



Rewriting the Quantum “Revolution”

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ABSTRACT

This paper is a critical analysis of the structure of the quantum revolution. I consider the factual question of how, historically and theoretically, the classical gave way to the quantum, and I argue for an answer that shows, contra Thomas Kuhn's influential philosophy of science, that it is the logic, and not the sociology and psychology, of research that correctly explains the classical-to-the-quantum paradigm shift. My approach is based not on archival studies but on a careful reading, in their original historical context, of Max Planck's and Albert Einstein's well-known papers; the burden of my argument, which at points will be outspoken, consists, then, in identifying and removing the impediments that prevent us from reading these papers in themselves. For this task I critically consider both the main, and mutually antagonistic, accounts of the origin of the quantum theory currently available in the literature—namely, the orthodox story, according to which Planck inaugurated the quantum theory in 1900, and that proposed by Thomas Kuhn in Black-Body Theory and Quantum Discontinuity—and I show that both of them are essentially incorrect. Both overlook the scientific status of the probabilistic kinetic theory of heat as of 1900, of which both Planck and Einstein were acutely aware. The orthodox story will be refuted by showing that Planck did not postulate energy discreteness to derive his black-body radiation law in 1900; and Kuhn, though he argued, as I do here, against the orthodoxy, did so on different grounds, and his own alternative is refuted by showing that Planck's black-body radiation formula did not trigger a Kuhnian “crisis” in classical physics. This conceptual housekeeping will serve its purpose by removing the obstacles that make it impossible to analyze Planck's and Einstein's papers in themselves; once this is done, my conclusions follow.

The soundest fact may fail or prevail in the style of its telling.

[Ursulla L. Le Guin, cited in (Becker, 2018)]

For over a century now there has been almost complete consensus among the international community of scholars that the quantum theory started in 1900 when Max Planck introduced in physics the concept of energy quantization to derive the correct black-body radiation law. The following quotations are representative:

1. Max von Laue, in his Memorial Address delivered at Planck's funeral in 1947, at the Albani Church in Göttingen:

On December 14, 1900, [...] before the German Physical Society, [Planck] was able to present the theoretic deduction of the law of radiation. *This was the birthday of the quantum*

theory. This achievement will perpetuate his name forever. (von Laue, 1947, 10, my italics)

2. I. Duck and E. C. G. Sudarshan, in their “100 years of Planck's quantum,” published in 2000:

When in the year 5000 people look back three thousand years to our era, we all hope that they will find some epochal events as myth-making for them as the Trojan War for us. Many events of such lasting significance are to be found among the achievements of our twentieth century scientific revolution in physics. The very first of these revolutionary events—certainly in time and arguably in significance—is Planck's invention of the energy quantum in 1900. (Duck & Sudarshan, 2000, 1)

A survey at the physics library evinces the ubiquity of this view: The statement that Max Planck inaugurated the quantum theory in 1900 is found in textbooks on quantum mechanics,¹ in historical

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¹ (Kramers, 1923, 115), (Bohm, 1951, 18), (Tomonaga, 1962, 1), (Peres, 1993, 3), (Liboff, 2003, 33), (Phillips, 2003, 1), (Griffiths, 2004, 14), (Serway et al., 2005, 1).

works,² in philosophical-foundational works,³ in mathematical-foundational treatises,⁴ in scientific biographies,⁵ and—even more significantly, historically speaking—in the works of founding fathers.⁶

So when Thomas Kuhn adopted the course of maintaining, in his iconoclastic *Black-Body Theory and the Quantum Discontinuity* (Kuhn, 1978), that Max Planck's work in 1900 was instead *classical*, many, if not all, scholars found his thesis incredible. Step-by-step, through painstaking archival study, Kuhn tried to establish continuity between Planck's early works on black-body radiation and the 1900 derivation that turned the tide of history, trying, thereby, to establish the claim that if quantization was not in Planck's thinking before 1900—not, in fact, until after 1908—, then it is difficult to believe that it was *only* in 1900.⁷ Such a conclusion was contrary to what was universally thought to be a firmly-established and well-known and understood historical matter-of-fact, “the point of view taken by physicists and by the historians of science who have written about these events,” as Martin Klein put it in his book review (Klein et al., 1979, 430–431)—that Max Planck introduced the quantum.

Kuhn's “historiographic heresy,” as he described it, awoke the criticism of other historians, who dissented in different ways. The catalogue of conflicting opinions is described in (Badino, 2009; Darrigol, 2001; Gargart, 2002)—positions range from reaffirming the orthodoxy (Jost, 1995) to denying the meaningfulness of the classical-quantum distinction (Badino, 2015; Needell, 1980). At present, the experts disagree. And behind difference in conclusion, according to (Darrigol, 2001, §2), is difference in approach: Darrigol says that Kuhn and himself “have most closely studied Planck's intricate considerations from the beginning of his program in 1894, through the quantum papers of 1900 and 1901, to later elaborations between 1906 and 1914” (Darrigol, 2001, 228); that indeterminists like Needell “have focused on Planck's expressed goals” (ibid); that orthodoxy subscribers “have given more weight to historiographical tradition” and “to the formal structure of Planck's reasonings of 1900 and 1901” (ibid). Martin Klein, scholar of the orthodox story, remarked that his misgivings with Kuhn's *Black-Body Theory* stem from Kuhn's lack of general consistency:

By insisting so strongly on this radical revision of all previous accounts, Kuhn has put himself in a position that requires him to explain away part of the available evidence and to look at the rest of it from the standpoint of his central thesis. (Klein et al., 1979, 431)

In the view of such disagreement it may conceivably be protested that the statement made at the outset of this essay, about the ubiquity of the orthodox story, is outdated, but this is not the case. The controversy is restricted to a small community of historians. Firmly entrenched in the scientific literature, and useful for pedagogical purposes, the standard story naturally lingers, in the lack of a shared and complete alternative to replace it.

² (Rosenfeld, 1936), (d'Abro, 1952, 438, 457–463), (Whittaker, 1954, 81), (Klein, 1961, 32), (Klein, 1963, 81), (Klein, 1985, 12, 217–240), (Waerden, 1967, 1), (Haar, 1967, preface, 3–14), (Peierls, 1965, 199), (Jammer, 1966, 21), (Hermann, 1971, 1, 11), (Kangro, 1976), (Weinberg, 1977, 20), (Mehra & Rechenberg, 1982, 79), (Gamow, 1985, preface, 1, 6–22), (Duck & Sudarshan, 2000), (Nauenberg, 2016, 709–711).

³ (Reichenbach, 1944, v), (Bub, 1974, 2), (van Fraassen, 1991, 377), (Omnès, 1994, 5), (Sklar, 1995, 159), (Beller, 1999, 99, 131, 214).

⁴ (von Neumann, 1932, 5), (Mackey, 1963, 59), (Emch, 1987, 209), (Moretti, 2017, 290).

⁵ (Pais, 1982, 15, 27, 368–378), (Moore, 1989, 3, 118), (Pais, 1993, 80).

⁶ (Bohr, 1913), (Bohr et al., 1924, 787), (Bohr, 1925, 846), (Born, 1938, 7), (Born, 1953, 502), (Sommerfeld & Bopp, 1951, 85), (Schrödinger, 1952, 112), (Heisenberg, 1958, 31), (Dirac, 1963, 46).

⁷ Kuhn contends that “Planck did not publicly acknowledge the need for discontinuity until 1909” (Kuhn, 1978, 140).

But there can be no denying that how, and why, and when, the quantum was introduced in physics, is a *factual* question. The quantum revolution occurred exactly one way. So what the disagreement between the historians does show is that the origin of the quantum theory is *as yet an unresolved historical problem*.

Good. The foregoing discussion has now served its purpose by preparing the reader for the following remarks. Taking stock of just how the different approaches to Planck's work were articulated, and how they have conditioned different accounts of the origin of the quantum theory, it may well be asked what happens if, *unlike* the orthodoxy subscribers (which comprise, let me repeat, most people) we do not take tradition for granted, but *like* them (and unlike the other approaches) we focus attention on Planck's seminal 1900 and 1901 works. The answer is that by working in this way the orthodox story appears so implausible it is very difficult to believe it. But if we reach this conclusion, then we are required to explain away part of the available evidence, as Klein has correctly pointed out. We are required to *undo* the connections between the facts that support the orthodox story, so that they can be connected differently. This burden I have tried to meet here, by taking an approach that is different from any in the existing literature.

History, let us carefully note, is not merely a list of facts. It is a list of *connected* facts. So, if it be admitted that the quantum revolution happened exactly one way, then like a jigsaw puzzle (the analogy serves us remarkably well) the right, complete picture can only emerge when all the relevant facts are found and interlocked the right way. Logical analysis of Planck's 1900–1901 works shall enable us to break the connections supporting the orthodox story so that we can see what are the mistakes, and where are the information gaps. This shall then enable us to look for further facts, the missing puzzle pieces that fit into the spots where we know that there's something missing. And we shall be able to find the answers, and therewith assemble the pieces together—and this is one of the key features of this paper—without detailed archival studies. A careful reading of the well-known papers and books by Planck and Einstein, *while considering factual data about studies in black body radiation, and the status of the probabilistic kinetic theory of heat, as of the beginning of the twentieth century*, shall suffice.

In saying this I am committing a historiographic heresy of my own so I must proceed by making its implications clear.

There are two main points. First, it will turn out that the orthodox story can be refuted *in its own terms, by general considerations, which fact renders the alternative detailed historical accounts superfluous to this task*.⁸ To appreciate what is involved in the second, let us first recall that historians, with a few exceptions, concentrate on meticulous projects on tightly circumscribed topics and periods; after Kuhn's *Black-Body Theory*, “the only major work available with an explicit historical theory of scientific revolutions in the background” (Büttner et al., 2003, 37), historians have adopted the course of criticizing, or developing further, specific results of Kuhn's book, but issues about the structure of the quantum revolution have played little, if any, role in their controversy.⁹ But as a philosopher I am not interested in the historical minutiae of the isolated parts: it is the complete assembled picture, “to grasp the idea of the whole correctly, and thence see the parts in their mutual relations,” as Kant would put it,¹⁰ that matters. And it will turn out that by taking the aforementioned route—this is the second point—an

⁸ Let there be no misunderstanding: This is not to say that the study undertaken here will not conform to the elementary standards of historical scholarship. It is to say that we shall look for no further facts, go into no more details, than such as those that are true and sufficient to a correct theoretical understanding of Planck's and Einstein's seminal works in their original historical context.

⁹ Exceptions are (Büttner et al., 2003) and (Badino, 2009). My account is however completely different from theirs.

¹⁰ Preface to (Kant & Gregor, 1997, 8).

account of the quantum revolution that differs from Kuhn's in all significant aspects will naturally and straightforwardly emerge.

Let me say a few words about why I think this result is relevant for the philosophy of science. Kuhn would probably agree when I say that the question of the origin of the quantum theory transcends historiographic concerns. It affects our understanding of theory-change, of scientific method, and of the specific nature of the quantum hypothesis which was carried over into contemporary physics in the course of the revolution. All of this may have an impact on current research. Now, with all this in the back of our thinking, let us remember that Kuhn's trailblazing *The Structure of the Scientific Revolutions* (1962) is perhaps the most influential work in philosophy in the second half of the twentieth century; Kuhn did not, it is true, use the terminology of the *Structure* in *Black-Body Theory*, but it doesn't take much effort to recognize, in the latter, the explicative pattern of the former. But the best of evidence is of course Kuhn's own words, and in (Kuhn, 1984), Kuhn explicitly remarked that the arguments of *Black-Body Theory* presuppose and reinforce the historiographic/philosophical views of the *Structure*. This is no trivial fact. For, once again, just how the classical gave way to the quantum provides data about the nature of knowledge and the methods employed in its pursuit. And for Kuhn, as is well-known, a choice of paradigm "can never be unequivocally settled by logic and experiment alone" (Kuhn, 1962, 95).

Yet, in this case, it was.

For Kuhn, "the explanation [for scientific progress] must, in the final analysis, be psychological or sociological" (Kuhn, 1970, 21). And perhaps because the *Structure* was so tremendously influential, it became so customary to embrace this view, and connect the facts pertaining to the history of science by muddling together arguments from authority, psychology, and sociology, that some of Kuhn's *Black-Body Theory* critics accused Kuhn of "trying too hard to make the thought of Planck coherent" (see, e.g., (Galison, 1981)). Isn't that the ultimate irony? It seems that it has been generally taken for granted that there cannot be, as a matter of principle, a description of the classical-to-quantum paradigm shift based on the authority of rational argument and experiment alone. But this is not right. The account of the origin of the quantum theory that I put forward here constitutes a direct counter-example.¹¹

One last remark. This paper is self-contained, so that previous acquaintance with the historians' works on Planck (the literature is indeed massive) is not needed. The only demand on the reader is perhaps a tolerance for gear-shifting. The road that leads to the understanding we seek shall not be easy—we have to be prepared for many twists and turns.¹²

So much for vindicating a philosopher's right to critically analyze the structure of the quantum revolution. Let us now come to grips with the subject proper. I start with a brief discussion of the facts that have motivated this study. The relevant places at which my interpretation of the facts differs from Klein's, Kuhn's, and others', will be, in what follows, carefully indicated.

¹¹ I should remark here that since the orthodox story, and Kuhn's alternative to it, make full use of psychological and sociological considerations, the latter will be, on occasion, mentioned in the text—but only insofar as they are needed to show that such considerations are actually irrelevant for a correct understanding of how and why the quantum revolution was set in motion, and, furthermore, that Kuhn and the orthodox story, by giving them undue importance, have created decisive handicaps for a correct understanding of the origin of the quantum theory which have been as misleading in the search for historical truth as pernicious for philosophical judgment.

¹² The strategy adopted here is not unlike that used by W. V. Quine, for example, in his path-breaking "Two Dogmas of Empiricism." More than once we'll take a line of thought, take it to its last consequences, reach conclusions that are manifestly implausible, and start all over, until the final, correct picture emerges.

1. The historical problem suggested by Einstein's critique to Planck

In work done jointly in the late 1850s R. Bunsen and G. Kirchhoff laid down the basis of what they took, correctly, to be a novel and powerful method of chemical analysis: spectroscopy. From investigating solar spectra and the spectral lines of various elements, Kirchhoff observed that the relative intensity of the emission and absorption of spectral lines obeys "a general law, which seems to be of importance in several aspects [...]: it expresses a property of all bodies which is connected with the emission and absorption of heat and light" (Kirchhoff, 1860, 664). This law states that the ratio $\frac{\epsilon_\nu}{\alpha_\nu}$ between the power of emission ϵ_ν and absorption α_ν is a universal—i.e., substance-independent—function of the frequency ν of the radiation and of the temperature T of the substance considered. It reads $\frac{\epsilon_\nu}{\alpha_\nu} = K_\nu(T)$ and it is interpreted as the equation describing, or governing, matter and radiation at equilibrium. The question that then remains is that of determining the function $K_\nu(T)$ theoretically. This is what Max Planck succeeded in doing.

It will be recalled that by definition, a black-body is a body which absorbs all radiation falling on it. Hence, for a black-body, $\alpha_\nu = 1$ in the above formula, and $\epsilon_\nu = K_\nu(T)$. Planck's black-body radiation law is the explicit form of Kirchhoff's function $K_\nu(T)$ for $\alpha_\nu = 1$. It reads

$$\epsilon_\nu = \frac{8\pi}{c^3} h\nu^3 \frac{1}{e^{\frac{h\nu}{kT}} - 1}. \quad (1)$$

In his approach, since the material in the cavity is irrelevant, Planck considered a collection of oscillators (he called them "resonators") in static equilibrium with radiation in an enclosure. For the purposes of this study it is convenient to analyze Planck's derivation of (1) by dividing the premises into the different categories within physics:

Premise 1. (Thermodynamics). The entropy S , the internal energy U , and the temperature T of a system in thermodynamical equilibrium are related by

$$\frac{\partial S}{\partial U} = \frac{1}{T} \quad (2)$$

and also, since U is an extensive quantity, the equilibrium condition enables one to write, for the total energy $E = \epsilon P$ of the system, the average energy per oscillator as

$$U = \frac{E}{N} = \epsilon \frac{P}{N}, \quad (3)$$

with $\epsilon \in \mathbb{R}^+$, $N, P \in \mathbb{N}$.

Premise 2. (Statistical Mechanics). The entropy S depends on the number W of possible states the system can be in as

$$S = k \ln W. \quad (4)$$

Premise 3. (Classical Electrodynamics). An ion oscillating about its equilibrium position emits and absorbs, on average, an equal amount of energy if its resonating frequency ν , its average energy U , and the energy density ϵ_ν of the radiation are related by

$$U = \frac{c^3}{8\pi\nu^2} \epsilon_\nu \quad (5)$$

and Planck's own personal twists, namely

Hypothesis 1. The formula

$$W = \frac{(N+P-1)!}{(N-1)!P!} \quad (6)$$

for distributing P objects of energy ϵ amongst N oscillators, and finally

Hypothesis 2. A formal postulate discretizing the energy of the individual oscillators,

$$\epsilon = h\nu. \quad (7)$$

I must emphasize that Planck did not explicitly pin down and distinguish premises and hypotheses in this way. The next concise proof is therefore not to be found verbatim in his work—but everything just said is there.

Proof. Upon using Stirling’s approximation in (6) and inserting the resulting equation into (3), we have the asymptotic formula

$$S = \frac{k}{N} \ln \left[\frac{N + P^{N+P}}{N^N P^P} \right] \quad (8)$$

for the average entropy per oscillator. Using then (3) to substitute for P into this expression, it follows that

$$S = k \ln \left(1 + \frac{U}{\varepsilon} \right) \left(1 + \frac{U}{\varepsilon} \right) - \frac{U}{\varepsilon} \ln \left(\frac{U}{\varepsilon} \right), \quad (9)$$

whence

$$\frac{\partial S}{\partial U} = \frac{k}{\varepsilon} \ln \left(1 + \frac{\varepsilon}{U} \right). \quad (10)$$

By putting together the latter and (2), we have

$$U = \varepsilon \frac{1}{e^{\frac{\varepsilon}{RT}} - 1}. \quad (11)$$

Inserting (7) into (11) then yields for the quantity U in (5) the desired Eq. (1). $\square$

The date Planck presented his proof to the German Physical Society—namely, December 14, 1900—pins down, it is generally claimed, the birth of the quantum theory.

It follows, therefore, trivially, that

to understand whence quantum theory is to understand Planck’s demonstration of the black-body radiation law.

We shall faithfully follow this statement as a heuristic principle in our study. For if Planck did, or if he did not, introduce the quantum in his 1900 derivation of (1), *then either conclusion must follow straightforwardly from a correct understanding of his 1900 derivation alone.*

Good. To start with, consider the following remark that Albert Einstein made on Planck’s seminal work.

It should not be forgotten that the Planck radiation formula is incompatible with the theoretical foundation from which Planck started out. (Einstein, 1909b, 363)

Einstein’s suggestion is that Planck’s formula and its proof should be considered separately. The partition is purely logical; the black-body radiation law is true, but not the set of premises from which it was inferred. Hence, Planck’s demonstration of (1)—the demonstration, that is, traditionally alleged to have inaugurated the quantum theory—is mistaken; the correct derivation of the black-body radiation law is *not* Planck’s.

Now let us think this over.

If Einstein is wrong, then his contention has only historical import—it need not detain us. But if Einstein is *right*, then the very deeply entrenched tradition of unqualifiedly identifying the origin of the quantum theory with Planck’s derivation of the black-body radiation law suddenly appears incomprehensible. For to say, “Planck’s proof is wrong, it must be replaced,” and to say, “it pins down the birth of quantum theory,” is to say that Planck’s proof is an untrustworthy source of knowledge, and yet it reveals the experimental truth of quantization upon which present-day physics rests.

And this seems uncomfortably *incoherent*.

And since an incorrect proof would hardly be taken seriously by the scientific community, it would surely appear to be a riddle, wrapped in a mystery, inside an enigma that thinkers trained for centuries to reason *with* Newtonian physics convinced themselves that though Planck’s derivation of the black-body radiation law is, as Einstein remarked, wrong, the proof is, at the same time, *so on target*, that it is time-tested classical physics that ought to be abandoned in favor of the new *a priori* quantum hypothesis that the mistaken proof conveyed.

In the view of the strong stress on empiricism, characteristic of the times, it seems utterly incredible that they would.

Many of the historians who have worked on this subject have thought this to be an unjustified concern. Planck’s proof, it is maintained, is “heuristic,” and hence the predicates “consistent” and “inconsistent” cannot be affirmed of it. Evidently, the difficulty, thereby, conveniently disappears. But to say that Planck’s proof is neither right nor wrong, to arbitrarily exempt it from the requirements of exact mathematical demonstrability that proofs are set out to meet, sounds very much like desperation. Why assume, in the search for historical truth, that this is the case? Might it not be that hitherto overlooked facts have the information that will bring coherence to the situation? For Planck clearly intended it as a serious proof; he finished it by cross-checking the values he obtained with it for the constant $k = \frac{R}{N}$ (the molar gas constant divided by Avogadro’s number), with values for Avogadro’s number, Loschmidt’s number, and “the quantum of electricity e ” that were available in the literature *to show how reliable his derivation was*. Here’s Planck, concluding his proof:

If the theory is at all correct, all these relations should be not approximately, but absolutely, valid. The accuracy of the calculated number [for e] is thus essentially the same as that of the relatively worst known, the radiation constant k , and is thus much better than all determinations up to now. To test it by more direct methods should be both an important and a necessary task for further research. (Planck, 1900a, 89–89)

So Planck closes his 1900 presentation of his derivation of the black-body radiation law asking for further independent measurements of N and e to confirm his derivation further still. I don’t see how the man could have possibly been more serious about his work. This notwithstanding, historians have gone as far as to say, to avoid the aforementioned difficulties, that the renowned theoretician Max Planck, in deriving the black-body radiation law, “did not really know what he was doing” ((Darrigol, 2001, 223); see also (Klein et al., 1979, 431) and (Galison, 1981, 84)). Can this really be? Is there really not a better and instructive explanation out there? But we can—and must—go still further with our critical analysis; the statement that Planck’s proof is “heuristic” also involves saying that Einstein’s critique of Planck is unjustified. And since new ideas are invariably judged in the light of prevailing beliefs, which fact inevitably entails that they are invariably greatly resisted at first, the statement that Planck’s proof is “heuristic” also involves saying that the quantum concept was an exception to the rule, and thereby allow it to be an inexplicable miraculous fact that the scientific community was ready to overthrow classical physics, and accept Planck’s suggestion that the right black-body radiation law *can only follow* if energy comes in chunks, without Planck having, well, *proved it*.

All of this strikes me as very implausible, but I will not pursue the argument further. I cannot disprove the historians’ right to call Planck’s proof “heuristic” and Planck’s reasoning, “mad” (Pais, 1982, 372). But I remark that this approach gives up the task of bringing coherence to this historical episode and gives us no new insights. Whereas if we grant Planck his intelligence, and his proof its formal status, we discover implications about the road that paved the way to the quantum revolution that may lead us to a new understanding of this all-too-important historical episode.

So let us make the effort to take seriously what is apparently a neat paradox: it seems that we cannot both maintain the orthodox story of the origin of the quantum, and admit, with Einstein, that Planck’s derivation of the black-body law is wrong, without contradicting ourselves, without being committed to the proposition that the proof is simultaneously right and wrong and so make our statement self-nullifying. Something, we feel, is missing in this picture; something has got to give. But to establish that Planck’s proof is, in this sense, both right and wrong, a historical problem of the very deepest import must be brought to the fore, so I will try to state this problem very precisely; the strategy will be this:

§2.1 to briefly review how outstanding historians have appraised Planck’s 1900 work;

§2.2 to contrast their accounts with how Einstein’s critique of Planck’s work has reverberated in the physics community, and then see what results.

It will take us only a little effort to uncover historical oversights and conceptual entanglements that will *indicate* —but not yet prove—that both the orthodox story of the birth of the quantum theory, and Kuhn’s alternative to it, cannot be squared with the facts. In §2.3, this conclusion will be shown to rest on a more solid basis of historical evidence; in §2.4, a clear and well-explained answer to the question of what, then, Planck did do in 1900 will be given, and the orthodox story, and Kuhn’s alternative to it, then conclusively refuted. In §3, we will put all the pieces of the origin-of-the-quantum-theory jigsaw puzzle together—and, by showing that they fall into place objectively, we will claim to have explained, on the level of historical insight and philosophical analysis, that the structure of the quantum revolution was unequivocally settled by logic and experiment, so that Kuhn was demonstrably wrong in assuming that this is impossible.

2. Thinking through Planck’s derivation of the black-body radiation law, and the factual ground of Einstein’s critique of it

2.1. The review of the standard historiographies is straightforward.

Olivier Darrigol’s excellent, detailed, and very perceptive essay, “On the Historians’ Disagreement Over the Meaning of Planck’s Quantum,” gives us exactly the information we are looking for. Consider his opening remarks:

During the past twenty years, historians have disagreed over the meaning of the quanta which Max Planck introduced in his black-body theory of 1900. The source of this confusion is the publication, in 1978, of Thomas Kuhn’s iconoclastic thesis that Planck did not mean his energy quanta to express a quantum discontinuity (Kuhn, 1978). The aim of the present essay is a comparison of the opinions of various historians on this issue. (Darrigol, 2001, 219)

The reader will now want to recall that in 1918 Max Planck won the Nobel Prize in Physics “in recognition of the services he rendered to the advancement of Physics by his discovery of energy quanta.” This fact alone may lead one to be overhasty, and readily conclude that Kuhn’s claim cannot be right. Otherwise we would apparently have to accept it as a mysterious inexplicable fact that Planck won the Nobel Prize for something he did not do. But, we shall see, the plot is a good bit thicker; nevertheless, it is factually true that, at first, very few were ready to accept Kuhn’s contention. Darrigol systematizes the opposing opinions in a chart reproduced in Table 1,

Table 1
Statements on Planck’s quantum.

Discontinuity	Indetermination	Continuity
Textbooks Klein 1961-7 Hund 1967 Jost 1995	Kangro 1970 Needell 1980, 1988 Galison 1981	Kuhn 1978
Rosenfeld 1936 Jammer 1966	Planck 1920, 1943, 1948 Darrigol 1988, 1992	

which is to be read, says Darrigol, as follows:

According to the first category [left to right], in 1900 Planck introduced the idea that some microphysical entities (his resonators) could only have discrete energy values. According to the second, Planck did not know himself what the precise meaning of the energy quanta was. According to the third, Planck still believed that the energy of his resonators varied continuously, and he had no intention to revolutionize the laws of dynamics. (Darrigol, 2001, 221-222)

A few comments: (1) Darrigol emphasizes that Martin Klein—most prominent and influential expert on Planck, Einstein and Boltzmann—is the “most powerful discontinuist,” and adds that “physics historians have usually agreed with Klein’s view” (223). The first category is, then, by virtue of an unbalanced combination of tradition and Klein’s authority, “the most abundantly represented” and “the typical textbook story of Planck’s discovery belongs to it” (ibid). These considerations make it plain that the question of the interpretation of Planck’s radiation theory is dominated, if not defined, by Klein and Kuhn; this is the reason Darrigol dubs the debate the “Klein–Kuhn controversy” (224). (2) With respect to the second category, let it suffice here to note that Allan Needell argues for “leav[ing] the meaning of the energy quanta in the dark” (223) and that Peter Galison “adopts a similar stance” (ibid). (3) The bottom boxes are meant to indicate an intermediate or nuanced or (in the case of Planck’s own writing) unclear position.

And the fact that Planck himself is not put in the “discontinuity” box is, in and of itself, undoubtedly suggestive.

A couple of illustrations may now be instructive. Max Jammer writes that Planck was “not yet quite sure” whether or not his energy elements “expressed a fundamental innovation of profound physical significance” (Jammer, 1966, 22) so his position is best catalogued between “discontinuity” and “indetermination.” Darrigol places his own work between “indetermination” and “continuity,” with the remark: “whereas Kuhn made Planck a persistent classical physicist, I believe with Needell that Planck left a non-classical behavior of his resonators open” (224). (4) Darrigol’s analysis omits the work of Helge Kragh. Thinking this is a lack serious enough to remedy, I devote a few lines to correcting it. In 1999 (two years before Darrigol’s essay came out) Kragh published an outstanding historical study, “Quantum Generations,” in the preface to which he speaks of “Planck’s introduction of the quantum discontinuity in 1900” (Kragh, 2000, xi). Therefrom, one can only infer that Kragh subscribes to orthodoxy. But later it appears that this was a slip of the pen: In chapter five, §1, Kragh writes that “Planck did not really understand the energy quanta as a quantization of energy” and that he “did not recognize that the new radiation law necessitated a break with classical physics” (62). A year later Kragh published a short commemorative essay for the centenary of Planck’s discovery, “Max Planck: The Reluctant Revolutionary,” wherein he repeats the latter remarks from the book. So doubtless Kragh is in agreement with Kuhn. But it is noteworthy that Kragh does not make any reference to Kuhn, Klein, or any other historian, for that matter, in these works. In the very brief account that Kragh gives of Planck’s discovery, the thesis that Planck did not introduce quantization in physics is *asserted*, disregarding the fact that the literature reflects considerable disagreement among prominent scholars in this respect, and leaving the question of how it was, then, that discontinuity historically came about, undecided.¹³

These considerations justify the conclusion, already stated above, that the story of the birth of the quantum theory—as the development of the theory itself—is not yet a closed file.

2.2. We now turn to Einstein’s critique of Planck’s derivation.

The first thing to be asked here is how the physics community reacted to Einstein’s contention that Planck’s proof is wrong. And the first indication that something is amiss is given by Einstein’s 1949 remark: “Planck actually did find a derivation,” he wrote in his *Autobiographical Notes*, “the imperfections of which remained at first hidden” (Einstein,

¹³ More recently, M. Badino has argued for a view that in Darrigol’s schema is best characterized between “discontinuity” and “indetermination” (Badino, 2015). Reviews of Kuhn’s book are (Galison, 1981; Klein et al., 1979; Needell, 1980). Other reviews of the historians’ controversy besides Darrigol’s (whose treatment nevertheless rely on it) are, to repeat, (Gargart, 2002) and (Badino, 2009, §1).

1949, 39). Considering, now, the forty-year lapse between the statement expressing Einstein's critique (see above) and this later one, it may appear that Einstein thought that though he expressed his point clearly, widespread recognition came only slowly. But there are reasons to think that such an account of the facts cannot possibly be right.

Even though Planck's work on black-body radiation was met with criticism by experts in statistical mechanics such as P. Ehrenfest, J. Jeans, and H. A. Lorentz, the assertion that Planck's formula and its proof are logically incompatible was repeated, insofar as I could verify, by no other physicist. It may conceivably be protested that it appears quite incredible that such a severe critique, by an authority like Einstein, would have gone unremarked. True—but this is just the point. There is manifestly a question here, a question of how this can be, that suggests that a non-trivial consideration related to Planck's work was largely ignored by the scientific literature, a non-trivial consideration that seems *prima facie* incompatible with the over-100-years-old orthodox story of the origin of the quantum, and we should be very unscientific indeed not to look beneath it to make sure that we have not been hitherto overlooking important matters-of-fact in our search for historical truth.

So we raise the simple question as to what Einstein meant. The most straightforward method of demonstration will be to analyze the logical status that Planck's 1900 proof, presented in §1, can legitimately be claimed to have *vis-à-vis* another contention of Einstein's, a contention he formulated, though not in so many words himself, in a series of works from (Einstein, 1905a) onward, and which we, for simplicity and future reference, indicate here in terms of a

Proposition (UVC). Planck's 1-3 premises imply another black-body radiation law—not Planck's.

Proof. According to the equipartition theorem of statistical mechanics, an oscillating ion interacting with gas molecules must have average energy

$$U = kT. \quad (12)$$

Upon inserting (12) in (5), one readily concludes: mechanics and electrodynamics imply that dynamical equilibrium between matter and radiation in an enclosure holds if, and only if,

$$\epsilon_v = \frac{8\pi}{c^3} v^2 kT. \quad \square \quad (13)$$

This formula was first derived by Rayleigh in 1900. It expresses the fact Ehrenfest dubbed, in his (Ehrenfest, 1911), the “ultraviolet catastrophe.” Observe: (13) leads to an infinite concentration of energy obtained from ϵ_v by integration of v over $\mathbb{R}^+$. This contradicts experience. Therefore, either premise (5), required by electromagnetic theory, or (12), required by mechanics, or both, are false.

“Objection!”, the reader may now shout. “It is precisely to fix this problem that Planck introduced the quantum!” This, it is true, is the standard version of the standard story of the origin of the quantum, which has, please note, very ancient roots—here, for example, is the influential H. Poincaré, remarking in 1913:

The old theories which seemed until recently able to account for all known phenomena have suddenly met with an unexpected check. Some modification has been seen to be necessary. A hypothesis has been suggested by M. Planck [...]. ((Poincaré, 1913), cited in (Jeans, 1914, 90))

The problem is that things cannot possibly have happened this way.

Was Planck aware of the radiation distribution law which Rayleigh had derived as a necessary consequence of classical physics? Most authors answer this question in the affirmative and describe Planck's introduction of quanta as his response to the challenge of the “crisis”

brought about by the disagreement between classical theory and experimental results and by the internal failure of classical theory as expressed in the “ultraviolet catastrophe.” As a matter of fact, there was no such crisis [prior to 1900], or perhaps one should say there was no awareness of such a crisis. All of [Planck's] work on black-body radiation prior to the summer of 1900 was done without benefit of the knowledge of just what classical physics did imply for this problem. (Klein, 1961, 461; Klein's italics)

We should hear warning bells about the orthodox story already.

It is noteworthy that Klein, “the most powerful discontinuist,” claims that the quantum revolution *preceded* the crisis traditionally assumed to have triggered it. This nontrivial amendment to the orthodox story did not, insofar as I could verify, reach the textbooks. This is small wonder. For if Klein is right, then we no longer know how to talk about the quantum revolution.

If Klein is right, then we can no longer see *why* the quantum was introduced. The fact that the crisis in classical physics hit the physics community only later—for Planck himself explicitly stated, in his 1914 *The Theory of Heat Radiation*, that “the logical reason for the quantum theory is found in the fact that the Rayleigh–Jeans radiation formula does not agree with experiment” (Planck, 1914, 265)—only pushes the mystery a stage further back. And the remark that Planck, whose authority on radiation thermodynamics was acknowledged by everyone, was not cognizant of the implications of classical physics (which was, let us remember, all there was to physics) is simply crazy. Klein's explanation—that Planck did not know what he was doing—is far too easy, and it is the one that we cannot afford to accept.

And so the plot thickens. The task of defining a problem is logically prior to that of finding out its solution—science, after all, *begins* with problems. Yet most, if not all, of the historians engaged in the “did Planck quantize or didn't he quantize” question (see §2.1) do not bring into focus the specific nature of the problem that the quantum was introduced to solve. The gap seems to be due to a difficulty in making sense of Klein's amendment: the historical record shows unequivocally, on the one hand, that the quantum was accepted as a way out of the ultraviolet catastrophe, and, on the other, that the ultraviolet catastrophe crisis hit the physics community only after the quantum had already been introduced. To the historian, the only solution to this predicament has seemed to be to deny that the quantum was introduced to solve the ultraviolet catastrophe; and therewith two possibilities. Either the quantum was introduced to solve a different problem than that for which it was accepted as a solution, or it was introduced to solve no problem at all.

Both options sound, of course, incredible. And since our present task is precisely to understand the implications of UVC, we are now required to look beneath these facts to see if they indeed force us to adopt one of them.

They don't, as we shall see. That an ambiguous question is bound to lead to a controversy, no one will deny; we have, then, a better chance of doing something more significant rather than merely adding another story to the Klein–Kuhn controversy if we be quite clear about the work that the quantum was introduced to do. This will take us some time to set up, however. I hope the reader will bear with me through a lengthy scene-setting.

To start with, let's consider the Klein–Kuhn controversy again, but now we take a different angle from that of Darrigol. The fact that it was Kuhn, of all people, who first produced a historical account of the origin of the quantum to rival Klein's was by no means a coincidence. For Kuhn published, let us remember, *The Structure of Scientific Revolutions* in the year immediately following Klein's study, 1962. The origin of the quantum is a scientific revolution *per excellence*; Klein was the most authoritative historian on Planck, Boltzmann, and Einstein, at the time that the *Structure* came out, and if Klein was right that Planck, concerned about no crisis whatsoever, introduced the quantum in 1900,

then Kuhn would have to admit that the fundamental thesis of the *Structure* was factually false. And so it would have been very natural for Kuhn to assume that the standard story *had* to be challenged.

And so we are driven to ask whether the facts in (Kuhn, 1978) are not analyzed and organized to fit a certain preconceived pattern. It will appear, as we have remarked upon in §1, that they are. Recall that historical evidence tells us that the ultraviolet catastrophe hit the scientific community only after 1908, and that the quantum was already around at that time; given facts such as these, if one wanted to establish that scientific development follows, indeed, the pattern of the *Structure*, then one would be required to show, first, that the crisis in classical physics, and the quantum revolution, both happened before 1908, and consequently, that the intractable anomaly plaguing classical physics was *not* the ultraviolet catastrophe, but something else. And that is exactly what Kuhn argues for in *Black-Body Theory*.

Here's Kuhn, in his 1984 "Revisiting Planck":

I have generally been well satisfied by the extent to which my narrative fit the developmental schema that *Structure* provides. *Black-Body Theory* is no exception. The start of the revolution that produced the old quantum theory is moved from the end of 1900 to 1906 [in *Black-Body Theory*]; most of the book deals with the period before the revolution occurred. The preceding crisis [i.e. the crisis that preceded the 1906 revolution], to the extent that there was one, resulted from the difficulties in reconciling Planck's derivation with the tenets of classical physics. (Kuhn, 1984, 245)

Readers who know their *Structure* will immediately see a problem in the latter remark: according to the *Structure*, a single anomaly is no crisis-maker. But Kuhn's inconsistency is not the real rub here. The problem, as (Büttner et al., 2003) correctly noted, is that there is no factual evidence whatever that Planck's derivation triggered a 1900–1905 crisis.

But then how in the world, we must now ask, can Kuhn seriously defend that it did? It is clear from Kuhn's writing that he was assuming, with Klein, that Rayleigh derived (13), as Klein had put it, "as a necessary a consequence of classical physics." And it is undeniable that, if this is right, then Planck's 1900 derivation cannot be made fit into the Newtonian straightjacket; a tension, then, seems to have been *bound* to have ensued, triggering a 1900–1905 crisis.

And therefore it would seem that if all of this were indeed right, then the crisis defined by Kuhn *should* have ensued. But that it didn't is simply a matter of fact. And so we have enough reason to suspect that the premise from which Kuhn derived this conclusion is not right.

Let's think about this carefully. It is noteworthy that both Klein and Kuhn, controversy notwithstanding, take for granted that (13) is a logical consequence of classical physics. And so it is comprehensible enough that further scholarly work in the field also took this as a given. And so, since historians could not find a logical explanation for the fact that there was "no awareness" of the ultraviolet catastrophe in 1900, they explained it in cognitive (Badino, 2015; Büttner et al., 2003) and sociological (Kuhn, 1978; Staley, 2005) terms. Indeed, if we think about it, we will quickly see that it was the assumption that (13) is "inevitable" that led many scholars to claim that Planck "did not know what he was doing" when he derived (1), that Planck "did not know the equipartition theorem," for, otherwise, "he surely would have considered its consequences" (Hermann, 1971, 3)(Klein, 1961, 474). I maintain that this assumption is factually false. (13) was *not* a necessary consequence of classical physics. Not in 1900. But let there be no misunderstanding: when I say this, I do not mean there was no "awareness" of it in 1900. I mean it *literally*. I mean that the *content* of classical physics was *redefined* between 1900 and 1911. And so there is a logical explanation for this matter, and the facts that lead to it are not far to seek.

First recall that between 1896 and 1900, the correct black-body radiation formula was universally believed to be Wien's:

$$\epsilon_\nu = \alpha \nu^3 e^{-\frac{h\nu}{T}}. \quad (14)$$

This fact alone should enable us to dismiss Kuhn's contention that a crisis emerged in classical physics because classically only (13) can follow; consider how Planck opened his seminal (Planck, 1901):

The recent spectral measurements of O. Lummer and E. Pringsheim and even more striking those of H. Rubens and F. Kurlbaum, both confirming more recent results obtained by H. Beckmann, would discover that the law of the energy distribution in the normal spectrum first stated by W. Wien from the molecular-kinetic consideration and later by me from the theory of electromagnetic radiation is not universally correct. (Planck, 1901, 553, my italics)

Wien's law carried no crisis in its train, and Wien and Planck both derived it using different models (this fact shall be important for us later on).¹⁴ And the reader should note in this connection that it was in the course of *this* derivation (Planck, 1899, §23–25) that Planck introduced the constants later defined as k and h ($\alpha = 6.10 \times 10^{-56} \cong \frac{8\pi}{c^3} h, \beta = 4.866 \times 10^{11} = \frac{h}{k}$); it was, indeed, after deriving *Wien's law*, in (Planck, 1899, §26), with *these* constants, that Planck proposed his famous system of natural units. And since it will be admitted that no quantum hypothesis was involved in Planck's 1899 derivation of Wien's law, it must be further admitted that the appearance of the constant h , in and of itself, does not imply the quantization of anything.

Good. To continue, then, with our story: it was only when, and only *because*, different groups of experimentalists found, in late 1899 and early 1900, that Wien's law fails in the far infrared that its epistemic status changed from an absolute to an approximate law.¹⁵ Motivated, then, by this fact, Lord Rayleigh—a fierce defender of Boltzmann's theory—proposed, in June 1900, his famous (13) in the often-quoted but otherwise little known "Remarks Upon the Law of Complete Radiation." In this paper, Rayleigh says explicitly that he is perfectly aware of "the difficulties which attend to the Boltzmann–Maxwell doctrine of the partition of energy," and that though "the doctrine fails in general" (!), it may nevertheless be possible to apply it "to the graver modes" of radiation (long wavelengths). And he hastens to add—mind you—that *not theory*, but *the evidence of experiments*, suggested this possibility out to him. For H. Rubens, experimentalist at the *Physikalisch Technische Reichsanstalt*, had reported that for temperatures above 1000 degrees, there is but little increase of radiation. And so Rayleigh, "difficulties which attend to the Boltzmann–Maxwell doctrine of the partition of energy" notwithstanding, proposed (13) as an *effective law possibly valid only* for "the graver modes" of radiation. And so it cannot be denied that it does seem to be a sort of *fact* that in no way, shape, or form, (13) was, as a *general law*, part of classical physics as of 1900. The specifics of Rayleigh's derivation of (13), and of Wien's and Planck's derivations of (14), both testify to this fact.

And so if (13) became a necessary consequence of classical physics years later, as we know that it did,¹⁶ then it cannot *be* that physicists were making the wrong assumptions before. If, years later, (13) was universally accepted as *the* classical prediction, then later—and not before—the principles of classical physics forced physicists to this conclusion.

Let's see how this can be. What is behind the statement that classically only (13) can follow is the fact, remarked e.g. by Einstein in (Einstein, 1909b, 186, footnote 3), that the equipartition theorem is an inevitable consequence of the kinetic theory of heat. And that it is,

¹⁴ See (Wien, 1896) and (Planck, 1899).

¹⁵ One may ask whether this failure triggered a crisis in radiation thermodynamics; it did not. In Paul Drude's *The Theory of Optics*, the most authoritative book of optics as of 1900, the deviations are remarked upon in a footnote (Drude, 1900, 526).

¹⁶ Recall the quote from Planck's *The Theory of Heat Radiation* given a few pages back. Consider also the remark about the undisputed validity of the Rayleigh–Jeans law as a general law made at the invitation letter for the 1911 Solvay Conference in Physics quoted in footnote 34.

is surely undisputable. But it must be carefully noted and appreciated that just like today, no one will contend that it ineluctably follows from contemporary physics that we live in a 11-dimension universe, for this fact is entailed by string theory, the validity of which has not yet been established, in a similar way, as of 1900, *nobody* contended that the equipartition theorem ineluctably follows from classical physics, for, at that time, the validity of the kinetic theory of heat had not yet been established. And that is that.

Mind you, everybody knew about the equipartition theorem.¹⁷ I find it impossible to believe, with many historians, that Planck, as of 1900, did not know about it. And everybody knew, just as well, that it “fails in general,” as Rayleigh remarked. The equipartition theorem implied wrong values for the specific heat of gases and it obviously implied the wrong spectrum for black-body radiation, just as Newton’s laws implied a wrong value for the perihelion of Mercury. And nobody knew what to do about the equipartition theorem, just as nobody knew what to do about the 43 arc-seconds per century residual discrepancy in the precession of the perihelion of Mercury. But, unlike the crisis in Newtonian gravity, *the crisis enforced by the equipartition theorem waited in abeyance until after 1908*. Not because of human confusion or sociological forces, as historians have made it appear, but because of evidence—or lack thereof—from experiments;¹⁸ for the probabilistic kinetic theory of heat earned its place in physics *only after indisputable empirical evidence in favor of its most fundamental postulate*, namely, the Gaussian distribution of displacement over time and velocity of discrete granular particles, *came from Perrin’s experiments after 1908*. That this is so was demonstrated recently with irrefutable consistency by Smith & Seth in their masterful *Brownian Motion and Molecular Reality: A Study in Theory-Mediated Measurement* (Smith & Seth, 2020). They make a very strong case to the effect of showing that it was the authority of evidence, stemming from Perrin’s 1908–1911 experiments, that gave the probabilistic kinetic theory of heat its scientific standing.

And therewith—I hasten to add—a crisis. Reader, please note: this is why, and when, and how, the ultraviolet catastrophe hit physics. The equipartition theorem comes along with the probabilistic kinetic theory of heat.¹⁹ Carrying in its train all of its intractable consequences.²⁰

¹⁷ I am indebted to Don Howard for calling my attention to this fact.

¹⁸ Kuhn, it must be remembered, defines “crisis” in psychological terms: According to the *Structure*, a crisis arises when the *confidence* of scientists is lost in the ability of the paradigm to solve worrying puzzles he calls “anomalies.” But the discrepancy in the perihelion of Mercury never shook the confidence of physicists in Newtonian gravity one iota; so, when I say that orbital astronomy was in a state of “crisis” due to the discrepancy in the perihelion of Mercury, I mean it objectively, in the sense that physicists were unable to find a robust source for the 43 arc-seconds per century residual discrepancy. When I speak of the ultraviolet catastrophe crisis, I refer to the fact that, theoretically, (13) could not but be true, and yet, it is evidently empirically false. I shall always use the term “crisis” in this objective sense here. (The fundamental difference between my use of the words and Kuhn’s technical terminology will be indicated, when necessary, by the use scare quotes around the relevant terms.)

¹⁹ I should emphasize that Perrin’s granule-rotation experiment provided a theory-mediated measured for N that was meant to test the equipartition assumption *specifically*; in that case, N represented the number of granules required to have their total rotational kinetic energy matching their mean translational energy, kT , as required by the equipartition theorem (see (Smith & Seth, 2020, §4.7), for a detailed discussion). I quote Perrin’s conclusion without comment:

The agreement is remarkable, if one thinks of the difficulties of measurement and the complete uncertainty which a priori surrounded even the order of magnitude of the rotation. The granules utilized for these measurements were about 100,000 times heavier than the granules of gamboge first studied. So, *the equipartition of energy is established throughout this great interval*. Incidentally, its verification for the rotations is an experimental confirmation of the reasoning from the kinetic theory which

This is my understanding of why the crisis manifest in the ultraviolet catastrophe established itself in physics only after 1908, as historical record shows that it did.²¹ If I am right, then the psychological, sociological, and cognitive reasons heretofore adduced by historians are only of secondary importance.

And now it cannot be denied that all these considerations have a great deal of powerful cumulative *suggestiveness*.

For what Perrin did in 1908–1911 (or this, at least, is one way of putting it) is to confirm Einstein’s 1905 predictions for the Gaussian character of Brownian motion.²² The Einstein who, from the same year on, kept on insisting, in a series of papers, (Einstein, 1905a, 1905b, 1906, 1907, 1909a, 1909b), that current theories of radiation and matter *inexorably entail* (13), which fact shows (he claimed) that the foundations of physics must be considered afresh. The same Einstein who contended, as we have seen, in 1909, that Planck’s 1900 proof is *incorrect*, and who stated the content of UVC, which, at last, we seem to be in possession of the relevant facts to properly analyze.

It is easy to see that UVC trivially entails that the conclusion of Planck’s 1900 derivation is “incompatible with the theoretical foundations from which Planck started out.” For if premises 1–3 entail the Rayleigh–Jeans law, while the whole set of premises entail the incompatible Planck’s law, then they cannot but be logically inconsistent.

But then how in the world could Planck have managed to get his minutely-confirmed-by-experiment formula out of his derivation?

Forget about the equipartition theorem for a moment. Let’s face Planck’s 1900 derivation head-on. Consider Eq. (5) again. Planck derived it following Maxwell’s equations, assuming that the energy of the resonators varies continuously. But the assumption that energy is partitioned among the N oscillators in a way that it can only vary by jumps of (7) contradicts the premise of electrodynamics that radiation energy is spread throughout the whole of space, and the basic premise

has enabled the ratio C/c of the specific heats of gases to be predicted. ((Perrin, 1910), cited in (Smith & Seth, 2020, 159))

²⁰ In the force of this conclusion we find a natural explanation for the otherwise inexplicable fact that the term “classical physics” we now take for granted was christened, as Richard Staley has shown in his well-known (Staley, 2005), at the 1911 Solvay Conference. For it must be remembered that this conference was designed by Walther Nernst “to expand the ‘circle of belief’ in the necessity of reformulation of the kinetic theory” (Barkan, 2011, 189, my italics); this, and Smith and Seth’s scholarly (Smith & Seth, 2020), expose a decisive component of the origin of the quantum theory which up to now had been hidden from our view: *It was the probabilistic kinetic theory of heat that shaped what “classical” meant in the beginning of the twentieth century*. Staley’s otherwise excellent analysis of what counted as “classical” in *fin de siècle* physics overlooks the indispensable factor of the complex relationship between theory and evidence that Smith and Seth have discussed in their (Smith & Seth, 2020) with irreproachable rigor; the probabilistic kinetic theory of heat, let me repeat, for vividness, this important point once again, was *not* considered part and parcel of established science as of the end of the nineteenth century (see footnote 29, below), and it must be acknowledged that without an *experimentum crucis* of the kind Einstein proposed (Einstein, 1905b) and Perrin performed (Perrin, 1908a, 1908b, 1908c, 1910) there would hardly have been an scientific warrant, any reason at all, to allow for the probabilistic kinetic theory of heat to rise to the ranks of the “classical” so that the collective consciousness of a “crisis” in “classical” physics, “a conscious need for a reform” (Planck), could emerge as a direct consequence of the “destructive simplicity” of its “inevitable” consequence (Lord Kelvin): the equipartition theorem.

²¹ See, e.g., (Staley, 2005) and (Barkan, 2011, chapter 10).

²² The fact Einstein writes as to makes clear in (Einstein, 1905b) that he was proposing nothing less than an *experimentum crucis* for the probabilistic kinetic theory of heat cannot be overemphasized: “If the prediction of this motion were to be proved wrong”, he insisted, “this fact would provide a weighty argument against the molecular-kinetic conception of heat” (Einstein, 1905b, 123).

of mechanics according to which the states of ponderable matter vary continuously. Hence, to bring (5) and (7) together is equivalent to bringing P and $\neg P$ together. It is a flat-out contradiction. And from a contradiction, *anything* can be shown. Trivially, therefore, the right black-body radiation law.

2.3. But it seems that there is something not altogether kosher about this conclusion.

Remember that what led us into this discussion of Einstein's contention that Planck's derivation of (1) is incorrect was its *prima facie* incompatibility with the standard story of the birth of the quantum theory. But our discussion so far has served only to confirm that the standard story leads to notoriously paradoxical consequences, without in any way removing the paradox. From showing that a matter is implausible, to proving that it is false, is a long step, and nothing in our investigation so far enables us to conclude that Planck did not introduce the quantum in 1900.

So let us see what there is to the above analysis of Planck's proof that gives it its air of sheer implausibility. If it is true indeed that Planck's 1900 derivation of (1) makes assertions that oppose each other, then there can be no difference in saying that the proof did, and that it did not, introduce the quantum. The question as to whether the orthodox story is right or wrong would therefore be *unanswerable* on the basis of Planck's proof alone. But then, what are we so hard at work on right now?

There *must* be a catch here, and to find it, let's try a different strategy. Perhaps the best way to test the contention that the orthodox story is false is to examine the cases in which it seems to be confuted. One can ask: How can we say, indeed, that Planck did *not* introduce discontinuity in physics, if he won the Nobel Prize in 1918 for the discovery of energy quanta? How could Planck have arrived at formulas (6) and (7), which, according to our present conception, encode particle indistinguishability and energy quantization respectively, if he did *not* willingly break with tradition? Both Klein and Kuhn have argued that Planck did it by mistake—once again, on the grounds that he “did not know what he was doing”; we have already rejected this possibility. But the alternative would be that Planck found these formulas by consistently thinking *with* classical physics, and it is unclear how this can be. Finally, it is quite difficult to believe that Planck contradicted himself *unwittingly*, as Einstein appears to have suggested, and many historians defended. For how in the world could Planck, renowned theoretician, end his seminal 1900 presentation of his proof of (1) calling for more independent tests of the values of N and e *in order to confirm the absolute validity of his derivation*, not noting that his proof was, actually, so off-target, that it fails to conform to the conditions under which alone the proof could be said to be *significant*? It is vital for us to give satisfactory answers to all these questions. If we are successful in this, then shall have shown that the orthodox story is false. The missing pieces of our origin-of-the-quantum jigsaw-puzzle will be found by discussing critically the facts described next in four short steps.

1. The first thing to note is that Planck did not arrive at (1) by reasoning from premises to conclusion. Things were, rather, just the other way around. Planck started with Wien's (14) (which, remember, was shown to fail in the far infra-red in late 1899 and early 1900), while appealing to careful experimental measurements; Planck reported, years later, that H. Rubens paid him a visit in October 7, 1900 to tell him that he had determined ϵ_ν to be proportional to T for small ν (the same experimental fact that led Lord Rayleigh to propose (13) as an effective law in June 1900, as we have seen). Interpolating, then, between these two regimes, and following general theoretical considerations that had enabled him to derive Wien's law in 1899 (we shall come back to this point later on), Planck was able to write down an expression that rendered measurements exceptionally well,

$$\epsilon_\nu = \frac{a\nu^3}{e^{\frac{b\nu}{T}} - 1}, \quad (15)$$

with α and β , of course, as in (14). This formula Planck reported publicly on October 19 (recall: the birth of quantum theory is said to be marked by the two-month-after presentation of the *derivation* of the formula); decades later, in his reminiscences, Planck wrote that the very next morning (October 20) he received a visit from Rubens:

He came to tell me that after the conclusion of the meeting he had that very night checked my formula against the results of his measurements and found a satisfactory concordance at every point... Later measurements, too, confirmed my radiation formula again and again—the finer the methods of measurement used, the more accurate the formula was found to be. (Planck, 1949, 40–41)

The reader should, then, take Eq. (1) for what it was in October 1900, namely, Eq. (15). In other words, a *guess* confirmed by experiment *in need of theoretical justification*.

But wait a moment. A quantum-theoretical formula cannot be derived from classical premises. And since it is a plain fact that there was no ultraviolet catastrophe to worry about as of 1900, no reason to think that the right black-body radiation law could not be found by following the all-encompassing, time-tested classical physics, it will now seem that if Planck had reasoned *deductively*, then he could not have found (1). Nothing else but a prior empirical commitment, namely (15), could, it now seems, have enabled Planck to derive the right black-body radiation law.

So just what is told, then, in textbooks about the birth of quantum theory—namely that Planck's law first appeared as a derived formula in December 1900—sounds simply nonsensical. Not only *was* Planck's black-body radiation law a guess. *It could only have been* a guess.

2. The second point is that there is a sense in which the guess was an inductive guide for Planck. This fact is what may seem to bring out, or justify, the assumption, shared by so many historians, that the problem of the origin of the quantum theory is resolved in cognitive terms, i.e., in Planck's way of thinking—from what we have already said it should be clear that this assumption is unjustified. But to make our own case, we cannot afford to overlook the textual evidence that has given to such cognitive or psychological explanations their *appearance* of plausibility. The gist of such evidence goes as follows.

We learn from Planck's own pen that by 1900 he had been wrestling “for six years with the problem of equilibrium between radiation and matter” and that it had been “his great ambition to derive the correct form of Kirchhoff's function *from first principles*” (Pais, 1982, 368, *my italics*). These assertions spell out what we call *motivation*; consider, in this connection, the following remarks by Planck:

Anybody who has been seriously engaged in scientific work of any kind realizes that over the entrance to the gates of the temple of science are written the words: You must have faith. It is a quality which the scientists cannot dispense with.

The man who handles a bulk of results obtained from an experimental process must have *an imaginative picture of the law that he is pursuing*. He must embody this in an imaginary hypothesis. The reasoning faculties alone will not help him forward a step, for no order can emerge from that chaos of elements *unless there is the constructive quality of mind which builds up the order by a process of elimination and choice*. Again and again, the imaginary plan on which one attempts to build up that order breaks down and then we must try another. This imaginative vision and faith in the ultimate success are indispensable. *The pure rationalist has no place here*. (Planck, 1933, 214–215, *my italics*)

In view of facts such as these it seems that we are now able to put the relevant elements of Planck's epistemic situation in their proper conceptual boxes. Recall that Planck's interests were focused on black-body radiation *theory*, for the elaboration of which *empirical input* is required. In Planck's early works, the fact of experience which guided his train of thought, as we have seen, was Wien's law. I have already remarked

upon the fact that Planck presented a derivation of it in 1899, but, soon after, deviations from Wien's distribution law were experimentally detected; the basis of Planck's work was thus thereby undercut. Planck solved the problem *himself*, then, as we have seen, within a few months' time (more specifically, in October 1900; the communication of the result, Eq. (15), had the title "An Improvement of the Wien Distribution"). The situation was then: Planck's phenomenological formula (product, as he put it (Planck, 1949, 41), of a "lucky intuition") was an outstanding achievement—it alone would have been sufficient to win for him enduring fame—but for the last time: his theoretician's ambition was the *derivation* of Kirchhoff's function; by October 1900, it turned out to be the case that the correct mathematical representation of the black-body radiation law was not, after all, Wien's—it was his own. The reader may well imagine the fervent urgency with which the man felt the need to solve his problem; the weeks that followed, Planck wrote in his reminiscences, were "the most strenuous work of my life" (Planck, 1933). And it happens—and this is the twist—that such motivation, such urgency, may seem to hold the key—as the reader shall find next—to Planck's inconsistency.

3. Note next that Planck, as of 1900, was an outspoken critic of the probabilistic kinetic theory of heat. But unlike so many, Planck did not base his criticism on general philosophical grounds; his reasons stemmed from physical theory.²³ The following passage from his *Scientific Autobiography* makes this point evident:

I was not only indifferent but to a certain extent even hostile to the atomic theory which was the entire foundation of [Boltzmann's] research. The reason was that at that time I regarded the principle of increase of entropy as no less immutably valid than the principle of conservation of energy itself, whereas Boltzmann treated the former merely as a law of probabilities—in other words, as a principle that could admit exceptions. (Planck, 1949, 32)

Note, now, that the radiation theory that Planck had been trying to develop hitherto, the theory with which he had been able to in fact derive Wien's law in 1899, was entirely based on the fundamental idea that the approach-to-thermodynamic-equilibrium of a radiation-in-a-box system could be described assuming only the laws of electrodynamics. We need not pause over details; the relevant fact here is merely qualitative, namely that one cannot, as Planck soon found out, derive his formula by this route. And all matters of fact and psychological influences and epistemological circumstances we have discussed up to this point have now served their purpose by preparing the reader for *this*: Under the sway of the emotional demand to solve his problem, Planck turned to Boltzmann's theory "in an act of desperation... I had to obtain a positive result, under any circumstance and at whatever cost" (Planck, 1949, 32). In an 1877 memoir of Boltzmann's, one can find a discussion of the mechanical properties of a gas of N absolutely elastic spherical molecules, each of which are allowed to take only finite values of velocity $0, \frac{1}{q}, \frac{2}{q}, \frac{3}{q}, \dots, \frac{p}{q}$ and of energy $0, \epsilon, 2\epsilon, 3\epsilon, \dots, p\epsilon$. The similarities between Planck's and Boltzmann's treatments seem to be too striking to be accidental; there can be, it may seem, no doubt that this essay was Planck's combinatorics guide. Now look again, reader, at the ingredients of the derivation of (1); the "non-classical" ingredients—those which Planck combined with the classical ones leading to a self-nullifying derivation, almost ubiquitously believed to have inaugurated quantum theory—those ingredients are *there*, in Boltzmann's 1877 essay, for anyone who cares to look. And mind you, Boltzmann warns the reader: the discreteness

assumption "*does not correspond to any realistic mechanical model, but it is easier to handle mathematically, and the actual problem to be solved is re-established by letting p and q go to infinity [hence ϵ goes to zero]*" (Boltzmann, 1877, 1976, my italics). From here, the full picture seems to follow readily. Planck, whose mind was in the state of hunting, of searching, of trying this and that, upon using Boltzmann's formal trick did not take the limit at the end of the calculation—*contra* Boltzmann. And the motive behind the omission seems to stand out sharply; it seems to have no theoretical or logical or empirical foundation.

Planck had "a picture of the law he was pursuing": his (15). He was eager to derive it "at whatever cost". The motive—and this is the punchline—was, then, it seems, very simple: the law doesn't follow otherwise.

That's the route that led Planck to derive his black-body radiation law.

4. Let's think this through with some care. Today we say that Planck's law "reduces" to (13) in the so-called "classical limit" (long wavelengths). But there is an alternative route: Consider, to start with, Eq. (10). Expand it in a Taylor series. Follow Boltzmann's instruction. Take the $\epsilon \rightarrow 0$ limit in order to reestablish, as Boltzmann put it, "the actual problem to be solved." (This, the reader will remember, Planck did not do.) Then we have

$$\frac{\partial S}{\partial U} = \frac{k}{U}. \quad (16)$$

By (2), this is just Eq. (12). And by (5), as was already shown, we are led, ineluctably, inexorably, to (13), the ultraviolet catastrophe, all over again.

But that can't be all there is to it. For when Rayleigh derived (13) (June 1900), he proposed it as an *effective* law, and so the long-wavelength limit, as of 1900, was *not* the "classical" limit; Wien's law, valid for the short wavelengths, had after all been proposed in 1896. And the truth of (1) ("the finer the methods of measurement used, the more accurate the formula was found to be") was surely beyond doubt for Planck; the possibility of the laws of electrodynamics and thermodynamics not being *right*, without qualification, for the reasons already pointed out, as I have been at pains to emphasize, could hardly be conceived. In which case it must be admitted that as far as Planck could *think*, as far as he could *perceive*, his formula, and the premises leading to it, were *classical*, period. Mind you: Planck said so himself. His derivation of (1) "does not seem to contradict any of the facts established so far" (Planck, 1901, 555). After all,

the fact that the chosen energy element ϵ for a given group of resonators must be proportional to the frequency ν follows immediately from the extremely important Wien displacement law. (Planck, 1900a, 87)

Wien's displacement law, derived by Wien in 1893, is a *purely thermodynamic result* (Wien, 1893). It is a telling, but now half-forgotten, fact that it implies (see, e.g., (Ehrenfest, 1911) and (Planck, 1914, §3)) the statement that the ratio

$$\frac{\epsilon_p}{\nu_p}, \quad (17)$$

where ϵ_p is the energy of each of the p modes of black-body radiation, and ν_p the associated frequency, is an *adiabatic invariant*. And this means that relative variations of the kind

$$\frac{\epsilon_p}{\nu_p^2} \quad (18)$$

would lead to a conflict with the second law of thermodynamics.²⁴

So the upshot of point 3, above, misfires. It misfires because it is not as if Planck's definition of the energy elements meant a departure from classical theory. His words imply the very opposite.

²³ See (Brush, 2003, 491), where the probabilistic kinetic theory of heat as of 1900 is described as a "much-maligned beauty". For a scholarly discussion about the scientific status of the probabilistic kinetic theory of heat in *fin de siècle* physics, see (Smith & Seth, 2020, chapter 2, 51-ff). The situation can also be evaluated from the tone and content of Boltzmann's remarks in the Foreword of his (Boltzmann, 1898)—compare with footnote 29, below.

²⁴ This fact will be discussed by the author in detail in a separate paper.

And since thermodynamic relations are model-independent, no further justification was apparently needed. And since classical physics also requires that dynamical states of physical systems vary continuously—the laws of dynamics, after all, are *differential* equations—, if one partitions the energy of the system into $\epsilon > 0$ cells, then the continuity requirement is satisfied if only the move is only, and *merely*, a formal artifice.

And therefore it is not at all surprising that this formal maneuver is exactly what Max Planck did to derive (1).

Consider the passage that has been perennially (mis)read as the introduction of quantization in physics:

We must now give the distribution of the energy over the separate resonators of each group, first of all the distribution of the energy E over the N resonators of frequency ν . If E is considered a continuously divisible quantity, this distribution is possible in infinitely many ways. We consider, however—this is the most essential point of the whole calculation— E to be composed of a very definite number of equal parts and use thereto the constant of nature $h = 6.55 \times 10^{27}$ erg s. This constant multiplied by the common frequency ν of the resonators gives us the energy element ϵ in erg, and dividing E by ϵ we get the number P of energy elements which must be divided over the N resonators. *If the ratio is not an integer, we take for P an integer in the neighborhood.* (Planck, 1900a, 84, my italics)

If the resonator energy were actually quantized, then $\frac{E}{\epsilon}$ should be an integer. But Planck says explicitly that it need not be. The abstraction of partitioning the continuous, infinitely divisible quantity E into P energy elements was necessary to carry out the combinatorics to compute the entropy of radiation in a way that was consistent with both Wien's displacement law and the second law of thermodynamics; it was—let me repeat it expressly once more—a *formal* maneuver. And formal maneuvers do not have physical consequences.

An analogy with statistical mechanics will now be instructive. In statistical mechanics, the abstraction of partitioning the infinitely divisible quantities position q and momentum p into cells of definite size $\Delta q \Delta p$ to count the states is routine. No one will say that this restricts the possible values of p and q of a particle. True, in statistical mechanics, the size of the cell of phase space does not matter, whereas for radiation Planck had to fix, for the reasons given, the cell size of phase space at $h\nu$. But the point remains unscathed: the energy elements are a restriction on cell size *only*. It is *not* a restriction on the possible values of resonator energy, *at all*.

And so we see that just like in statistical mechanics particles move freely and continuously through and between the $\Delta q \Delta p$ cells of phase space in accordance to Newton's laws, in Planck's radiation theory the resonators move freely and continuously through and between the energy cells in accordance to Maxwell's laws, period.

And so we see that if we just look at Planck's 1900 derivation of the black-body radiation law with unbiased eyes, if we just get out of our heads the preconception that he introduced discontinuity in physics and consider what Planck actually *said*, then the conclusion that he quantized resonator-energy will not occur to us. And so we can indeed conclude—as I mentioned at the outset—from Planck's 1900 proof *alone* that it did *not* inaugurate the quantum theory, period, end of story.

But the climax of our plot is actually *this*: By the upshot of the previous paragraphs, it must now be acknowledged that Planck *did not*, after all, make the error of which we, *following Einstein*, have accused him of committing, for in that earlier analysis we have assumed that the quantity ϵ is some *actual physical something, some indivisible atom of energy*, P of which are distributed amongst N oscillators. Now in this, of course, we have simply taken for granted the almost universal conviction that it is. But the present considerations show that the quantity ϵ *cannot* be given such physical interpretation. Not in the context in which it appeared originally anyway. In saying this, I am

making what may be held to be an extremely controversial statement, but please note that the alternative (which is the standard account of the birth of quantum theory) admits too many irrationalities, it cannot be correct. In fact, it is surprising that outstanding scholars have gotten this wrong for so many decades when a careful reading of the above seminal passages of (Planck, 1900a) makes it straightforward. And that we have gotten the facts exactly right is vouched for by Planck's own words, written in a letter to his friend Robert Wood in 1931, quoted next.

The energy elements were a purely formal assumption, and I really did not give it much thought except that, no matter what the cost, I must bring about a positive result. (cited in (Hermann, 1971, 23))

This passage of Planck's rules out any debate.²⁵

2.4. Now let's draw the significant threads together and pin down the conclusions that the above considerations seem to teach.

We began with the intuition that the orthodox story of the origin of the quantum and Einstein's contention that Planck's proof is wrong are *prima facie* incompatible. Analyzing, then, Planck's proof, we first concluded, in §2.2, that it is inconsistent. But now, one step beyond, we found, by the upshot of the previous section, that, relative to the conceptual situation in which it first appeared, Planck's proof is *not* inconsistent; one can only call it “inconsistent” if a departure from classical physics is assumed, i.e., by dint of a prior appeal to discontinuity. But if Planck's proof is indeed consistent, then now it may seem that the problem we started with has somehow disappeared. What should we make, by the upshot of the previous analysis, of UVC? What was the content of Einstein's critique to Planck, then? Nothing?

Of course, that *absolutely cannot be*. For if (1) is, as we know it to be, a *quantum* formula, and yet Planck himself gave it a classical proof in 1900, then that proof can't be right. But how, indeed, do we get *at* this conclusion? How do we justify the assumption that (1) is quantum? Well, our grounds for having reevaluated Planck's 1900 proof, and concluding therewith that it is not inconsistent, was the acknowledgment that $\epsilon > 0$ was “a purely formal,” and not a physical, assumption. But note that we did not ask the further question as to whether Planck was *justified* in making it. And the fact historical works have heretofore failed to make explicit, the fact that puts all the pieces of the origin-of-the-quantum jigsaw puzzle in their proper conceptual places, is that there is a factual, well-defined sense in which Planck *was*, and a factual, well-defined sense in which Planck *wasn't*, justified in making his “purely formal assumption.”

These facts I shall now proceed to spell out.

First of all we ought to correct a misunderstanding that is deeply-rooted in the literature. The reader will remember from §2.2 that commentators have persistently alleged that (13) was “an inevitable consequence of classical physics.” And we saw, a couple of pages back, that if one takes, in the appropriate mathematical step, the $\epsilon \rightarrow 0$ limit as *bona fide* statistical mechanics requires, then equipartition of energy, and thereby (13), ineluctably follows. Now the case of the orthodox story (or this, at least, is one way of putting it) was that Planck tried to *modify* the kinetic theory of heat by not taking the limit to correct for the ultraviolet catastrophe. So the orthodox story, *on top of* assuming that (13) was inevitable, *on top of* ignoring the consequences encoded in the Wien displacement law, further assumes that Planck, in deriving (1), was (mis)using the kinetic theory of heat. And so, when

²⁵ Such a purely formal relationship between energy and frequency that follows as a consequence of the Wien displacement law was also indicated by Lorentz in a highly technical paper published in 1901; see (Lorentz, 1901, 449–450). The thermodynamic connection between the energy-frequency relation and the Wien displacement law will be proven and explained in great detail by the author in a sequel to this paper.

(Klein, 1961) claimed that Planck “was not aware” of the ultraviolet catastrophe, it followed that Planck couldn’t have misapplied Boltzmann’s theory as a stop-gap measure. And therewith, the question: How did Planck manage to avoid the allegedly “inevitable”? Klein, and Kuhn, adopted the course of maintaining that Planck “misunderstood” Boltzmann’s instructions ((Klein, 1961, 474); (Kuhn, 1978, 128)); they differed only in how they interpreted (what they saw as) Planck’s mistake:

for Klein, Planck’s misunderstanding allows him to go forward with quantization without realizing the full import of what he had done. For Kuhn, Planck’s misunderstanding allows him to proceed ‘fully classically’ without ever meaning to have quantized resonator energy. (Galison, 1981, 79)

Thus, as the conclusion that Planck was not justified in not taking the $\epsilon \rightarrow 0$ limit in Klein’s and Kuhn’s accounts follows from assuming, first, that (13) is inevitable, and second, that Planck was following the kinetic theory of heat, and we have already shown, in §2.2, that the first assumption is false, all that is now required to show that Planck made no mistake in the sense meant by Klein and Kuhn is to make indubitable the fact that the latter assumption is also false. Our discussion in §2.3 already argued to this conclusion. But if any doubt on this point still remains, we may remove it with the help of the following remarks.

We saw in §2.2 that in his (Planck, 1900b) Planck said that Wien’s law was first derived by “W. Wien from the molecular-kinetic consideration and later by me from the theory of electromagnetic radiation.” In (Planck, 1900a), in the course of talking about the necessity (!) of his derivation of (1), Planck was careful to remark:

[...] Apart from that, the whole deduction is based upon the theorem that the entropy of a system of resonators with given energy is proportional to the logarithm of the total number of possible complexions for the given energy. This theorem can be split into two other theorems: (1) The entropy of the system in a given state is proportional to the logarithm of the probability of that state, and (2) The probability of any state is proportional to the number of corresponding complexions, or, in other words, any definite complexion is equally probable as any other complexion. The first theorem is, *as far as radiative phenomena are concerned, just a definition of the probability of the state*, insofar as we have for energy radiation no other a priori way to define the probability that the definition of its entropy. *We have here a distinction from the corresponding situation in the kinetic theory of gases.* (Planck, 1900a, 87–88, my italics)

Planck’s point, then, in the italicized remarks, cannot be clearer: *statistical considerations are introduced differently in his theory of radiation and in the probabilistic kinetic theory of heat*. Planck, after all, was applying the entropy–probability relation to *radiation*, not to atoms. Finally, still in (Planck, 1900a), in a footnote, after presenting the steps to derive (1), Planck defended his right to put forward a different microphysical model:

When Mr. W. Wien in his Paris report about the theoretical radiation laws did not find my theory on the irreversible radiation phenomena satisfactory since it did not give the proof that the hypothesis of natural radiation is the only one which leads to irreversibility, he surely demanded, in my opinion, too much of this hypothesis. [...] From the same point of view *one should also declare the kinetic theory of gases to be unsatisfactory since nobody has yet proved that the atomistic hypothesis is the only one which explains irreversibility.* (Planck, 1900a, 88, my italics)

That Planck was *not* using the probabilistic kinetic theory of heat, but his own theory, to derive (1), is once again transparent. It is ironic that the contention that Planck misapplied, either wittingly or unwittingly, the kinetic theory of heat when he did not take the $\epsilon \rightarrow 0$ limit is in itself a misapplication. The energy elements, to repeat the point expressly once more, *are adiabatic invariants mathematically deduced from the second law of thermodynamics*; they “follow immediately” from the Wien displacement law—that’s why Planck could call them at the same time “purely formal” and “the most essential point of the whole calculation.” And by way of these remarks we see once again how explicit evidence that the orthodox story of the origin of the quantum is false is indeed *there*, in Planck’s 1900 proof of (1), for anybody who might have cared to look.

And we also see that the habit of leaning on assumptions about people’s way of thinking to make sense of the dynamics of science may make us prone to overlook the matters-of-fact. Indeed, the foregoing mistaken assumptions, on top of the fact that Planck derived (1) by inferring premises from conclusion, on top of the fact that he was “desperate” to find a derivation, misled generations of scholars into unqualifiedly assuming that Planck reasoned—as physicists do, according to Kuhn’s *Structure*, at times preceding a scientific revolution—without foundation. But this conclusion is wholly unjustified! In (Planck, 1900b), Planck said that he had been led to believe that Wien’s law was “absolutely valid” not merely because it fit observations but because of the relationship between “the electromagnetic theory developed by me and the experimental data.” His 1899 derivation of Wien’s law, remember, had enabled him to construct his famous system of units based entirely on physical constants; when Wien’s law was falsified, Planck then “looked for the *next* simpler formula” that seemed to fit the available data and his theory: this was his (15). Now note, reader, that this formula looks, on the surface, quite peculiar: (15) has two independent constants, one in the numerator, and the other in the exponent of e in the denominator, while (1), the derived formula, has not one but both constants in the exponent of e in the denominator. Finding a way for the exponent of e to be dimensionless was no doubt a challenge in the theoretical transition from (15) to (1); to meet this challenge, Planck, “in an act of desperation,” *adapted* his theory: he borrowed from Boltzmann the entropy–probability relation. But for Planck the measure is not blended, as is for Boltzmann, with a statistical interpretation of the second law: “We have here a distinction,” Planck warned us, “from the corresponding situation in the kinetic theory of gases”; Planck indicated explicitly that the expression he found for W should be considered “a *definition* of the probability of the state” (see the quote above). Now, the reader must note—and this point is crucial, mind you, to our whole story—that *the purely formal nature of the energy elements does not allow Planck to construct W in any other way*. But that this move is perfectly consistent with *bona fide* classical-physics practice, I think the reader will concede without further ado. Planck had every right to expand *his* theory.

And so it seems to be a grave injustice to Planck and his dignity as a physicist to say, like so many historians have done, that he didn’t know what he was doing. The contrary seems to be the truth. For Planck could and did claim that it was not, then, Wien’s, *but his own formula*, that was “absolutely valid” not merely because it fit observations, but because of the “relationship between his theory and the experimental data.” Recall the confidence with which he closed his presentation of his seminal 1900 proof of (1)—for the convenience of the reader, I repeat below the quote from §1:

If the theory is at all correct, all these relations [i.e., the values obtained for Avogadro’s number N , Loschmidt’s number L , and for the “quantum of electricity” e] should be not approximately, but absolutely, valid. The accuracy of the calculated number [for e] is thus essentially the same as that of the relatively worst known, the radiation constant k , and is thus much better than all determinations up to now. To test it by more direct methods should be both an important and a necessary task for further research. (Planck, 1900a, 89–90)

Measurements, after all, are *theory-mediated*, and the argument Planck presented at the end of his proof—namely, a cross-check between the values for the constants he obtained *via* his 1900 derivation with the values for constants obtained by other investigators *via* independent methods²⁶—was the best he could possibly give in favor of the correctness of his theory.²⁷

And so it is perfectly clear that Planck's 1900 proof was not inconsistent in the sense that it was self-contradictory, nor inconsistent in the sense that it misapplied the probabilistic kinetic theory of heat. On the contrary. *By the authority of the experimental evidence available at that time*, his derivation was perfectly consistent. But there's yet another objection, and this one, *in due time*, shall not miss its target. I refer to the objection that Planck should have used the probabilistic kinetic theory of heat *instead*.

Here's where Einstein comes in. In (Einstein, 1905a, §1), he—the then-unknown patent officer, who sent his paper to be published at the *Annalen der Physik*, whose editor at that time was Max Planck—asks us to consider a system of gas molecules, free electrons, and a collection of resonators (he defines them as “electrons bound to points in space that emit and absorb electromagnetic waves of definite period”) together in a box. The kinetic theory of heat tells us that the equipartition theorem holds for the gas, and therefore, via the dynamical equilibrium condition, for the whole system.²⁸ Hence, the energy distribution of the system is necessarily (13), in irreconcilable contradiction with the facts.

The critique of Planck's 1900 derivation is evidently between the lines in the argument. But Einstein's point, though implicit, to those who have followed our discussion so far, is clear enough: Planck had no right to ignore the equipartition theorem, no right in refraining from taking the $\epsilon \rightarrow 0$ limit, no right to his formal maneuver. If Planck's resonators are mechanical systems, then they obey the equipartition theorem, period. *This*, I now emphasize, is Einstein's point with UVC. We failed to see it back in §2.2 because hitherto we had mistaken the target of Einstein's critique: we thought it was the orthodox version of the derivation, when in fact it was the derivation argued for here. We were, back then, still under the spell of the orthodox-story myth.

But our considerations in §2.2 will have now served their main purpose by enabling the reader to recognize immediately that Einstein's objection—as of 1900—had little, if any, force against Planck.

For Einstein's argument patently depends on the validity of the equipartition theorem, which depends on the validity of the probabilistic kinetic theory of heat, which—as I have been at pains to emphasize—was *not* established in 1900. It is not a coincidence that in 1905 Einstein criticized Planck almost without saying it but in 1909, as we saw in §1, he was point-blank about it. *The empirical authority of the theoretical ammunition he was using changed drastically between 1905 and 1909.*²⁹ Remember Einstein's 1949 remark: “the imperfections of

Planck's derivation remained at first hidden.” The information that brought the imperfections of Planck's derivation to the fore—namely, conclusive empirical evidence of the validity of the probabilistic kinetic theory of heat—was not, as of 1900, literally in the field of vision.

And so we now see not only that Planck *did not* follow the probabilistic kinetic theory of heat in his 1900 derivation, notwithstanding nearly everybody's impression to the contrary. (His deduction of (1), to quote from the seminal 1900 paper again, “is based on the laws of electromagnetic radiation, thermodynamics, and probability calculus.”) Planck, as a matter of fact, *need not to have followed* the kinetic theory of heat as of 1900, for, to borrow from the 1900 paper again, “nobody has yet proved that the atomistic hypothesis is the only one which explains irreversibility.” Unlike Einstein, *Planck left the structure and motions of his resonators undetermined* (see (Planck, 1899, §1)); the aim was to develop a more general theory, free of special microphysical hypothesis. And if we just stop to think about the upshot of our §2.3 discussion, if we just remember the difficulties plaguing, at that time, the probabilistic kinetic theory of heat (see footnote 29 again), doesn't Planck's seem like a plausible—perhaps even the best—course of action?

This is the sense, to which I referred above, in which Planck was justified, in 1900, in not taking the $\epsilon \rightarrow 0$ limit. The sense in which he wasn't comes of course from the stringent confirmation, by Perrin, starting in 1908, of the probabilistic kinetic theory of heat predictions.³⁰ Thereafter, Planck's resonators had to be (re)interpreted

²⁹ “It is not at all easy for us today,” writes the kinetic theory of heat expert Marian Smoluchowski in 1914, in a very instructive paper called “Limits of Validity of the Second Law of Thermodynamics,”

to recall the mood that was prevalent toward the end of the last century. At that time the scientific leaders in Germany and in France were convinced—with a few exceptions—that *the kinetic theory of heat had ceased to be of any importance*. Considering the great successes of thermodynamics, the second law had been raised to the rank of an exactly valid, absolute dogma that was true without exception. And as the kinetic theory had run into certain difficulties in the attempt to arrive at a satisfactory interpretation, especially as regards to irreversible processes, *one had condemned it together with the atomistic hypothesis* [...]. All this has changed today. (Smoluchowski (1913), cited in (Feyerabend, 1965, 230, my italics))

Compare with footnote 34.

³⁰ In *Black-Body Theory*, and again in (Kuhn, 1984), Kuhn claims that it was Lorentz's authority that changed Planck's mind (Lorentz presented a derivation of the Rayleigh–Jeans law at the *International Congress of Mathematicians* that took place in Rome between April 6–11, 1908, and Planck attended it). That, however, is impossible. For the letter from which Kuhn partially quotes to make his claim (see (Kuhn, 1978, 197)) dates from April 1, 1908—before the congress happened, and if one just reads the *full* letter, it becomes rather obvious that Planck was already convinced of the absolute validity of the Rayleigh–Jeans law. Now Kuhn also remarks that Lorentz's 1908 proof of the Rayleigh–Jeans law was “in its own right, the fullest, most general, and most compelling yet presented” (Kuhn, 1978, 195), and he thereby implies that it was Lorentz's authority that convinced the scientific community at large of the inevitability of the Rayleigh–Jeans law. But that too is impossible. For, as the editor of Lorentz's scientific correspondence, Anne J. Kox, remarks, Lorentz's 1908 derivation of the Rayleigh–Jeans law “was contested very quickly,” among others by W. Wien, and Lorentz himself wrote that he “was quickly convinced and quite embarrassed by his mistake, for which he could not find any good excuse” (Kox, 2018, viii). What I have been suggesting here instead is the following: If we consider the assumptions Planck made in his 1900 derivation of the black-body radiation law *vis-à-vis* the assumptions stringently tested by Perrin in 1908 (see (Smith & Seth, 2020)), we will see that we have solid historical and theoretical justification for concluding that it was probably *the authority of the empirical evidence* that had a decisive impact on the scientific community after 1908. The general proofs of the Rayleigh–Jeans law that appeared around that time only widened the gap in theory that Perrin's 1908–1911 experiments split wide open; these proofs spelled out

²⁶ For a discussion of the accuracy of the values Planck obtained for N , L , and e , *vis-à-vis* other values available at that time, see (Smith & Seth, 2020, 16–18).

²⁷ There's yet another telling fact to be detected in the above passage. Today we call the constant k “Boltzmann constant.” We associate it with the probabilistic kinetic theory of heat. But Boltzmann himself never introduced it. Planck did. In 1900. To derive (1). Without using the atomistic hypothesis. And Planck called it—and this is the telling fact—“radiation constant” in the above quote.

²⁸ The reader will recall that black-body radiation is by definition identical to the radiation in equilibrium with a body in an enclosure; it is a body that takes up by absorption as much heat as it loses by emission that expresses the existence of a certain temperature equilibrium for radiation. It follows, then, *from this definition* that matter and radiation must have the same energy distribution to satisfy the equilibrium condition—and from this it further follows that if equipartition of energy must hold for matter (say, a gas), as the kinetic theory of heat says that it must, then it must also hold for the modes of the radiation that the gas is in equilibrium with; it must hold, that is to say, for black-body radiation.

as mechanical systems, in which case, Einstein's considerations apply,³¹ and so we have no choice but to take the $\epsilon \rightarrow 0$ limit, and so we have equipartition, and so we have the ultraviolet catastrophe, yet again. We have the conclusion that Planck's 1900 classical derivation of (1) is mistaken because it ignored the dynamical equilibrium condition between matter and radiation.³² We have a minutely-confirmed-by-experiments black-body radiation formula with a constant h in it that doesn't appear at all in the "classical" prediction.

This is the last piece of the puzzle. It now takes only a step back to see the full picture.

3. The origin of the quantum theory

To establish that there was a crisis in physics in the beginning of the 20th century one would have to first establish the validity of the probabilistic kinetic theory of heat. And this, we all know, is what *Einstein* tried to do in 1905 through the means of his predictions regarding Brownian motion. It is therefore little wonder that the same man, from the same year on, started repeating that there was no escape from (13), that Planck's 1900 derivation of (1) is *wrong*, that the foundations of physics must be considered afresh. That the quantum revolution was

the fact that this gap in the probabilistic kinetic theory of heat could not be filled—or ignored or avoided—by appealing to artificial assumptions, and *ad hoc* hypotheses (like shoving the blame onto the ether, for example). See also footnote 31, below.

³¹ To verify the correctness of the interpretation of his criticism to Planck argued for here, check (Einstein, 1909b, §2-4). For the convenience of the reader, I will summarize the main points: Einstein devotes §2 to a derivation of (13), and starts §3 by emphasizing that "there can be no doubt" that "our current theoretical views lead inevitably" to this formula. In §4 he asks how Planck's radiation theory is related to the probabilistic kinetic theory of heat which entails (13). "In my view the answer to this question is made harder," he says, "by the fact Planck's own presentation of his theory suffers from a certain logical imperfection." The imperfection, Einstein says, is this:

Mr. Planck would have had no freedom in the choice of complexions in the resonator theory of radiation. He could have been permitted to postulate the pair of equations

$$S = k \log W \quad (19)$$

and

$$W = \text{number of complexions} \quad (20)$$

only if he had appended the condition that the complexions must be chosen such that in the theoretical model chosen by him they had been found to be equally probable on the basis of statistical considerations. In this way he would have arrived at the formula defended by Jeans [i.e., (13)]. Though every physicist must rejoice that Mr. Planck disregarded these requirements in such a fortunate manner, it should not be forgotten that the Planck radiation formula is incompatible with the theoretical foundation from which Mr. Planck started out. (Einstein, 1909b, 362-363)

This last statement is exactly the critique to Planck we used to motivate our study in §1. The reader will recall that probability theory is introduced in Planck's theory of natural radiation, and in Boltzmann's probabilistic kinetic theory of heat, indeed in very different ways—Planck was, as we have seen, careful to point this out in his 1900 proof. Now, the punch line, which I cite as proof of the correctness of the account of the origin of the quantum theory that I put forward here: in a footnote in §4, Einstein adds that the fact that "the only tenable way" of distributing states statistically is the way he was arguing for, the way which coincides with Boltzmann's, the way that leads "ineluctably" to (13), and evinces that Planck's way of doing statistics is wrong, "follows immediately from the properties of Brownian motion" (Einstein, 1909b, 361, footnote 1). It is the fact that the irregularity of Brownian motion requires one to impose equiprobability of states. See also footnote 33, below.

³² See footnote 28.

set in motion by Albert Einstein seems now to follow as a matter of logic. I shall finish by giving you *evidence*.

Consider (Einstein, 1905a) again. In §1, Einstein argued, as we have seen, that (13) was inevitable. The point of his next section, though Einstein, characteristically, does not make this really explicit, is to show just what the inevitability of (13) implies, *given what Planck did*. Planck's (1) yields, Einstein remarks, $E = kT$ for high temperatures, while the kinetic theory of heat yields $E = \frac{R}{N}T$. So we can set $N = \frac{R}{k}$, and the value for N calculated with Planck's theory, and the values for N calculated with the kinetic theory of heat, are indeed in good agreement. But what this agreement shows, Einstein says, is *not*, as Planck thought, that Planck's derivation of (1) is "absolutely valid," but rather, on the contrary, that "Planck's determination of the elementary quanta is to some extent *independent of his theory of black-body radiation*." Einstein leaves two points between the lines here. First, though Planck's derivation is wrong, *his constants are universal*; second, though Planck's constant k is predicated in classical physics, *his constant h is not*. And what all of this tells us, Einstein concludes, is that

the greater the energy density and the wavelength of radiation, the more useful the theoretical principles we have been using prove to be; however, *these principles fail completely in the case of small wavelengths and small radiation densities*. (Einstein, 1905a, 91, my italics)

This, of course, is the statement of a crisis. This is the statement that (13) is "the classical limit," as we say today. After Perrin confirmed his Brownian motion predictions, Einstein's arguments gained force, conviction, and eventually, currency.³³ And it is surely little wonder that the same man who noted, and found the means to empirically establish, *that* classical physics failed, was the man who came up with an explanation, *on the basis of a new hypothesis*, for *why* it failed. Just how Einstein managed to do this—just how he stepped outside classical physics to engage in this revisionary task—shows how skillfully he had schooled himself in careful epistemological thinking: "Without having a substitute for classical mechanics," he reminisced in 1949, in his *Autobiographical Notes*, "I could nevertheless see to what kind of consequences [Planck's] law of temperature–radiation leads" (Einstein, 1949, 45). The question as to what Einstein's (1905a) involves, aims, and achieves, seems now to be ripe for reanalysis. I cannot undertake this detailed task here, so I shall pursue it in a separate paper. But the conclusion of Einstein's (Einstein, 1905a) is, anyway, known to everyone: Radiation has a granular structure. In complete contradiction with the laws of electrodynamics.

Only one must note the consequences which this fact involves.

Consider again resonators and radiation in equilibrium in a box. They are, of course, exchanging energy. If radiation is discrete, then radiation-energy can be transferred to the resonator only in discrete amounts, *so that the energy states available to the resonators are also discrete*. The process of emission and absorption of radiation, i.e., the dynamics of the resonators, is therefore discontinuous: the resonator jumps from one state to the other without passing through a continuous series of intermediate states. *In complete contradiction with the laws of mechanics*.

³³ Compare footnotes 20, 29, and 31. A further evidence of the importance of Perrin's experiments for the whys and wherefores of the quantum revolution—for they gave to Einstein's argument in favor of the absolute inevitability of (13) the empirical validation that they scientifically required, and it is from such inevitability that the contention that "the logical reason for the quantum theory is found in the fact that the Rayleigh–Jeans radiation formula does not agree with experiment" (Planck, 1914, 265) derives its pull—is the fact that Perrin was part of the selected group of physicists that participated of the 1911 Solvay Conference. The topic of this conference, let us remember, was "Radiation Theory and the Quanta," yet Perrin was invited to talk about neither radiation nor quanta. He talked about his Brownian motion experiments. I thank George Smith for calling this latter fact to my attention.

Einstein leaves all of this between the lines in (Einstein, 1905a), and we can easily understand why. But a year later, in (Einstein, 1906), he reiterates the point he made in (Einstein, 1905a, §1): resonators in equilibrium with radiation in an enclosure should, according to classical physics, obey the Rayleigh–Jeans law, but, as a matter of empirical fact, they do not. Einstein then rhetorically asks how can it be that Planck arrived a different, empirically correct, law. His own answer is that “Planck’s theory makes implicit use of [my] hypothesis of energy quanta” (Einstein, 1906, 192).

To see this, one must only apply the foregoing conclusions to Planck’s law. “To arrive at Planck’s formula,” Einstein remarks, “one has to postulate that, rather than assume any value whatever, the energy E of a resonator can only assume values that are integral multiples of ϵ , where $\epsilon = h\nu$ ” (Einstein, 1906, 195). The reader must note that this yields *exactly* the self-contradictory derivation we mistakenly attributed to Planck at the end of §2.2; we are finally in position to understand what *that* contradiction entails. Commentators who have described it merely as another derivation of Planck’s (1) completely underestimate its significance. For it is a different kind of game that Einstein is playing here.

In my opinion the above considerations do not at all disprove Planck’s theory of radiation; rather, they seem to me to show that with his theory of radiation Mr. Planck introduced into physics a new hypothetical element: the hypothesis of light-quanta. (Einstein, 1906, 196)

Contradictory statements cannot be either both true or both false. The truth of one necessarily implies the falsity of the other and vice-versa. Thus, if the hypothesis of light-quanta stands in contradiction with the mechanical and electrodynamical laws that, together, entail (13), then there can be no doubt about how to distribute truth-values between the contradictories.

Think of the fact that in 1900 Lord Rayleigh proved equipartition using only the elementary principles of mechanics. “We are here faced with a fundamental difficulty,” he concluded,

relating not to the theory of gases merely, but rather to general dynamics. [...] What would appear to be wanted is some escape from the destructive simplicity of the general conclusion relating to the partition of kinetic energy. (Rayleigh, 1900, 118)

Einstein said it himself: he didn’t have a substitute for classical mechanics. But he could nevertheless

see to what kind of consequences [Planck’s] law of temperature-radiation leads for the photoelectric effect and for other related phenomena of the transformation of energy-radiation, as well as the specific heat of solid bodies. (Einstein, 1949, 45)

The escape from the destructive simplicity of energy equipartition (which leads, let me repeat once more, to the Rayleigh–Jeans law of black-body radiation, and to the Dulong–Petit law of specific heats) followed from the light-quanta hypothesis: the energy states of radiation, and thereby of matter, are actually discrete, and hence they cannot vary continuously. And therewith, of course—since the laws of classical physics are all differential laws that require continuity—, *on top of* the ultraviolet catastrophe crisis, *another* crisis ensued:³⁴

³⁴ We can see this crisis-on-top-of-a-crisis spirit reflected in the opening paragraphs of the invitation letter, drafted by Walther Nernst, to attend the 1911 Solvay Conference in Physics:

Invitation to an 'International scientific conference to elucidate certain current questions of the kinetic theory. It appears that we find ourselves at present in the midst of an all-encompassing reformulation of the principles on

All my attempts [...] to adapt the theoretical foundation of physics to this [new type of] knowledge failed completely. It was as if the ground had been pulled out from under one, with no firm foundation to be seen anywhere, upon which one could have built. (ibid)

Newtonian laws would no longer do. What was required was a new—quantum—mechanics.

The rest, as they say, is history. Further developments only confirmed the correctness of Einstein’s fundamental insight.³⁵ And if a final comment be desired, to understand how can it be that Max Planck won the Nobel Prize in 1918 for the discovery of energy quanta, let us note that Einstein always called the quantum hypothesis (encompassing both energy quanta and light-quanta) “Planck’s theory,” that everyone followed him in this regard, and that the person who suggested Planck for the Nobel Prize Committee that year was Albert Einstein.

If any doubt on this point still remains, the following excerpt, from a letter of M. Besso to Einstein, dated January 1928, will remove it.

For my part, I was your audience in 1904 and 1905; if, when compiling your communications on the quantum problem, I robbed you of a part of your fame, in return I got you a friend in Planck.³⁶

which the erstwhile kinetic theory of matter has been based. On the one hand, this theory leads to a logical formulation—which nobody contests—of a radiation formula whose validity is contradicted by all experiments; on the other, there follow from the same theory certain results on the specific heat (constancy of the specific heat of a gas with the variation of temperature, the validity of Dulong and Petit’s law up to the lowest temperature), which are also completely refuted by many measurements. [...] these contradictions disappear if one places certain limits (doctrine of energy quanta) on the motion of electrons and atoms in the case of their oscillations around a position of rest. But this interpretation, in turn, is so far-removed from the equations of motion of material points employed until now, that its acceptance would incontestably lead to a far-reaching reformation of our erstwhile fundamental notions. (Solvay 1911, cited in (Mehra, 1975, 6))

This quote provides an outstanding illustration of the point made earlier in footnote 20: If what we call, today, “classical” and “quantum” are names bestowed, as (Staley, 2005) has given textual evidence of, by the participants of the 1911 Solvay Conference, then we can understand, now, why this was so: theory-mediated measurements had just established, *contra* “classical thermodynamics,” the posits of energetics, and the views of the structure of matter as a continuous medium, the validity of the claims of the probabilistic kinetic theory of heat (Perrin). *This theory then rightly became the standard against which claims to change or permanence in the foundations of physics should be compared.*

³⁵ “Einstein’s ‘quantum hypothesis,’” wrote Nernst to a colleague in 1910, “is probably among the most remarkable [constructions] ever; if it is correct, then it indicates completely new paths both for the so-called ‘physics of the ether’ and for all molecular theories” (W. Nernst to A. Schuster, March 10, 1910; cited in (Barkan, 2011, 183). It would be a serious mistake to forget here, given the forcefulness with which I have been emphasizing the role of the logic of evidential reasoning in the whys and wherefores of the quantum revolution, the fact that Einstein’s application of the quantum hypothesis to the behavior of the specific heat of solids (Einstein, 1907)—a logical move, mind you, given that energy quantization is meant to be the remedy for equipartition—was stringently tested by W. Nernst in a series of experiments after 1910 (Nernst, 1910, 1911a, 1911b, 1911c). Nernst’s theory-mediated measurements of the specific heat of solids at low temperatures provided fresh and high-quality empirical proof of the essential correctness of the quantum hypothesis; to appreciate the extent to which the quantum revolution owes its momentum to W. Nernst’s magnificent work and diligence—for it was also Nernst, let us remember, who initiated the 1911 Solvay Conference (see footnote 34)—, see Diana K. Barkan’s excellent (Barkan, 2011), in particular chapters 9–10.

³⁶ Michelle Besso to Albert Einstein, January 17, 1928. Translation from *The Collected Papers of Albert Einstein*, Vol. 16, Document 132: “Meinerseits war ich in den Jahren 1904 und ’05 Dein Publikum habe ich bei der Fassung Deiner Mitteilungen zum Quantenproblem Dich um einen Teil Deines Ruhms gebracht, Dir dafür in Planck einen Freund verschafft”.

This completes my description of how and why the classical gave way to the quantum.³⁷ By looking at the logic, and not at the psychology and the sociology, of research we have “rescued” Planck and Einstein from the tales told about them, and we have established, *contra* Kuhn, that the structure of the quantum revolution, set in motion by Einstein in 1905, is carved at its joints by theory and experiment, and is therefore objective throughout.

One last remark. The “clouds” obscuring 19th century physics, let us remember, to use Lord Kelvin’s famous figure, were the relative motion of ether and ponderable bodies and the dynamical consequences of the equipartition theorem. The former was empirically reflected in the Michelson–Morley experiments; the latter came to a head, as we have seen, when Perrin’s Brownian motion experiments did so much to establish the probabilistic kinetic theory of heat, and thereby, Boltzmann’s construal of entropy. Such problems represented an internal failure of Newtonian physics (a “crisis,” in an objective sense) so that the solutions, to be theoretically meaningful, should be sought outside the Newtonian framework (i.e., outside the “paradigm”). The theory of special relativity, as is well-known, proposed by Albert Einstein in 1905, solved the first problem, by a method that carefully avoids Newtonian premises (the so-called “principle-theory approach”). The same man, following the same method,³⁸ proposed, in the same year, the solution to the second problem: the quantum hypothesis. And therewith it was a logical necessity to construct a new conceptual framework to accommodate it. This was what the quantum “revolution” was all about.

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³⁷ For the moment—see the footnote below.

³⁸ The exact way this was done by Einstein will be shown by the author in a sequel to this paper.

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