook of optics by Paul Drude (1863–1906). Drude does mention Planck's use of the formulas $S = k \log W$ and $\varepsilon = h\nu$. But it is the total energy U_N, not the energy of individual resonators, that he speaks of as "made up of a kind of atomistic energy elements."[51] Like Planck, too, Drude emphasizes both the significance of the computation of the electronic charge e from radiation measurements and the importance of discovering the still unknown "electro-dynamic significance of the elementary quantum of action h."[52]

The British references to Planck's theory during these early years were no more numerous than the German, but, coming from the country that was still the only center of significant interest in statistical mechanics, they were often more analytic and original. Burbury, in 1902, published a long, sympathetic, and ultimately important study of the techniques that had led Planck by 1899 to an electromagnetic H-theorem. Near the end he noted that Planck had recently, in the face of experimental counterinstances, introduced a new form of the entropy function "*without altering the general theory as developed in the former treatise.*"[53] In the same year Joseph Larmor (1857–1942) briefly indicated the general structure of Planck's new derivation in the article "Radiation," published in the supplementary volumes that transformed the ninth edition of the *Encyclopaedia Britannica* into the tenth.

For Larmor, unlike Burbury, the papers Planck had published from December 1900 did represent "a fresh start," but their novelty was only the use of Boltzmann's combinatorial definition of entropy. After sketching that departure, Larmor went on to say: "Whatever may be thought of the cogency of his [Planck's] argument, especially in view of the fact that his vibrators cannot change the types of the radiation,

the result gains support from the fact that it involves determinations o the absolute physical constants of molecular theory that prove to be o the right order of magnitude."[54] A footnote to that sentence informe readers that "The argument has recently been recast by Larmor, so a to avoid the introduction of vibrators," a presumptive reference to paper, published only in abstract, that he had read to the 1902 meetin of the British Association at Belfast.[55] Thereafter, Larmor, wh appears to have been the first to take Planck's combinatorial derivations seriously, occasionally presented lectures on the subject, includin a talk at Columbia University in 1905. But his first full publicatio was delayed until 1909, by which time his views could have little effec on the manner and rate at which the quantum theory developed.[5 Even then he said nothing to suggest that Planck's theory implie discontinuity, and in the next year he argued that it need not do so since only the ratio U/ν had to be conserved.[57]

After these first references of 1902, there is no mention of Planck' theory in the British literature until 1905, when a remark by Lor Rayleigh (1842–1919) opened a controversy that marked the beginnin of a continuous discussion in print. Some of the papers in the exchang will be considered at greater length in Part Two, but its beginning i relevant here. For some years James Jeans had been developing theory of the transfer of energy between matter and the ether. Hi approach involved the assumption that the equipartition theore could properly be applied to high frequency vibrations in the ether and that assumption was questioned by Rayleigh in a letter to *Natur* during 1905. Assembling arguments against the general applicabilit of equipartition, Rayleigh noted that Planck's work appeared to b both empirically successful and also incompatible with the standar statistical basis of Jeans's approach. "A critical comparison of th two processes would be of interest," he continued, "but not havin succeeded in following Planck's reasoning I am unable to undertak it.... My difficulty is to understand how another process also based o Boltzmann's ideas, can lead to a different result."[58]

Jeans responded at once in a famous critique of Planck's approach Noting, among other shortcomings, Planck's failure to justify th choice of equal energy intervals as equally probable, Jeans especiall emphasized Planck's having stopped short of setting $h = 0$, a relationship he mistakenly believed to be demanded by the principles o statistical mechanics. If only Planck had taken this required step Jeans pointed out, his distribution law would be the same as the on

Jeans himself had recently derived from equipartition. Though he saw clearly the role of the energy element $h\nu$ in Planck's computation, Jeans regarded it as "a small quantity, a sort of indivisible atom of energy, introduced to simplify the calculations."[59] Not until 1910, when he somewhat ingenuously acted as though the discovery were his own, did Jeans suggest that Planck's theory required discontinuities in the classically continuous range of energies available to a physical body or to the radiation field.[60]

Turn, finally, to two anomalous readings of Planck. In 1903, as previously mentioned, Lorentz derived from electron theory the form of the distribution function appropriate to long wavelengths. Before doing so he mentioned Planck's distribution function, which he regarded as "remarkable...[because it] represents very exactly the energy of the radiations for all values of the wavelength, whereas the following considerations are from the outset confined to long wavelengths." Planck's derivation was not described, but Lorentz devoted a paragraph to remarks about it. Among other things, he said:

> I shall not here discuss the way in which the notion of probability is introduced in Planck's theory and which is not the only one that may be chosen. It will suffice to mention an assumption that is made about the quantities of energy that may be gained or lost by the resonators. These quantities are supposed to be made up of a certain number of *finite* portions, whose amount is fixed for every resonator; according to Planck, the energy that is stored up in a resonator cannot increase or diminish by gradual changes, but only by whole "units of energy," as we may call the portions we have just spoken of.[61]

Two years later, Paul Ehrenfest (1880–1933) rephrased the relevant part of that description in a paper to be discussed at length in Chapter VI. His own concern was restricted entirely to Planck's electromagnetic H-theorem as it had been developed through 1899. Like Burbury, however, Ehrenfest closed by mentioning Planck's introduction of a special entropy function, adding that it was based upon a Boltzmann-like combinatorial analysis. Promising to discuss Planck's derivation in a later article, Ehrenfest meanwhile noted the two hypotheses on which, in his view, it depended. The first was, of course, the special choice of equiprobable states, the second "that the radiant energy of the various colors consists of minuscule energy particles of magnitude: $E_\nu = \nu \cdot 6.55 \times 10^{-27}$ erg·sec., where ν is the frequency of the color in question."[62]

Almost certainly these two non-standard readings of Planck's first quantum papers are mutually dependent, for Ehrenfest, whose

paper opens with a reference to Lorentz's black-body publications, had first learned of Planck's work from Lorentz's lectures at Leyden during 1903.[63] Perhaps one or both men recognized that, whatever Planck may have thought, his theory would not work if energy were absorbed and emitted continuously. But, in that case, they would probably have attributed the hypothesis of energy quanta not to Planck himself but to the demands of his theory. Given their phraseology and the universal difficulty in recognizing where Planck's derivation went astray, it seems far more likely that they were simply following Planck's misleading discussion of his way of populating states. Lorentz, in any case, soon recognized that Planck's own theory did not restrict resonator energy. Writing to Wien in 1908, he noted that "according to Planck's theory resonators receive or give up energy to the ether in an entirely continuous manner (without there being any talk of a finite energy quantum)."[64] And in 1913, during a discussion of remarks by Jeans at the Birmingham meeting of the British Association, he began by outlining his own approach to black-body theory and then continued:

> We might now suppose that the exchange of energy between a vibrator and the ether can only take place by finite jumps, no quantity less than a quantum being ever transferred to the medium or taken from it. Something may be said, however, in favor of the opposite hypothesis of a gradual action between the ether and the vibrator, governed by the ordinary law of electromagnetism. Indeed, it has been shown already, *in Planck's first treatment of the subject*, that by simply adhering to those laws, one is led to a relation between the energy of the vibrator and that of the black radiation, of whose validity we have no reason to doubt.[65]

For the elimination of Lorentz's initial misunderstanding, a likely cause is Planck's *Lectures* of 1906. It was, as previously noted, far clearer than his early papers, and, if the three known early reviews provide representative guidance, it was not misunderstood. The earliest of these reviews is the most interesting, for its author, Albert Einstein, had just published a paper demonstrating that Planck's combinatorial form can be derived only by assuming resonator energies restricted to integral multiples of $h\nu$. No hint of that idea however, is contained in his careful and generally laudatory summary of Planck's viewpoint.[66] Instead, he describes Planck's use of Maxwell's equations to develop relations between the energy of a resonator and that of the surrounding field, emphasizes the need to supplement those relations with Boltzmann's combinatorial definition of entropy, and identifies the difference between Boltzmann's and Planck's approaches as the use of an energy element of fixed size by the latter. The other two reviews contain even fewer hints that there has been a break with classical theory. Bryan, writing in *Nature*, wonders only whether Planck's choice of equiprobable energy ranges can be justified.[67] Clemens Schaefer (1878–1968), in the *Physikalische Zeitschrift*, views the key step in Planck's specification of entropy as the introduction of natural radiation.[68] Though attitudes towards the significance of Planck's work had begun to shift when these reviews were written, only two or three people were yet aware of any reason to suppose that a break with classical physics was implied. Planck himself did not publicly acknowledge the need for discontinuity until 1909, and there is no evidence that he had recognized it until the year before.

Part Two

THE EMERGENCE OF THE QUANTUM DISCONTINUITY, 1905–1912