Einstein's Fluctuation Formula and the Wave-Particle Duality

Alexei Kojevnikov

1. Introduction

1.1 THE PROBLEM OF ENERGY FLUCTUATIONS

Apart from the history of physical concepts, one can also write the history of persistent problems to which various approaches were applied and, therefore, tested. In the theory of radiation during the first thirty years of this century, one of the most important problems of this kind was the problem of energy fluctuations. The theory of radiation was then a troublesome area of physics. The apparently well-established classical wave theory of light unexpectedly found itself in a crisis, which lasted several decades until a new fundamental theoretical scheme, quantum electrodynamics, became gradually established. In the meantime, many alternative approaches were suggested, yet none of them, despite partial successes, developed into a complete, consistent and universally accepted theory.[1]

The debate centered around several key problems of the classical theory. These anomalies were pushing the theory ahead, suggesting some interpretations, while confirming or refuting others. The list of these problems, in the chronological order of their appearance, included: the derivation and interpretation of the Planck formula, fluctuations of the energy and momentum of radiation, frequencies and intensities (transition probabilities) of atomic spectral lines, the Compton effect, and the dispersion of light.

Contemporary textbooks usually discuss Einstein's fluctuation formula in connection with the wave-particle duality, which the formula is supposed to illustrate. The standard story runs approximately as follows: Einstein believed in the duality of waves and particles from relatively early on. At

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least in the case of light, he already thought about duality in 1909, when he derived the formula for fluctuations of electromagnetic energy with its two equally important terms, one wave and the other corpuscular (Einstein 1909a, Einstein 1909b). But Einstein had not discussed his dualistic beliefs publicly until 1924, when he became acquainted with similar ideas by Louis de Broglie (de Broglie 1924). In his second paper on the gas of Bose-particles, Einstein sided with the dualistic model proposed by the young French physicists (Einstein 1925). Thanks to this support, the previously unknown de Broglie's thesis began to be widely read, his idea of matter waves, in particular, was developed by Erwin Schrödinger into wave mechanics, and the principle of the wave-particle duality became generally accepted as one of the basic foundations of quantum physics.

Some important parts of the above story lack sufficient documentary support, especially regarding ideas in which Einstein believed but which he did not express publicly. Moreover, Einstein did not use the word "duality" either before or after 1925, nor did he make any clear assertion of the principle of the wave-particle duality. Rather than reflecting his personal views on the matter, the story seems to be telling more about perceptions of Einstein's position that were widespread among the physics community. Historians disagreed in their interpretations of the events. John Hendry assumes that Einstein was advocating the wave-particle duality already in 1909 (Hendry 1980, pp. 59, 66–67, 74) and also attributes the same view to Martin Klein (1963). Klein, however, is more careful in his statements and, without disagreeing with the standard interpretation explicitly, stops short of asserting it either, and so do major histories of quantum mechanics (Jammer 1966, Mehra and Rechenberg 1982). Among historically minded physicists, Friedrich Hund (1967, p. 44) writes: "With his paper on the statistical fluctuations in radiation Einstein introduced the duality of waves and particles for light," only to add immediately that "he certainly could not yet see the full consequences of this revolutionary idea."[2] Leon Rosenfeld, on the contrary, asserts that Einstein's 1909 paper was generally understood as, and indeed was, an argument in favor of the corpuscular model of light (Rosenfeld 1973, p. 252), while according to Abraham Pais, Einstein in 1909 "was prepared for a fusion theory (of wave and corpuscles). . . . This fusion now goes by the name of complementarity" (Pais 1982, p. 404).

This spectrum of opinions reflects the fact that the wave-particle duality was and remains a rather vague concept that has neither been well defined nor used with sufficient consistency. Leaving aside the question of whether a precise definition is possible, the present paper looks more carefully into the development of Einstein's and others' views regarding the energy fluctuations in radiation, which could and have been interpreted in a variety of different ways. In fact, the history of one single formula can lead us through practically all major rival approaches in the theory of radiation, duality being only one of them.

1.2 FORMULAE

The formula for the energy fluctuations in radiation appeared first in Einstein 1909a. Consider a thermodynamical system divided into two parts: one, relatively small, which from now on will be called the “system,” and the remaining larger one, the “reservoir.” Their total energy is conserved, but the energy of the smaller part fluctuates around certain equilibrium value E due to exchanges with the reservoir. Regardless of the nature of the system, general laws of thermodynamics give the following expression for the mean square of these fluctuations:

$$\overline{\varepsilon^2} = kT^2 \frac{\partial E}{\partial T}, \tag{1}$$

where k is the Boltzmann constant and T the temperature in equilibrium.

This generally valid formula can be applied to the specific case of a system consisting of a blackbody radiation with temperature T in the frequency interval $(\nu,\nu + d\nu)$ placed in a cavity of the total volume V. In this case, E is determined by the Planck law of the spectral distribution of radiation energy

$$E = \frac{8\pi h\nu^3}{c^3} \frac{1}{e^{h\nu/kT}-1} V d\nu. \tag{2}$$

Inserting (2) into (1) leads directly to Einstein’s main fluctuation formula

$$\overline{\varepsilon^2} = h\nu E + \frac{c^3}{8\pi\nu^2 V d\nu} E^2. \tag{3}$$

It can also be rewritten in the following convenient way:

$$\overline{\left(\frac{\varepsilon}{E}\right)^2} = \frac{h\nu}{E} + \frac{c^3}{8\pi\nu^2 V d\nu} = \frac{1}{N} + \frac{1}{Z}, \tag{4}$$

where N is the number of energy quanta $h\nu$ in E, and Z the number of modes of stationary waves within volume V and frequency interval $d\nu$.

The above derivation can be called "thermodynamical." One can also proceed from the "statistical" point of view which considers microscopic states of the system and their probabilities. To derive (3) this way, one has to postulate certain microscopic models of the structure of radiation, and this is where difficulties arise. Each of the two terms in either (3) or (4) can be obtained rather easily, but only separately and from two apparently incompatible microscopic models: the first one from the model of radiation as an ideal gas of particles with energy $h\nu$; the second one from the classical model of radiation as waves with frequency ν. In the first case, fluctuations of energy arise because the number of particles within the volume V fluctuates. In the second case, the energy of the system fluctuates due to the interference between waves with accidental amplitudes and phases. The subsequent history dealt primarily with the problem of how to obtain the complete result with both of its terms from one unambiguous microscopic model of radiation.

Two other fluctuation formulas, also coming from Einstein, are relevant to the story. The first one is quite similar to (3) and describes the fluctuations of the momentum transmitted by radiation. A mirror with surface area f placed in a cavity will have its momentum fluctuate due to the fluctuations of radiation pressure. From the Planck law, one can obtain the following expression for the mean square of these fluctuations during the time interval T:

$$\overline{\Delta^2} = \frac{f\tau}{cV}\left(h\nu E + \frac{c^3}{8\pi\nu^2 V d\nu}E^2\right), \tag{5}$$

which has a form similar to (3) and poses a similar difficulty with regard to its microscopic derivation and interpretation.

The last of the fluctuation formulas applies only in the limiting case of short waves (the so-called "corpuscular" region). The Planck law in this limit can be approximated by the Wien formula

$$E = \frac{8\pi h\nu^3}{c^3}\left(e^{-h\nu/kT}\right)V d\nu, \tag{6}$$

from which one obtains only the first, quantum or corpuscular, term of energy fluctuations (3). In this limit, one can also calculate the probability of another, quite different and extremely rare fluctuation: an event in which the entire energy of radiation concentrates in a fraction U of the total volume V of the cavity:

$$W = (U/V)^{E/h\nu}. \tag{7}$$

Formula (7) has the same form as the expression for the probability for all atoms of an ideal gas of N particles to be found within the same fraction of the total volume V:

$$W = (U/V)^{N}. \tag{8}$$

This striking similarity constitutes a challenge for the classical wave theory of radiation.

2. Fluctuations before Duality

2.1 THE ATOMICITY OF LIGHT (1905–1909)

The very way Equations (3–5) are written explains why it is so tempting for textbook authors looking for pedagogical simplicity to connect the fluctuation formula with the wave-particle duality and to attribute the same interpretation to Einstein's paper of 1909. However, Einstein's early papers and letters reveal, rather surprisingly, little concern over the opposition of waves and particles. Later he would recognize the dilemma and would become troubled in a most serious way by the impossibility of combining the two concepts, but during the first several years he formulated the main problem regarding light quanta and the structure of radiation in somewhat different terms.

His most persistent idea was that of an analogy between radiation and matter and he sought a uniform treatment for both. His very first paper introducing the hypothesis of light quanta opens with an observation on the existing dissimilarity between theoretical descriptions of ponderable matter and of electromagnetic phenomena (Einstein 1905, p. 132), which he then attempts to overcome. "Light carries mass with it," was his immediate conclusion after the discovery of the equivalence of mass and energy in relativity theory (Einstein to Conrad Habicht, June–September 1905, Einstein 1993b, Doc. 28, p. 33). "The theory of relativity," Einstein added further in his 1909 Salzburg presentation, "has thus changed our view of the nature of light insofar as it does not conceive of light as a sequence of states of a hypothetical medium, but rather *as something having an independent existence just like matter*" (Einstein 1909b, p. 490, italics added).

In his first paper (Einstein 1905) on the analogy between radiation and an ideal gas, Einstein described radiation with the help of the Wien formula, which perfectly suited the analogy, and derived there the fluctuation formula (7). Planck's more general law of the energy spectrum of radiation complicated the analogy and, therefore, Einstein regarded it as somewhat contradicting his views.[3] Already in 1905, he possessed all the necessary tools for the derivation of the first term of the fluctuation formula (3),[4] but he did not make this step until 1909. In the meantime (Einstein 1906), he accepted the Planck law and thus obtained formulas (3) and (5) with both their terms (Einstein 1909a). Yet even in 1909 Einstein still regarded primarily the Wien part of the energy spectrum as experimentally verified, while apparently retaining some reservations about the exact validity of the Planck formula and mentioning in passing a possibility that it might later be found incorrect.[5]

In a letter to Hendrik Antoon Lorentz, Einstein referred to formulas (3) and (5) as "das geringfügige Ergebnis von jahrelangem Nachdenken" ("the trifling result of years of reflection"), the only real progress he had achieved in the problem of radiation since presenting his initial 1905 hypothesis on the structure of light (Einstein to Lorentz, 30 March 1909, Einstein 1993b, Doc. 146, p. 166). The low tone of his remark reflected an acknowledgment that he had not yet succeeded in developing a comprehensive new theory of radiation and an attempt to enlist Lorentz into effort. Despite obtaining both terms of (3), Einstein devoted most of his attention, rhetoric, and actual thoughts to the first one, which, "if present alone, would yield a fluctuation of the radiation energy equal to that produced if the radiation consisted of point quanta of energy $h\nu$ moving independently of each other."[6] Yet he constantly avoided—in the letter to Lorentz as well as in other papers and private letters of the time—referring to light quanta as either "particles" or "corpuscles." Rather than invoking the visual image of a mechanical particle, Einstein repeatedly compared his idea of light quantum to the (non-mechanical) electron: "Still for the time being the most natural interpretation seems to me that the occurrence of electromagnetic fields of light is associated with singular points just like the occurrence of electrostatic fields according to the electron theory" (Einstein 1909b, p. 499); "one should keep in mind the possibility of conceiving of light quanta and electrons as mathematically identically defined singularities" (Einstein to Lorentz, 23 May 1909, Einstein 1993b, Doc. 163, p. 195)

Lorentz's reply was, simultaneously, also the first draft of a paper published the following year (Lorentz 1910). Lorentz pointed out that the latest experimental results had confirmed the exact validity of the phenomenon of interference of light over very large distances in space, which could not be brought into agreement with the idea of spatially localized light quanta. Lorentz's rejection of Einstein's hypothesis was based on careful consideration rather than a prejudice or an a priori conviction, and he even expressed regret: "It is a real pity that the light quantum hypothesis encounters such serious difficulties, because otherwise the hypothesis is very pretty." In the published paper, however, these emotions were omitted and replaced by a categorical statement: "What is said is enough to demonstrate that there can be no discussion about the light quanta which are permanently indivisible and localized within a small space during the motion."[7]

Einstein was already aware of the substance of Lorentz's critique when he discussed the light quantum hypothesis at the annual meeting of the *Gesellschaft Deutscher Naturforscher* in Salzburg in September 1909. This did not make him change the content or tone of his argument in the main part of his paper, but only add two soothing statements at the beginning and at the end of his talk: "It is therefore my opinion that the next stage in the development of theoretical physics will bring us a theory of light that can be understood as a kind of fusion of the wave and emission theories of light"; and "All I wanted is briefly to indicate that the two structural properties (the undulatory structure and the quantum structure) simultaneously displayed by radiation according to the Planck formula should not be considered as mutually incompatible."[8]

These two remarks are often cited as an indication of Einstein's early dualistic views, yet a closer look at Einstein's own description of the kind of "fusion" he was looking for (in the last paragraph of his talk and in more detail in his reply to Lorentz's letter) reveals that his visions were rooted in classical electrodynamics and in Lorentz's concept of the electron. Einstein wrote to Lorentz:

> I believe that the light groups around singular points [a note appended at this point at the foot of the page reads: "not necessarily singular in the mathematical sense"] in a way similar to what we are accustomed to assume about the electrostatic field. Thus I think of a single light quantum as a point surrounded by a greatly extended vector field that somehow decreases with distance. The point is a singularity without which the vector field cannot exist. I wouldn't know to say whether one has to envision a simple superposition of the vector fields when many light quanta with mutually overlapping fields are present. In

> any case, in order to determine the processes one would also have to have equations of motion for the singular points in addition to the differential equations for the vector field, if mathematical singularities are introduced. The energy of the electromagnetic field—at least in the case of sufficiently diffuse radiation—should be related, in some way or another, to the number of these singular points. Absorption would take place only in association with the disappearance of such a singular point or degeneration of the radiation field belonging to this point [a note appended at this point at the foot of the page reads: "Better: 'radiation based upon this point'"]. By specifying the motions of all singularities one would completely determine the vector field, so that the number of variables necessary for the characterization of radiation would be *finite*. . . .
>
> By the way, the essential thing seems to me to be not the assumption of singular points but the assumption of field equations of a kind that permit solutions in which finite quantities of energy propagate with velocity *c* in a specific direction without dispersion.
>
> One would think that this goal could be achieved through a slight modification of Maxwell's theory.[9]

In fact, two different models are discussed in this paragraph: one of light quanta as sources of the field, like electrons in electrodynamics, and the other of particle-like solutions of field equations, similar to many turn-of-the-century attempts to construct field models of the electron and to late Einstein's struggles with the field theory of quanta. Field models of the electron eventually disappeared from classical electrodynamics largely because relativity theory made them unnecessary, yet Einstein's attitude towards them was more positive than one could have derived from this fact. Einstein regarded the relationship between relativity theory and the electron models not as an opposition or rivalry but as being similar to the relationship between the thermodynamics of Thomson and Clausius and the underlying microscopic molecular dynamics of Boltzmann. Like the former, relativity theory and Maxwell's electrodynamics were deductive theories based on universal principles and formally complete. The Maxwell theory would remain, according to Einstein, valid as the description of average properties of macroscopic systems, yet this did not eliminate the need for an underlying microscopic theory of electrons and light quanta.[10]

Rather than either "dualistic" or "corpuscular," Einstein's early views on radiation should better be characterized as "atomistic," and his light quantum as modeled after the (non-mechanical, non-corpuscular) Lorentz electron. The electron—the singularity, the atom of electricity—was initially as foreign to continuous Maxwell's electrodynamics as Einstein's atom of light was to the wave theory of light, yet Lorentz succeeded in this case to marry the two seemingly incompatible concepts into a general theoretical scheme. This feat was exemplar for Einstein, and Lorentz was his hero. Therefore, when his persistent attempts at a comprehensive theory failed, Einstein turned to Lorentz in the hope that the latter would take the job. The success would have meant adding the quantum of radiation to the already existing quantum of electric charge and thus completing the atomistic reform of electrodynamics started by Lorentz at the end of the nineteenth century. As for the precise combination of continuous and discontinuous, field and quantum, aspects in the future theory of radiation, Einstein was ready to try different possibilities which were usually similar to the variety of field and corpuscular combinations discussed in contemporary theories of the electron[11] and hoped that either one or another would eventually work. Only gradually did he become aware of the difficulty, or futility, of the task.

2.2 ACKNOWLEDGING THE ANOMALY

Unlike Einstein's other arguments in favor of light quanta, fluctuation formulas did not offer any realistic hope for experimental test and thus remained purely theoretical considerations. Yet they were, in some sense, Einstein's strongest arguments: all his other examples could be downplayed as dealing with discontinuities in the process of the interaction between light and matter. Fluctuations, on the contrary, occurred in pure radiation and apparently were pointing directly towards its microscopic structure.

As is well known, most of Einstein's colleagues initially did not accept his arguments. His first papers on light quanta (Einstein 1905 and Einstein 1906) appear to have remained practically unnoticed. By the time of his report at the Salzburg meeting, Einstein was already a recognized scholar whose opinions had to be heard, and he did get a number of responses, but mostly critical ones (Discussion 1909, Lorentz 1910, Planck 1910, Ehrenfest 1911). Spokesmen of the theoretical community—Lorentz, Wilhelm Wien, Max Planck—criticized Einstein's suggestion in a strong tone that indicated the existence of a certain consensus. The only theoretician who expressed a more cautious attitude was Paul Ehrenfest, who was as young as Einstein and did not have the authority of the opponents. The list of main objections derived from a variety of publications, including Lorentz 1916, Einstein 1911, Discussion 1911, Wien 1913, Ehrenfest and Kamerlingh Onnes 1915, can be summarized as follows:

1. The division of fluctuation into two terms is artificial (Planck 1910, Discussion 1911). With the help of dimensional considerations, Einstein estimated that the classical theory could only account for the second term

in (3), but he failed to provide a direct proof. It might still be possible to derive the complete formula (3) from the wave theory of light (Planck 1910).

2. Radiation in a cavity with perfectly reflecting walls without absorption or emission is non-ergodic. Wave modes with different frequencies cannot exchange energy among themselves and hence thermal equilibrium cannot be established. Therefore, in general, statistical methods are not applicable to a system consisting of pure radiation without taking into account the presence of emitting and absorbing matter.

3. The first term in (3) refers to the interaction between matter and radiation rather than to the structure of radiation itself. Planck thought that "first of all one should attempt to transfer the whole problem of the quantum theory to the area of *interaction* between matter and radiation energy; the processes in pure vacuum could then temporarily be explained with the aid of the Maxwell equations" (Discussion 1909, pp. 585–586). This opinion was a rather widespread one, shared by most participants of the debate.

4. In order to derive formula (1), one had to assume the additivity of entropies of the system and the reservoir. It was not clear whether this was, indeed, the case for the blackbody radiation. For example, one could not make such an assumption in the case of coherent rays (Discussion 1911).

5. Statistically independent light quanta (independent in the sense of Boltzmann's statistics) do not correspond to the Planck law. Einstein's hypothesis is consistent only with Wien's approximate formula (Ehrenfest 1911, Ehrenfest and Kamerlingh Onnes 1915).

Most of these objections are very serious, reflecting deep understanding of the problems involved, and they should not be downplayed as a failure to understand Einstein's genius. By and large, they reflected the prevailing mood of justified conservatism. There were still too many blank spots in quantum theory that could have possibly caused the difficulty. Moreover, light quanta failed to provide any better alternative to the classical theory because, as had been already pointed out by Lorentz, they could not account for solidly verified phenomena of interference and diffraction. It looked as if it was still too premature to declare the end of the wave theory of light.

The fates of these objections were different, as will be discussed in subsequent sections. Lorentz refuted the first one by a direct and explicit calculation. The fifth one proved to be entirely correct and was resolved, eventually, in the adoption of Bose statistics instead of the classical Boltzmann statistics for light quanta. Lorentz's attempt to formulate the third idea in mathematical terms succeeded only partially. The issue remained unresolved and doubts about the need to quantize pure radiation would still be expressed at least until the late 1920s – early 1930s. They disappeared only gradually with the gradual establishment of quantum electrodynamics. Debates pro and contra the second and the forth objections remained on the level of verbal rather than mathematical discourse and did not disappear until many years later. They would still be remembered as a possible source of problem on a number of occasions, for example by Ehrenfest (1925a, 1925b) and by Heisenberg (1931), whenever the emerging quantum electrodynamics encountered new difficulties.

In view of the existing conflict of opinions, the fluctuation problem remained without an accepted solution for a number of years. Taking one side or the other was a matter of taste or fashion. The fashion, however, was changing. In the early period, it was established by the first Solvay meeting in 1911, which achieved a certain compromise concerning the theory of radiation. On the one hand, fluctuations were recognized as an “anomaly,” but on the other hand, the line of “justified conservatism” prevailed and the small community of concerned theoretical physicists preferred to stick for the time being to the wave theory of light while placing the causes of all encountered difficulties in the yet unknown process of the interaction between light and matter.

Einstein’s position also shifted towards a compromise, although he probably did not agree with any of the aforementioned critiques. In his report to the Solvay meeting (Einstein 1911), he modified the derivation of the fluctuation formula in order to circumvent the second objection, but the final result remained unchanged. He also became much more cautious in interpretation. Whereas previously Einstein had emphasized the importance of the first term in equation (3), by 1911 he had become aware that the second term contradicted the hypothesis of light quanta just about as strongly as the first one contradicted the wave theory of light. He thus called the problem an “unsolved puzzle,”and the hypothesis of light quanta “a provisional attempt, . . . auxiliary idea, which does not seem compatible with the experimentally verified conclusions of the wave theory.”[12] No longer insisting on his initial proposal, Einstein had to acknowledge his inability to solve the problem.

Most of the opponents of light quanta conceded, on their part, the general correctness of formula (3) as well as the impossibility of obtaining its first term from the wave theory of light. They hoped the solution would be found later and were inclined to see the first term as originating from the obscure process of the interaction between radiation and matter. This opinion, which was very close to consensus, was presented in the most careful and clear way in Lorentz’s Paris lectures of 1912, which were published with a significant delay caused by the war in Europe (Lorentz 1916).

Although Lorentz's lectures covered a textbook topic, statistical mechanics, some parts, and in particular the treatment of fluctuations, constituted original new contributions to the field. In Lorentz's presentation, fluctuations were crucial for the understanding of the entire theory of blackbody radiation. Equation (3) was for him as fundamental as the Planck law: both were strictly equivalent and could be derived from each other, both were challenging the established theories in most serious ways (in the case of the Planck formula, the problem was in its derivation and in the interpretation of the constant h), and both, in Lorentz's expectation, were the most important keys towards further progress in theoretical physics. Subsequent developments confirmed his judgement: various approaches to the derivation of the Planck formula had their parallel attempts at resolving the difficulty with fluctuations, similar models were applied to both problems and usually with a similar degree of success. The following sections discuss these attempts to find the solution. There was quite a variety of them, as is expected at the time of crisis.

2.3 RESOURCES OF THE WAVE THEORY

Was the wave theory of light, in fact, incapable of explaining both terms in formula (3)? Einstein's assertion was not indisputable. Despite his firm conviction that classical theory only accounts for the second term, he was unable to provide a direct calculation and, at least for the time being, there remained some room for doubt. The question was clarified only gradually.

The first step was made by Einstein and Ludwig Hopf in 1910 (Einstein and Hopf 1910). In order to circumvent Planck's objection, they did not apply statistical methods to wave modes of the electromagnetic field, but only to material oscillators. Having diligently calculated the radiant pressure of light and the fluctuations of the momentum transmitted from the field to an oscillator according to the classical theory, they obtained the result that corresponded to the second, wave term in the fluctuation formula and to the Rayleigh–Jeans, instead of Planck's, law of energy distribution. Because of the then almost universal rejection of the hypothesis of light quanta, Einstein and Hopf did not dare to mention them explicitly in the paper, concluding it with a modest statement that intrinsic contradictions of the theory of radiation could not be resolved by forbidding the application of statistics to radiation.

Planck's former student Max von Laue came out in defense of the wave theory (Laue 1915b). Although he did not directly discuss fluctuations in his paper, his objections were obviously rooted in disagreement over the interpretation of the fluctuation formula in the preceding paper (Laue 1915a). Laue questioned one of the basic assumptions made by Einstein and Hopf, namely, that the Fourier coefficients of the wave modes are statistically independent variables. As a possible physical cause of additional fluctuations, Laue suggested, without elaborating, that the system's effective number of degrees of freedom decreases with the transition to quantum domain due to some yet unknown mechanism.

The debate continued in a couple of further papers (Einstein 1915, Laue 1915c) without any noticeable progress. Subsequent polemic died off either by itself or because of the publication of Lorentz's lectures. Lorentz furnished Einstein's claim that the classical theory is responsible only for the second term in energy fluctuations with a long, elaborate, and straightforward mathematical proof (Lorentz 1916, Appendix IX). He considered radiation in a cavity as a superposition of Fourier modes with statistically independent coefficients. Their interference with each other made the energy within a portion of the cavity fluctuate in time. Lorentz managed to calculate these fluctuations directly, and his result coincided with Einstein's earlier estimates leading exactly to the second term in formula (3).

Lorentz's mastery of mathematical calculations and his authority convinced almost everybody, as is obvious from references in later publications. Planck alone stuck to his earlier ideas and repeated once again Laue's argument as late as 1924. But even he had to acknowledge in the introduction to his paper that, because of Lorentz's calculations, "an opinion about the incompatibility between the classical theory of propagation of light and the requirements of the quantum theory has found much stronger support and is presently widespread" (Planck 1924, p. 273). By that time it was much more than merely an "opinion," but a generally accepted point of view.

2.4 QUANTIZED INTERACTION WITH MATTER

In the same course of Paris lectures, Lorentz also managed to translate into mathematical equations the idea that the first term of fluctuations originates from discontinuities in the processes of the emission and absorption of light by matter rather than by the structure of light itself. Lorentz did not require any precise model of such interaction: already the assumption that matter and radiation exchange energy in finite quantitites $h\nu$ sufficed for him to derive the first, quantum term in equation (3) (Lorentz 1916, pp. 73–76). With the second term already calculated from the interference of radiation waves, Lorentz thus explained the complete formula (3) as resulting from the combination of two independent causes.

In his own judgment, the explanation was only partially satisfactory. The idea worked well in the case of the system represented by the radiation inside the cavity of volume V and the reservoir represented by matter, or the walls of the cavity. But Einstein's formula was also supposed to be equally valid in the case of the system consisting only of a portion U of the whole volume V of the cavity and the reservoir represented by the walls and by the remaining volume $V - U$. Thermodynamical calculation would give for this case

$$\overline{\varepsilon^2} = h\nu e + \frac{c^3}{8\pi\nu^2 U d\nu} e^2, \tag{9}$$

where $e = EU/V$ is the average energy in the volume U. Leonard Salomon Ornstein and Frits Zernike (Ornstein and Zernike 1919) applied Lorentz's approach to this case but obtained for the first fluctuation term only a portion of the required result

$$\frac{U}{V} h\nu e = h\nu \frac{e^2}{E}. \tag{10}$$

Despite its failure to deliver the full formula (3) in this specific case, Lorentz's calculation appears to have inspired Einstein to another major breakthrough in the field. Lorentz's basic approach consisted in considering the emission and absorption of radiation as statistically random acts and the resulting radiation in the cavity as statistical equilibrium. He assumed that the exchange of energy between matter and radiation takes place only in quanta $h\nu$ and that the absorption of radiation is proportional to the radiation density $\rho = E/V$. These assumptions are practically identical with the assumptions in Einstein's famous 1916 derivation of the Planck law in which he first introduced the probability coefficients $A^n{}_m\downarrow$ for spontaneous, $B_n{}^m\uparrow$ and $B^n{}_m\downarrow$ for induced radiation transitions. Einstein's paper appeared later the same year in which Lorentz's lectures were published, after several years of impasse in Einstein's quantum studies during which he failed to achieve any substantial progress in solving the riddle of radiation and was mainly preoccupied with the completion of his theory of general relativity (arriving at its basic equations in November 1915 and studying further consequences, such as gravitational waves and conservation laws, throughout 1916). The published version of the paper (Einstein 1916) contains no references and practically no clues to what could be the incentive and the starting point for this work. It appears very likely that Einstein was influenced by reading Lorentz’s derivation of the fluctuation formula.

There is a strong internal relationship between the two calculations. In his discussion of the fluctuation formula, Lorentz characterized it as being essentially equivalent to the Planck law and thus having the same fundamental importance. A possible solution of one of these problems would also offer the way to solve the other one. While Lorentz in his lectures applied the model of statistical equilibrium in discontinuous energy exchanges between matter and radiation to the derivation of the fluctuation formula, Einstein in his paper used a very similar approach in order to produce a new derivation of the Planck law. Starting from Lorentz’s basic model, Einstein added to it two important innovations. First, he used a more detailed mechanism of energy exchanges that implicitly relied on Bohr’s idea of quantum states of an atom and radiation transitions between them. In Einstein’s notations, Lorentz’s statistical equation would be written as

$$A\binom{n}{m}\downarrow \;=\; \rho B\binom{m}{n}\uparrow e^{h\nu(n,m)/kT}, \tag{11}$$

where $\rho = E/V$ is the radiation density, while $\nu(n,m)$ is its frequency corresponding to the transition $n \rightarrow m$. Since Lorentz was concerned in this part of his lectures only with the explanation of the first term of the fluctuations, this equation corresponded to the Wien law for ρ. Its mathematical form, however, suggests a one-step modification required in order to obtain the Planck law, namely the subtraction of 1 from the right-side exponent. This would be tantamount to adding a new term, the stimulated emission of radiation, which is the second and the most important novelty of Einstein’s paper.

Einstein’s correspondence with Lorentz provides some further support for the above historical reconstruction. It appears from it that in the course of 1915 and the first half of 1916, Einstein was working full time on general relativity. In a postscript to the letter of 17 January 1916 he thanked Lorentz “for sending your lectures on statistical mechanics,” which had just been published,[13] and returned to the discussion of the book later, in the letter of 17 June, in a paragraph that establishes the transition from Einstein’s preoccupation with gravitational theory back to his earlier concerns about quanta:

> I am not quite finished yet with the theory of emission in material systems.[14] But this much is clear to me: that the quanta difficulties affect the new theory of gravitation just as much as Maxwell's theory. I was delighted that in your Parisian lectures you gave the fluctuation properties of radiation deservedly thorough discussion; there the theories' inaccuracies become most clearly evident.[15]

Exactly one month later, on 17 July 1916, Einstein's paper "Strahlungs-Emission und -Absorption nach der Quantentheorie" was received by the editors of the *Verhandlungen der Deutschen Physikalischen Gesellschaft.*

2.5 QUANTIZATION OF WAVES

Classical waves and light quanta were not the only models of radiation applied to the explanation of the fluctuation formula. Another, more complex one, relied on the idea of quantized waves, or stationary wave modes of the cavity with discrete energy levels separated by the interval $h\nu$. The model was first proposed by Ehrenfest as a possible interpretation of the Planck law (Ehrenfest 1906), and later also mentioned by Lorentz (Lorentz 1910) and Debye (Debye 1910), although in the latter case in a combination with light quanta. At that initial stage, physicists' attention was devoted mostly to the problem of discontinuities in energy values, but once that feature of the new theories had become firmly established, discussion shifted to the problem of statistics.

Two different ways of introducing statistics of microscopic states appeared already in Planck's *Vorlesungen* (Planck 1906), where they were both applied to material oscillators.[16] According to the first of them, the energy quantum $h\nu$ is chosen as a microscopic object that can be distributed over different oscillators (whether material oscillators of atoms, or radiation modes, or vibrations of a solid body). To introduce statistics, one assigns equal probabilities to different microscopic distributions of quanta over the oscillators. In order to obtain the Planck formula, one had to deal with these quanta as indistinguishable entities, assuming, either explicitly or implicitly, that microscopic distributions are defined only by the number of quanta in each oscillator state,[17] a procedure essentially equivalent to the later Bose–Einstein statistics for light quanta.

The other way to introduce statistics was to choose oscillators as microscopic objects and distribute them over the discrete set of energy values 0, $h\nu$, $2h\nu$, $3h\nu$, . . . as possible states. One can derive the Planck formula this way without postulating microscopic indistinguishability: probabilities for oscillators correspond to the usual Boltzmann statistics of distinguishable objects. Both methods lead to the same result at the end, the Planck law (2). I will call the first of them "the statistics of quanta," and the second "the statistics of oscillators."[18] The basic combinatorial formula is also the same in both methods, only its interpretation differs: in the first case, energy quanta are chosen as "distributed objects" while oscillators represent "boxes over which the objects are distributed"; in the second case, it is the other way round.

The statistics of oscillators had already been used by Planck in application to material oscillators (Planck 1906). Later the same year, Ehrenfest suggested that it be applied to electromagnetic oscillations in a cavity, thus introducing a new model, that of quantized radiation waves (Ehrenfest 1906, Ehrenfest 1911), which offered a new way of deriving the Planck law (2). The clearest and most laconic presentation of this method came from Laue in (Laue 1915a), where he used the model of quantized waves to derive fluctuation formulas (3) and (4) for blackbody radiation as well as for two other kinds of systems.[19]

The formalism of quantized waves allowed Laue to obtain, for the first time, both fluctuation terms from a single microscopic model. Despite this apparent achievement, the model of quantized waves did not attract much attention in those years. Although it was known to most of concerned experts in quantum theory, the method did not become as famous and widely discussed as the model of light quanta. The advocates of both rival approaches—the wave and the corpuscular ones—apparently regarded the quantization of waves as a merely formal trick. Einstein never discussed it in a published form in connection with the problem of radiation. Although Laue and Planck used this model at some points, they both abandoned it soon thereafter in order to return to the model of classical waves (Laue 1915b, Planck 1924). Bohr discussed it once as a plausible one, still preferring classical waves and definitely rejecting light quanta (Bohr 1921).

Among the main experts, Ehrenfest appears to be the only one who took the approach seriously and returned to it quite regularly over an extended period of time. He applied it once again in 1925 to the remaining difficulty in the derivation of the fluctuation formula (3). After von Laue's 1915 paper, only one problem remained: the case when the system was represented by a partial volume U of the whole cavity V. Ehrenfest's detailed calculations on quantized waves led to the same unsatisfactory result as the earlier attempt by Ornstein and Zernike with the model of quantized energy exchange between radiation and matter: he obtained the full second term but only a portion of the first term (Ehrenfest 1925a and 1925b).

2.6 MOLECULES OF LIGHT

If quantized waves can be regarded as a quantum mutation of classical waves, one could similarly try to modify somewhat the model of light quanta in order to bring it in correspondence with the Planck law. The existing contradiction, which amounted to differences in statistics, was clarified largely thanks to the efforts by Ehrenfest. He explained that statistically independent energy quanta led directly to the Wien law, while in order to obtain the Planck law, one had to assume that quanta were not independent (in the classical sense of the term, which was then the only available one) but indistinguishable objects (Ehrenfest 1911).

This peculiarity of Planck's combinatorics was also understood around the same time by Ladislas Natanson (Natanson 1911) and a few years later explained with ultimate clarity by Ehrenfest and Kamerlingh Onnes (1915), who formulated the statistics of indistinguishable objects in comparison with the statistics of independent, or distinguishable objects in exactly the same way in which contemporary textbooks explain the difference between the Bose–Einstein and Boltzmann statistics.[20] Their understanding, however, did not immediately become part of the common knowledge in the field, which led, in particular, to further polemics in 1914, between Mieczyslaw Wolfke and Yuri Aleksandrovich Krutkov (Wolfke 1914a and 1914b, Krutkov 1914a and 1914b). Wolfke had published a derivation of the Planck law from realistically interpreted light quanta and came under critique by Krutkov, Ehrenfest's student from St. Petersburg who was visiting Leiden at the time,[21] for the unawareness of the exact kind of statistics required for the derivation. Krutkov demonstrated explicitly that the application of Boltzmann statistics to light quanta immediately yields the Wien law. Krutkov mentioned an additional possibility in his paper: one could derive the Planck law from the model of independent particles if, in addition to the ordinary light quanta $h\nu$, the quanta of multiple energies $2h\nu$, $3h\nu$, . . . were also present.

The idea originated from a mathematical identity

$$\frac{1}{e^{\alpha}-1} = \sum_{i=1}^{\infty} e^{-i\alpha}, \tag{12}$$

which, if used to reformulate the Planck law (2), transforms it into an infinite series with the first term given by the Wien law (6) and with similar further terms only involving quanta $2h\nu$, $3h\nu$, ... Krutkov mentioned this idea only as an illustration of his (or rather Ehrenfest's) thesis that the original hypothesis of light quantum could not fully account for the blackbody radiation, but without actually arguing for the existence of multiple light quanta. According to Krutkov, the idea was suggested by his St. Petersburg colleague Abram Ioffe. Indeed, Ioffe was apparently the first to mention multiple quanta in print (Ioffe 1911, p. 550), if only in passing and as something already known. It is likely that the possibility of interpreting the Planck law this way had been discussed around 1910 within the St. Petersburg circle of physicists—in which Ioffe and Ehrenfest were major participants—but considered of no real importance.

Wolfke, on the contrary, having learned the idea from Krutkov, started to advocate the real existence of the "molecules of light." This became possible due to a general change of attitude in physics following the end of World War I. At the time of the first Solvay Conference of 1911 and for several years thereafter, light quanta were considered too radical, viewed rather negatively and, as a result, practically disappeared from physical journals. Einstein himself either changed his mind or yielded to the general mood. This attitude, however, changed dramatically after the end of the war: by 1920 light quanta grew out of oblivion into an extremely popular concept and began to be widely understood as particles or corpuscles. Traditional historiography saw the explanation of this change in the discovery of the Compton effect in 1923, but the development had already been in place for several years before that and was crowned by, rather then caused by, Compton's landmark achievement. Before the new experimental evidence became available, the post-war wave of publications on light quanta usually justified them by reference to Einstein's theory of transition coefficients (Einstein 1916, Einstein 1917).[22]

Although that paper by Einstein neither provided any new argument in favor of light quanta nor even dared to mention them explicitly, it could still be used as a kind of psychological argument since its results could very easily be visualized and interpreted with the help of light quanta. Rather than being caused by new experimental or theoretical developments, the revival of light quanta appears more like a shift in the prevailing fashion among the physicists. Most of the authors who started using this concept soon after the end of the war actually belonged to a younger generation who also favored different approaches to physical problems. If the first generation of quantum theorists (including Planck, Einstein, and Ehrenfest) were strongly influenced by statistical mechanics and thermodynamics,[23] the younger generation often lacked a thorough background in statistical methods, preferred dynamics to statistics, and tended to discuss physical processes in terms of individual microscopic events. The comeback of dynamical theories occurred in a number of different ways, the revival of light quanta being only one of them.

The spectrum of problems that occupied physicists also shifted from the discussion of blackbody radiation towards atomic spectra and the interaction between radiation and atoms. However, some of the older unsolved problems, including the derivation of the Planck law and Einstein's fluctuations, were not completely forgotten. The general rise in the popularity of light quanta as realistically interpreted corpuscles paved the way to considering such modifications of them as the combination of quanta into "molecules of light."

Wolfke recollects the idea in (Wolfke 1921). For each Wienian term in the series, he calculated the probability of spatial concentration of the entire energy of radiation in a smaller fraction of volume V and obtained the series of the fluctuation formulas (7) for each type of multiple quanta, concluding from this that light molecules really existed in radiation. Apparently, Louis de Broglie independently arrived at the same model of multiple quanta and used it in de Broglie 1922a and 1922b to derive the Planck law and the fluctuation formula (3). Once again, the same possibility was discovered by Walther Bothe (Bothe 1923 and 1924), although Bothe preferred the term "multiples" rather than "molecules" and did not consider multiple light quanta as bound to one another, but only as coherent and moving closely in the same direction. He thought that such "multiples" could actually be created in induced radiation transitions.

Although the "molecules of light" approach resembles the later Bose–Einstein statistics in this particular respect that light quanta within such a molecule would lose their individuality, one should not confuse molecules of light with the mathematical equivalent of Bose cells with several indistinguishable quanta.[24] The number of Bose cells with i quanta, as derived by Satyendra Nath Bose (1924),

$$p_i = A(1 - e^{-h\nu/kT})\, e^{-ih\nu/kT}, \tag{13}$$

where $A = 8\pi V\nu^2 d\nu/c^3$, differs from the number of light molecules $ih\nu$ required for the derivation of the Planck formula

$$n_i = A\frac{e^{-ih\nu/kT}}{i}. \tag{14}$$

It is difficult to assign any reasonable statistics to the model of light molecules. For example, if one assumes that quanta form a molecule when they are put in one cell, so that each molecule and each single quantum occupy a separate cell, then the total number of available cells *A* will not be sufficient to contain all the quanta

$$N = \sum_{i=1}^{\infty} i n_i \tag{15}$$

for the region of short waves ($h\nu << kT$). This explains, perhaps, why no attempt was made to formulate any kind of statistics in the papers on light molecules. The proposal completely disappeared from journals after the introduction of Bose–Einstein statistics.

3. Duality and Dualism

3.1 BOSE STATISTICS

Ehrenfest was unlucky in that he understood some problems, in particular, the discrepancy between the statistics of independent quanta and the statistics of indistinguishable energy quanta, too well, perhaps better than anybody else. Bose, on the contrary, was luckily unaware of the difficulty and of the boldness of his calculations when he authored a paper contradicting the classical statistics (Bose 1924): "I had no idea that what I had done was really novel. . . . I was not a statistician to the extent of really knowing that I was doing something totally different from what Boltzmann would have done, from Boltzmann statistics."[25]

Like many other younger enthusiasts of light quanta, Bose was not as well trained in statistical mechanics as the generation of Einstein and Ehrenfest. Thus, it can be said that an "erroneous or opportunistic transposition of [combinatorical] formulas resulted in what we now call the Bose–Einstein statistics" (Darrigol 1991, p. 239).

But what was obscure to Bose should have been well known to Einstein, at least from earlier publications by Ehrenfest, his close friend and colleague. Einstein, however, had demonstrated an astonishing indifference to the problem: neither his publications nor, to the best of my knowledge, correspondence, contain a response to Ehrenfest's criticism or any other remark on the issue of independence or distinguishability. Even if he did not agree with Ehrenfest, he did not express the disagreement either. Darrigol suggests that Einstein and Ehrenfest stood behind the polemics between Wolfke and Krutkov in 1914 (Darrigol 1991, p. 256), but while Krutkov was obviously representing Ehrenfest's view that light quanta, when taken as independent particles, cannot explain the Planck law, it is much less clear to what degree the somewhat obscure ideas of Wolfke—that quanta must not be necessarily independent, or strictly independent, or independent in one sense or another—represented Einstein's views, despite Wolfke's allusion to his discussion with Einstein (Wolfke 1921).

Even in his first paper on the ideal gas with Bose statistics, Einstein did not mention the peculiarity of statistics at all (Einstein 1924b). As the main result of his paper, Bose regarded the purely corpuscular method of deriving the factor $8\pi \nu^2 V d\nu / c^3$ in the Planck law (2). Rather than representing the number of wave modes of radiation, the factor corresponded, according to Bose, to the number of cells in the phase space of light quanta. Einstein, apparently, also saw, in this aspect, the main novelty of the approach:[26]

> Bohr, Kramers and one more have abolished the "loose" quanta. These, however, do not allow us to get along without them. The Indian Bose has given a beautiful derivation of Planck's law *together with the constant* on the basis of loose quanta. Derivation is elegant, but its nature remains unclear. I have applied his theory to the ideal gas. Strict theory of "degeneracy." No zero-point energy and no energy defect. God knows whether this is so.[27]

Einstein first commented on the issue of statistics only three months later, in his second publication on the Bose gas, responding to a letter from Ehrenfest:

> Ehrenfest and other colleagues blame Bose's theory of radiation and my theory of ideal gas because in these theories quanta and, correspondingly, molecules are not treated as statistically independent objects, and what's more, this fact was not mentioned in our papers. That is true. (Einstein 1925, p. 5)

Einstein thus acknowledged for the first time the existence of the problem, but could justify the new procedure only by its successful applications. Long after it had become generally accepted, Bose statistics continued to worry a few concerned physicists, but the interpretation that eventually became predominant consisted in turning the assumption and the peculiarity into a postulate. The quantum statistics was declared fundamentally, a priori, different from the classical one and requiring no further explanation than the statement that "particles are indistinguishable." This possibility was suggested almost immediately, for example, in Landè 1925, and was later introduced and became standard in the new quantum mechanics (Dirac 1926), together with the second kind of indistinguishable statistics, the Fermi statistics.

The Bose method of deriving the Planck law can also be applied to fluctuations and deliver the full formula (3) with both its terms interpreted as fluctuations in the number of particles N within the given volume V.[28] Einstein demonstrated this immediately in his paper (Einstein 1925), obtaining

$$\frac{\overline{(\Delta N)^2}}{N^2} = \frac{1}{N} + \frac{1}{Z}\,. \tag{16}$$

No additional difficulty emerged even for the case of a system represented by the part U of the entire volume V of radiation. If one accepts the fundamentality of the statistics of indistinguishable particles, the problem can be regarded as solved without any reference to either waves or the wave-particle duality. This was not, however, the option chosen by Einstein. For him, acknowledging that Bose quanta were not independent did not mean that the classical notion of statistical independence had to be abandoned and indistinguishability accepted instead as a fundamental postulate. Like Ehrenfest and several other theoreticians, mostly of the statistically conscious older generation, Einstein was trying to find a deeper explanation behind the appearance of indistinguishable statistics.

The other available option consisted in switching from the statistics of (indistinguishable) quanta to the statistics of (distinguishable) oscillators. This possibility of interpreting the results of Bose was most probably preferred by Ehrenfest and quickly proposed in a published form by Schrödinger and independently by Pascual Jordan (Schrödinger 1926; Born, Heisenberg, and Jordan 1926). This would also mean choosing quantized waves rather than light quanta as the fundamental object of the new quantum theory, thus shifting the wave-particle balance towards the wave side. Einstein, who had never used Ehrenfest's model of quantized waves, apparently continued to have reservations about this option in 1925 and did not choose it, either. Instead, he suggested that the failure of independence of light quanta should be explained by some yet unknown kind of interaction between them, the interaction that had properties of a wave field. At this point Einstein referred to the dualistic model of de Broglie (de Broglie 1924), who had proposed to associate every particle with a scalar field.[29]

3.2 TURNING TO DE BROGLIE. EINSTEIN ON FIELD AND QUANTA

After his response to Lorentz's objections in the 1909 Salzburg talk, Einstein's vocabulary changed. He was becoming increasingly aware of the difficulty in combining the concepts of waves and quanta (he still avoided using the word "particle" or "corpuscle"), which occupied much of his thought. At first, he did not view the problem as insoluble and actively tried to develop a field-like theory of light quanta and thus to understand the meaning of the Planck constant "in an intuitive (visual) way" ["in anschaulicher Weise"] (Einstein to Sommerfeld, 14 January 1908, Einstein 1993b, Doc. 73, p. 87). Brief remarks in Einstein's letters give the chronology of his multiple attempts. Early in 1909 he looked for a nonlinear and nonhomogenous differential equation that would have explained both the electron and the light quantum, by reducing the number of fundamental constants from three to two (Einstein 1909a). He then tried linear and homogenous equations with singularities, as explained in his letter to Lorentz. After a year of unsuccessful attempts, Einstein attempted a radically different approach:

> At the moment I am very hopeful that I will solve the radiation problem, and that I will do so *without light quanta*. I am awfully curious how the thing will turn out. One would have to give up the energy principle in its present form. . . . I no longer believe (at the present) in spatial light quanta.[30]

Even sacrificing the strict conservation of energy did not help, and in 1911 Einstein stopped his active search for models and turned to more formal applications of quantization:

> I no longer ask whether these quanta really exist. Nor do I try to construct them any longer, for I now know that my brain cannot get through in this way. But I rummage through the consequences as carefully as possible so as to learn about the range of applicability of this conception. (Einstein to Michele Besso, 13 May 1911, Einstein 1993b, Doc. 267, p. 295)

Over years, after many unsuccessful attempts to solve the riddle of the theory of radiation, Einstein's views shifted from earlier revolutionary emphasis on the atomicity of light towards gradual acknowledgment of the symmetrical nature of the difficulty. He became increasingly concerned about the inability of light quanta to account for wave-like properties such as interference and diffraction, which, for him, rose to the same level of difficulty as the inability of the classical theory to account for quantum effects in radiation. Already in 1917, he made a remark that his theory of spontaneous and induced radiative transitions made "the establishment of a quantum-like theory of radiation appear as almost unavoidable," followed by "[T]he weakness of the theory is . . . that it does not bring us closer to a link-up with the undulation theory."[31] Even with corpuscular light quanta becoming immensely popular among the younger generation of post-war physicists, Einstein remained less enthusiastic, expressing views that the ultimate solution would have to combine features of both classical and quantum theories of light.

Einstein repeated these cautious attitudes in the midst of the triumph of light quanta after the discovery of the Compton effect in 1923:

> Newton's corpuscular theory is reanimated again, although it proved to be completely unsound in the domain of geometrical properties of light. We have now, therefore, two theories of light; both are necessary, and both, we have to admit this, exist without any logical connection, despite the twenty years of tremendous efforts of theoretical physicists. (Einstein 1924a)

Establishing this logical connection between two equally indispensable theories became Einstein's foremost concern. Solving the wave-quantum problem, in his view, was the biggest challenge for quantum theory and the criterion of its success. In 1921 he hoped to design an experiment that could distinguish between the quantum and classical mechanisms of light emission (Einstein 1921). In connection with it, he discussed, in correspondence with Lorentz and Ehrenfest, the idea of *Gespensterfeld* ("ghost field"), according to which both waves and quanta were emitted in a process of radiative transition. Waves were responsible for interference but carried no energy (perhaps because their amplitudes were small). Waves, however, directed the motion of quanta, which transported energy (see Lorentz to Einstein, 13 November 1921 and Einstein to Ehrenfest, 11 January 1922, EA 10-003). Einstein did not publish his theory and abandoned it soon after his proposed experiment failed. He mentioned once again the idea of the statistical conservation of energy, but rejected it again and even more strongly than in 1911.[32] Another idea, which he considered repeatedly, was that quantum restrictions arise due to overdetermination in the system of differential equations, if the number of equations is larger than the number of independent variables. Although unable to develop this idea very far, Einstein mentioned it in a published paper (Einstein 1923), as well as in his later attempts at a unified field theory.

Altogether, combining field and quantum aspects in a single model of radiation was the most important goal for Einstein. He was open to more than one possibility, yet always looked towards realistic, *anschauliche*, combinations. Although de Broglie did not achieve such a definite solution, Einstein regarded his attempt as a very promising hint pointing in the right direction: “He lifted the edge of a large veil” [“Er hat einen Zipfel des grossen Schleiers gelüftet”] (Einstein to Langevin,16 December 1924, EA 15-377), and in this sense referred to de Broglie’s work when trying to answer Ehrenfest’s critique regarding the difficulty with the new statistics (Einstein 1925).

Einstein’s choice, however important, was not a logical necessity but reflected his reluctance to accept any of the other two available options: the fundamentality of indistinguishable particles or quantized waves. His supportive reference to de Broglie was interpreted by most readers as an assertion of the wave-particle duality. What was the meaning of duality in popular interpretation and how true was Einstein’s reputation as a dualist? The answer depends on some boring terminological distinctions.

3.3 EINSTEIN’S REPUTATION AS A DUALIST

Before it entered physics, the word *Dualismus* was used in theology and philosophy. Articles in encyclopedic dictionaries, borrowing from one another in a sequence, usually attribute the first use of the term to Thomas Hyde’s 1700 history of the Persian religion. The original source of this attribution, Rudolf Eisler, referred to Hyde’s book more cautiously as merely an example of the early meaning of the term which was related to the fundamental theological problem of Evil (Eisler 1909). From the point of view of Christian theology, Persian Zoroastrianism, as well as a number of Christian heresies, were dualistic because they regarded the Good and the Evil as the two equally fundamental elements of the world. Christian orthodoxy, on the contrary, accepted St. Augustine’s monistic solution, according to which only the Good was real, while the Evil had only a negative existence, as the relative lack of Good.

In philosophy, Christian Wolff applied the term *dualistae* in 1732 to those who, like himself, accepted the existence of two fundamental substances, material and immaterial. The philosophical meaning of the word as the opposition to both idealism and realism became standard, and philosophically educated German physicists of the early twentieth century were certainly aware of it. It is not easy to establish who first brought the term into physics. Neither Einstein nor de Broglie, to my knowledge, had used it, but by 1927, *Dualismus* had already been present in a number of German-language physical papers and typically attributed to Einstein’s and de Broglie’s views (sometimes with disapproval). From Bohr’s Copenhagen perspective, at least, dualism was not consistent as a philosophy of quantum physics and had to be replaced by complementarity. Professional philosophers concentrated on debates about complementarity and remained largely aloof to the issue of the wave-particle duality, which figures more prominently in popular and textbook accounts of quantum physics. As a result, duality remained a rather vaguely defined, or rather undefined, concept.

A minimal pragmatic definition would probably be one that would distinguish between the typical reasoning about waves and about particles in, correspondingly, classical and quantum physics. Classical physics at the turn of the century knew both waves and particles, as well as a variety of their combinations, yet the tension between the two concepts was largely derived from a more fundamental opposition between action-at-a-distance and field theories, or continuous and discontinuous descriptions. Whether certain phenomena and objects were particle-like or field-like in nature was often unclear. In classical electrodynamics of the late 19th and early 20th century there were many attempts to get rid of the particles by treating the electron as an artefact of the field, while on the other hand, electrodynamics could also be reformulated as an action-at-a-distance theory without any field at all. A variety of more complex combinations of fields and particles was also discussed, the assumption being, however, that each element would be of either one or the other distinctive type. Most of Einstein's earlier and later proposals to solve the basic problem of radiation belonged to this category, many of his models had direct roots in the theories of the electron at the turn of the century. If one wants to call them "dualistic," the same term should probably also be applied to the early twentieth-century electrodynamics.

The failure of his and others' many attempts resulted in the opposition of waves and particles attaining more fundamental importance in quantum physics compared to what it had in the classical period and, gradually, to the widespread acknowledgment of the impossibility of making the ultimate *either-or* distinction. Complementarity tends to say *neither-nor* instead, regarding microscopic quantum objects as "*unanschaulich*" in principle. Werner Heisenberg addressed the problem directly in his classical Chicago lectures (Heisenberg 1930). According to him, both waves and particles are classical notions that prove equally inadequate when applied to quantum phenomena, yet both are also equally indispensable as the only available means for physicists to express their intuitions and interpretations. The ultimate quantum description is, therefore, abstract, while visual models of waves and particles can only be used with certain reservations and should not be taken too literally.

De Broglie, Schrödinger, as well as many popular and textbook expositions of quantum physics, on the contrary, tended to say "*both together*": quantum objects have the properties of both wave and particles, which could not be separated. In the actual practice of physics, one encounters unrestricted opportunism. Two mathematical formalisms—one involving a particle obeying quantum laws of motion and the other involving quantized waves—exist, none of which uses waves and particles on an equal basis. Both formalisms are used rather interchangeably, depending on the context and on convenience, with the assumption that results would always be equivalent (and, at least in many cases, this can be proven). Physicists learned which of the existing visual intuitive languages to apply to which problem for the discussion and interpretation of results, and how to do this without getting into arguments or contradictions. It might be convenient to use two somewhat different words, 'dualism' and 'duality', in order to distinguish between ontological statements by some physicists about the centaur-like nature of quantum objects, which is a kind of philosophical interpretation, and the opportunistic freedom of the actual practice of modern physics in using different languages while avoiding the polemics.

What would Einstein's place be in this classification? He realized that both wave- and particle-like descriptions were indispensable but did not fit well together, but was reluctant to abandon the classical ideal of visualization and to accept as an ultimate solution either any of the available quantum mutations of classical models—indistinguishable particles or quantized waves—or even their interchangeable, opportunistic use. Einstein recognized duality in a negative rather than positive sense, as a crucially important problem rather than as a basic principle, a feature of quantum theory with which he struggled for most of his life without much success.

Einstein's 1925 paper on quantum gas in which he acknowledged duality as the main problem which had to be solved in, or rather eliminated from, the quantum theory was his last great contribution to the field's mainstream development. With the nascence of the new quantum mechanics, he was becoming more of an outsider looking for deviant strategies. Throughout 1926, while most quantum theorists were occupied with Heisenberg's and Schrödinger's new schemes, Einstein was struggling with the fatal problem of distinguishing between waves and quanta. He designed another experiment aimed to clarify whether the process of the emission of radiation by an atom occurs instantaneously or takes a certain amount of time, and spent much time and effort discussing the details and the preparation of this experiment with Emil Rupp. Rupp's experimental data and Einstein's more refined theoretical considerations forced him to abandon the initial hope of finding new disagreements between experimental results and the predictions of the classical theory, and no further clarification of the respective roles of waves and quanta ensued (Einstein 1926a, Einstein 1926b, Rupp 1926).

Many representatives of the new quantum mechanics—not only Schrödinger, but also such important authors of matrix mechanics as Max Born and Jordan—were inspired by reading Einstein's 1925 paper, followed his advice to take de Broglie's ideas seriously, and accepted dualism as a positive principle and one of the basic foundations of quantum theory. Due to their works and frequent references to dualism as Einstein's (and de Broglie's) idea, Einstein's reputation as a dualist became solidly established. For many of them, quantum mechanics (or at least wave mechanics) was, in some way, a realization of the Einstein–de Broglie program. Einstein, however, viewed the situation differently. From early on, already at the end of 1925, he expressed some skepticism about matrix mechanics, and in early 1926 he did not become ultimately satisfied with wave mechanics either.

Einstein's criticism had developed already before quantum mechanics took the acausal turn and thus must have had different reasons than Einstein's later disagreement with the mature theory expressed in philosophical notions of causality and completeness. At the early stage, his cautious welcome of the emerging theory seems to be related to the fact that neither of its versions—even wave mechanics, Schrödinger's own claims notwithstanding—was able to solve satisfactorily, by Einstein's strict criteria, the crucial problem of waves and particles. A newspaper report of his lecture on 23 February 1927 on recent developments in quantum theory makes this point rather clearly:

> The principal issue arising before us in the field of light phenomena is as follows: either to show that the corpuscular theory captures the true essence of light, or that the wave theory is right and the quantum properties are only illusory, or, finally, that both conceptions correspond to the true nature of light and that light has both wave and quantum characteristics. There were attempts to find a synthesis of both these features, but they have not succeeded mathematically so far. The latest great progress in the theory of light has been achieved through returning back again from the corpuscular conception and making a step in a direction opposite to the one which had led us from the wave to the corpuscular theory. Einstein refers here to the works of de Broglie and Schrödinger. . . . Nature requires from us a synthesis of both properties, but so far, this has been way beyond the intellectual abilities of physicists.[33]

Just at the time when the wave-particle duality was about to be triumphantly proclaimed as one of the basic (some thought the most basic) and verified pillars of quantum mechanics, Einstein was still regarding it as the most troublesome mystery, which both the old and new quantum theories ultimately failed to solve.

4. Fluctuations in Quantum Electrodynamics

4.1 QUANTIZATION OF ELECTROMAGNETIC FIELD

Once the problem of the derivation of Planck's law could be considered solved by Bose statistics, the parallel problem of fluctuations lost its prominence as well. After 1925, formula (3) appeared more frequently in textbooks than in original research papers. The story could have ended at this point, if it were not for the role the fluctuation formulas played in the emergence of the new discipline of quantum electrodynamics.

Jordan, who would become one of the main creators of the new theory, had completed his doctorate with Max Born in Göttingen (Jordan 1924) and soon thereafter, in summer 1925, happened to attend a visiting lecture by Ehrenfest on the then still unpublished paper dealing with fluctuations and quantized waves (Ehrenfest 1925a, Ehrenfest 1925b). Jordan's thesis had been on the conflict between light quanta and the Bohr–Kramers–Slater theory of 1924, the last major attempt to rescue the classical theory of light at the expense of sacrificing the strict validity of energy conservation. Jordan thus had some background in the problem of radiation in the old quantum theory, though as a recent student he was not familiar with many earlier details and ramifications. From Ehrenfest, he learned about the remaining problem in calculating fluctuations: the case of radiation in the partial volume U of the entire cavity V.

Trying to solve the problem by his method of quantized waves, Ehrenfest obtained the same partial, and therefore unsatisfactory, result as Ornstein and Zernike with the Lorentz model of quantized interaction between radiation and matter. Both papers tried to explain the discrepancy verbally, referring to the uncertainty about whether it was justified to assume the additivity of entropies of the two parts of the volume. Ehrenfest also discussed with Göttingen physicists Einstein's recent theory of quantum gas, whereupon Born wrote to Einstein:

> Your brain, heavens know, looks much neater; its products are clear, simple and to the point. With luck, we may come to understand them in a few years'

> time. This is what happened in the case of your and Bose's gas degeneracy statistics. Fortunately, Ehrenfest turned up here and cast some light on it. Then I read Louis de Broglie's paper, and gradually saw what they were up to. I now believe that the wave theory of matter could be of very great importance.[34]

Together with Walter Elsasser, another student of Born's, Jordan, too, became an explicit advocate of the wave-particle dualism and of the symmetry between radiation and matter. These ideas influenced to a great extent his subsequent contributions to new quantum mechanics and electrodynamics.

Towards the end of August 1925, Jordan was already collaborating with Born on the development of Heisenberg's proposal of the new theory into its first relatively completed form, matrix mechanics. Both his paper with Born and the subsequent one, the famous "Dreimännerarbeit," which they wrote together with Heisenberg, included sections on electrodynamics authored by Jordan and dealing with the problem of the quantization of electromagnetic field and its energy fluctuations (Born and Jordan 1925; Born, Heisenberg, and Jordan 1926).[35]

Jordan, and almost simultaneously and independently Schrödinger, observed that there was an exact equivalence and a one-to-one correspondence between the two methods of introducing statistics, called above the "statistics of quanta" and the "statistics of oscillators," or the statistics of indistinguishable particles and (distinguishable) quantized oscillators (Born, Heisenberg, and Jordan 1926, p. 609; Schrödinger 1926).[36] Calculations with Bose particles and with quantized waves produced equivalent results, which, according to Jordan, constituted the mathematical formulation of the wave-particle dualism. Only in the case recently treated by Ehrenfest there did seem to be a remaining discrepancy between the two methods. Jordan hoped that quantum mechanics would prove more successful in dealing with the problem than the old quantum theory and took the method of quantized waves over into the new theory. Jordan considered energy fluctuations in a simple one-dimensional case of an oscillating string and, following closely Ehrenfest's procedures, calculated fluctuations in a small segment a of the whole string of length L. In the classical case, he obtained, like Lorentz and Ehrenfest before (Lorentz 1916, Ehrenfest 1925a), the result corresponding to the second term in (9)

$$\overline{\varepsilon^2} = \frac{e^2}{2a}. \tag{17}$$

Quantization of an oscillator according to matrix mechanics added an extra zero-point energy $h\nu/2$ to all its energy values. Jordan thus substituted total energy e in (17) with the sum $e_t + h\nu/2$ of the "real thermal energy" e_t and zero-point energy for each available mode of oscillation. Only the thermal energy was supposed to figure in Einstein's formulas (1), (3), and (9). For its fluctuations, Jordan's simple substitution delivered the required result with both expected terms. The origin of the quantum term in fluctuations thus seemed to be connected with the zero-point energy of quantized waves.

At that early stage, the new quantum theory could still claim only very few successes. Enthusiastic about the apparent advantage of his calculation over the methods of the old quantum theory, Jordan wrote a postcard to Einstein, but Einstein was not very impressed:

> I have been occupied myself much with Heisenberg–Born. I tend more and more to consider it as inappropriate despite all the admiration for the idea. The zero-point energy of the black body radiation cannot exist. The corresponding argument by Heisenberg, Born and Jordan (fluctuations) I consider to be untenable, already because the probability of large fluctuation (for instance, for the total energy in the part v of volume V) surely cannot be derived in this way.[37]

Einstein made the same two objections in his reply to Jordan: the sum of zero-point energies $h\nu/2$ for all oscillating modes would give an infinitely large contribution to the total energy, and, besides, the new procedure did not offer a way of deriving the other fluctuation formula (7). Einstein's skepticism did not discourage Jordan, who was hoping to develop "a systematic matrix theory of electromagnetic field."[38] He considered the infinitely large energy of zero-point modes as being of no real physical importance. The methods of matrix mechanics, indeed, did not allow one to handle formula (7), but Jordan returned to the problem two years later and solved it with the help of the method of second quantization and the probability interpretation of the wave function (Jordan 1927b).

The quantization of waves eventually became the standard method in quantum field theory. In the view of at least some historians, Jordan's 1925 quantization of electromagnetic waves marks the beginning of quantum electrodynamics, its first successful application being the solution of the problem of energy fluctuations (Pais 1986). Yet it remains somewhat uncertain to what extent that success was unambiguous. Initially, not only Einstein, but also Born, Heisenberg, and Pauli, Jordan's collaborators on matrix mechanics, were not entirely confident in the result and in the usefulness of the entire procedure of wave quantization. The quantization of electromagnetic waves was generally accepted only after Dirac's impressive success with it in calculating Einstein's radiation transition coefficients *A* and *B* (Dirac 1927). As for the calculation of fluctuations, doubts still remained and were raised on various occasions.

Unlike triumphant quantum mechanics, quantum electrodynamics had a rather bumpy start. Its obvious successes mixed with equally obvious failures, old and newly found problems, most of them connected with divergences. Around 1930, the theory passed through a major crisis when its entire foundation was called in question and criticized once again. In this situation, Heisenberg reconsidered Jordan's calculation and found divergent integrals in it, which made fluctuations infinite and which were previously overlooked. The divergences appeared in the ultraviolet region, were connected with the first term of fluctuations and did not depend on the existence of the zero-point energy, which was also infinite. Heisenberg realized that one could get rid of them mathematically by considering the volume with "smeared-out" rather than precise boundaries and interpreted this result along the lines of the uncertainty principle: an experimental attempt to precisely determine the volume in space leads to great uncontrolled disturbances in the physical system (Heisenberg 1931).

Several years later, both Jordan's and Heisenberg's treatment came under a strong critique by Born and Klaus Fuchs, who claimed to have shown that all divergences were caused by zero-point oscillations, yet were disastrous, leading to observable deviations from Einstein's formula (Born and Fuchs 1939a). Both these conclusions rested upon a mistake in calculation, which was found by Markus Fierz (Born and Fuchs 1939b). In 1933 Niels Bohr and Leon Rosenfeld proposed a way of ignoring the problem by arguing that the energy of the electromagnetic field within a particular volume cannot be measured. What could be measured was the intensity of the field whose fluctuations are relatively small in the physically interesting domain (Bohr and Rosenfeld 1933). The latter claim, however, was refuted by later calculations (Corinaldesi 1953).

The present status of the problem of energy fluctuations in quantum electrodynamics does not seem to have improved much since Heisenberg. The divergences which he had found remained in renormalized quantum electrodynamics (Dyson 1951). Some authors could ignore this by insisting that volume boundaries have to be "smooth," while others could use this as a way of challenging conventional quantum field theory. At least one later calculation dealt with the question directly and led to a confusing result that "the fluctuation of the spectral density is always finite, but it is not in general given by Einstein's formula" (González and Wergeland 1973, p. 1). Overall, it seems that the problem of fluctuations of electromagnetic energy, once very acute and most actively debated, has not been resolved in modern quantum electrodynamics, after all, but rather has been marginalized and has disappeared from the center of attention of field theorists.

4.2 FLUCTUATIONS IN FERMI GAS AND SECOND QUANTIZATION

In early 1926, Enrico Fermi understood that electrons obeyed a different kind of statistics from light quanta. Later that year Dirac rediscovered this feature and included it into the general formalism of emerging quantum mechanics (Dirac 1926). Particles obeying Fermi statistics cannot occupy the same cell in phase space, each cell therefore contains either 0 or 1 particle. Once the difference between Bose and Fermi statistics was understood, it was not so difficult to derive the formula for fluctuations in the number of electrons within a particular volume. It differed from the analogous formula for Bose particles (16) only in the sign (minus instead of plus):

$$\frac{\overline{(\Delta N)^2}}{N^2} = \frac{1}{N} - \frac{1}{Z}\,. \tag{18}$$

Formula (18) was first published in (Pauli 1927) among other important results in the theory of the electron gas. As in the Bose case, one could use the statistics of either quanta or oscillators. Pauli chose the latter and distributed oscillators over two possible energy states (0 or 1 particle). Following Einstein's example, he interpreted the second term as the interference of de Broglie waves.

The model of quantized waves did not seem to offer any straightforward way of deriving the formula (18), but in the fall of 1927 Jordan realized how the method should be altered in order to account for Fermi statistics. This constituted his other great contribution to quantum electrodynamics, the method of second quantization for fermions. Instead of the standard quantization with the help of the commutation relations

$$a_r a_r^* - a_r^* a_r = 1, \tag{19}$$

one had to use a modified anticommutator set

$$b_r b_r^* + b_r^* b_r = 1. \tag{20}$$

The difference again could be reduced to one sign, but Jordan's path towards this discovery was a very difficult one, since it required a change in the standard form of canonical commutator, which was regarded as the very essence of quantum mechanics. Jordan arrived at the anticommutator via a rather complicated route, working at first with quaternions, finding in (Jordan 1927a) a concrete matrix representation of what is currently known as the operators of creation and annihilation

$$b_r = \begin{pmatrix} 0 & 1 \\ 0 & 0 \end{pmatrix}, \quad b_r^* = \begin{pmatrix} 0 & 0 \\ 1 & 0 \end{pmatrix} \tag{21}$$

and only later, in (Jordan and Wigner 1928), realizing that they correspond to the commutation relations (20).[39]

In Jordan's understanding, he was quantizing the electron's de Broglie wave (or the Schrödinger function in its initial wave-mechanical rather than probabilistic interpretation). He was guided in this search by his own version of the "dualism between corpuscles and quantized waves in quantum mechanics" ["quantenmechanishe Dualismus von Korpuskeln und gequantelten Wellen"] (Jordan 1927b, p. 766). As he had already demonstrated in late 1925, quantized electromagnetic oscillations delivered results equivalent to those obtained with Bose quanta. Once Schrödinger's wave mechanics of the electron appeared in early 1926, Jordan developed hopes to prove that the same equivalence can be demonstrated for particles of matter and quantized matter waves, though he did not publish the idea, partly because of Heisenberg's and Pauli's criticism and partly because he did not see the mathematical solution then.[40] He hoped that the quantization of wave would provide a mathematical method of describing an ensemble of many particles, as it did in the case of radiation, but it took him almost two years to find out how to make this ensemble obey Fermi statistics instead of Bose statistics.

In the meantime, the many-body problem in quantum mechanics was developing along a different route, by considering symmetrical and antisymmetrical ψ-functions in $3N$-dimensional space (N is the number of particles) (Dirac 1926). This trend undermined Schrödinger's initial *anschaulich* interpretation of the ψ-function as a matter wave, since it was no longer possible to visualize the multi-dimensional mathematical function in the ordinary three-dimensional space. The probabilistic interpretation of the ψ completed the demise of realistically interpreted waves. Although Jordan was among the first proponents of the probabilistic interpretation and readily sacrificed the multidimensional ψ-function for this purpose, he simultaneously maintained a hope of restoring the initial visualization of the three-dimensional wave. He achieved this, at least partly, in his method of second quantization for fermions, since the description of many-body ensemble of electrons was achieved there by the quantization of a three-dimensional wave for one electron.

As he did two years earlier, Jordan tested his new method on the problem of energy fluctuations. Repeating his previous calculations from (Born, Heisenberg, and Jordan 1926) but with both kinds of commutators, he derived expressions for the fluctuations in the number of particles within a given volume,

$$a_r^* a_r^2 a_r^* = N_r (1 + N_r) \tag{22}$$

in the Bose case, and

$$b_r^* b_r^2 b_r^* = N_r (1 - N_r) \tag{23}$$

in the case of Fermi statistics, which are equivalent to Equations (16) and (18) (Jordan 1927a). Like his earlier calculation of fluctuations, the result did not look very impressive to others, but Jordan felt encouraged by it enough to formulate, in the last section of his paper, a bold vision of the future relativistic quantum theory based on the fundamental concept of the quantized wave (electromagnetic waves for radiation and matter waves for electrons). Although expressed a little too prematurely and publicly rebuffed by Pauli, Jordan's proposal eventually became realized in modern theory.

Although fluctuations once again played an important role as the pretext for the very first programmatic proposal of quantum field theory, Jordan's calculation later encountered the same kind of difficulties as his derivation of the Einstein formula. Heisenberg, again, pointed out that the actual integrals diverge (in the case of electrons' fluctuations, this is related to the processes of pair creation in the vacuum state) and suggested the same mathematical trick of making results finite by "smearing out" the boundaries of the volume (Heisenberg 1934). Bohr and Rosenfeld could not declare the number of electrons (or electric charge) unmeasurable in their discussion of the possibilities of measurement in quantum electrodynamics (Bohr and Rosenfeld 1950). With regards to the difficulty with infinite fluctuations, they simply repeated Heisenberg's argument about volume boundaries. Their paper reminded the post-war generation of physicists of the existence of the problem. Some discussion arose, which led to more precise calculations of the divergences, but not to their elimination.[41]

5. Conclusions

Einstein initially used fluctuations as an argument for an atomistic theory of light that would complete Lorentz's atomistic reform of classical electrodynamics of the late nineteenth century. The closest analog of Einstein's light quantum was the non-mechanical electron of contemporary electrodynamics. In exactly what way field and quanta combined was not known, but combinations actually tried by Einstein in his multiple attempts to design a model for the light quantum—although someone might want to call them dualistic—were actually rooted in models of the electron discussed in classical electrodynamics around 1900.

Responding to criticism, first of all by Lorentz, Einstein changed his views towards a gradual recognition of the grave contradiction between the wave and quantum aspects of radiation. Many of his critics came to a similar conclusion from the opposite direction, after many unsuccessful attempts to defend the wave theory of light. A compromise achieved at the first Solvay meeting in 1911 saw the origin of quantum effects in discontinuities in the interaction between light and matter rather than in the structure of radiation itself. Lorentz developed a corresponding explanation for the formula of energy fluctuations, which influenced Einstein's 1916 theory of spontaneous and stimulated radiation transitions.

Despite the enormous rise in popularity of corpuscular light quanta after the end of World War I, due to the generational change and the change of prevailing mood within the physics community, Einstein remained acutely aware of their insufficiency and of the necessity to resolve the contradiction with the wave theory. His 1924 reference to de Broglie's dualistic hypothesis reflected both the inability to find a solution that would satisfy him and his reluctance to accept either of the two available alternatives: the fundamentality of Bose statistics that could explain both terms in fluctuations without any reference to waves and Ehrenfest's model of quantized waves. Einstein continued to hope for a solution in some kind of an *anschaulich* combination of waves and quanta, seeing in de Broglie's work a promising step in that direction. It can be said that Einstein accepted the wave-particle duality only in a negative sense, as a fundamental difficulty of quantum theory that had to be resolved rather than turned into a postulate. By his strict criteria, neither matrix nor wave mechanics succeeded in this crucial task, which was an important source of his critical attitude towards the emerging quantum mechanics.

Many of the authors of quantum mechanics, however, became aware of the idea of the wave-particle duality via the discussion of fluctuations in (Einstein 1925) and accepted it in a positive sense, as one of the most basic principles of the new theory. This historical contingency established a permanent link in popular perception between fluctuations and duality, which was later transformed into textbook statements about the necessary logical relationship between them. The difficulties with the interpretation of the fluctuation formula, however, did not come to an end with the establishment of quantum mechanics. Fluctuations played an important role in Jordan's early proposals of relativistic quantum theory, including such basic methods as field quantization and second quantization, yet even in renormalized quantum electrodynamics, the problem of energy fluctuations does not seem to have found an entirely satisfactory solution. The problem that once was crucial and moved the theory ahead became largely forgotten rather than ultimately resolved.

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NOTES

[1] The Kuhnian word "crisis" was also the term widely used by participants in the events themselves, and the entire situation, indeed, is reminiscent of a crisis in science as described by Kuhn. For some time, I thought that the history of quantum theory of radiation and of quantum electrodynamics offers one of the very few—if not the only—example in the history of physics, when historical actors, in their explicit words and deeds, behaved as if they were consciously playing according to the Kuhn scenario of crisis. Now [1999] I rather suspect that the very process of the historical development of quantum electrodynamics and the eventual resolution of its crisis, which Kuhn witnessed directly while doing his graduate studies in physics, could have served as an important hidden source for his later philosophical doctrine.

[2] "Mit seiner Abhandlung über die statistischen Schwankungen in der Strahlung hat Einstein den Dualismus Welle-Teilchen beim Licht eingeführt. Die volle

Tragweite dieses umwälzenden Gedankens konne er natürlich noch nicht sehen" (Hund 1967, p. 44).

[3] "Damals schien es mir, als ob die Plancksche Theorie der Strahlung in gewisser Beziehung ein Gegenstück bildete zu meiner Arbeit" (Einstein 1906, p. 199).

[4] Indeed, one year earlier Einstein derived, independently of Gibbs, the general formula (1) and all he needed was to apply it to the Wien law.

[5] "Es ist hervorzuheben, daß die angegebenen Überlegungen im wesentlichen keineswegs ihren Wert verlieren würden, falls die Plancksche Formel noch als ungültig erweisen sollte; gerade der von der Erfahrung genügend bestätigte Teil der Planckschen Formel (das für große ν/T in der Grenze gültige Wiensche Strahlungsgesetz) ist es, welcher zur Lichtquantentheorie führt" (Einstein 1909a, p. 191).

[6] "Das erste Glied, wenn es allein vorhanden wäre, (würde) eine solche Schwankung der Strahlungsenergie liefern, wie wenn die Strahlung aus voneinander unabhängig beweglichen, punktförmigen Quanten von der Energie $h\nu$ bestünde" (Einstein 1909a, p. 189).

[7] "Deze bezwaren jammer want theorie lichtquanta wel mooi" (Lorentz to Einstein, 6 May 1909, draft in Dutch, EA 16-417). For the actual letter, in German, and its English translation, see Einstein 1993b, p. 176, and the accompanying volume of translations. "Das Gesagte dürfte genügen, um zu zeigen, daß von Lichtquanten, die bei der Fortbewegung in kleinen Räumen konzentriert und stets ungeteilt bleiben, keine Rede sein kann" (Lorentz 1910, p. 354).

[8] "Deshalb ist es meine Meinung, daß die nächste Phase der Entwickelung der theoretischen Physik uns eine Theorie des Lichtes bringen wird, welche sich als eine Art Verschmelzung von Undulations- und Emissionstheorie des Lichtes auffassen läßt"; and "Ich wollte durch dasselbe nur kurz veranschaulichen, daß die beiden Struktureigenschaften (Undulationsstruktur und Quantenstruktur), welche gemäß der Planckschen Formel beide der Strahlung zukommen sollen, nicht als miteinander unvereibar anzusehen sind" (Einstein 1909b, pp. 482–483, 500).

[9] "Ich glaube vielmehr, daß sich das Licht in ähnlicher Wiesse um singuläre Punkte (es brauchen nicht notwendig mathematisch singuläre Punkte zu sein) herum gruppiert, wie wir das vom elektrostatischen Felde anzunehmen gewohnt sind. Ich denke mir also ein einzelnes Lichtquant als einen Punkt, der von einem sehr ausgedehnten Vektorenfeld umgeben ist, das mit der Entfernung irgendwie abnimmt. Der Punkt ist eine Singularität, ohne welche das Vektorenfeld nicht existieren kann. Ob man sich beim Vorhandensein vieler Lichtquanten mit einander überdeckenden Feldern eine einfache Superposition der Vektorenfelder vorzustellen hat, das kann ich nicht sagen. Jedenfalls müsste man zur Bestimmung der Vorgänge ausser den Differentialgleichungen für das Vektorfeld auch noch Bewegungsgleichungen für die singulären Punkte haben. Die Energie des elektromagnetischen Feldes müsste—wenigstens bei genügend verdünnter Strahlung—mit der Anzahl dieser singulären Punkte in gewisser Weise zusammenhängen. Absorption fände nur statt beim Verschwinden eines derartigen singulären Punktes bezw. bei Degenerieren des zu diesem Punkt gehörigen Strahlungsfeldes (besser "von auf

diesen Punkt stützender Strahlung"). Durch die Angabe der Bewegungen aller Singularitäten wäre das Vektorenfeld vollkommen bestimmt, sodass die Anzahl der zur Charakterisierung einer Strahlung nötigen Variabeln eine *endliche* wäre. . . . Das Wesentliche scheint mir übrigens gar nicht in der Annahme singulärer Punkte zu liegen, sondern in der Annahme solcher Feldgleichungen, welche Lösungen zulassen, bei welchen sich endliche Engergiemengen ohne Zerstreuung in einer bestimmten Richtung mit der Geschwindigkeit *c* fortpflanzen. Man sollte meinen, dass das Ziel mit einer geringen Modifikation der Maxwell'schen Theorie zu erreichen sei" (Einstein to Lorentz, 23 May 1909, Einstein 1993b, Doc. 163, pp. 193–194).

[10] See Einstein to Arnold Sommerfeld, 14 January 1908, Einstein 1993b, Doc. 73, pp. 86–89. The same year, Einstein also turned to a spectroscopist August Hagenbach asking whether experiment could hint towards any possible decrease in the ability of two rays to interfere with the decrease in their intensity. Despite Hagenbach's disappointing reply, Einstein did not become convinced and retained a hope that interference would come out as a result of the interaction of many quanta, while vanishing with the decrease in their number. (See Einstein to and from Hagenbach, 6, 9, and 14 July 1908, Einstein 1993b, Docs. 109–111, pp. 128–130.)

[11] For a further discussion of this point, see Kojevnikov 1994.

[12] "ungelösten Rätsel" (Einstein 1911, p. 347) and "Ein provisorischer Versuch, . . . Hilfsvorstellung, die sich mit den experimentell gesicherten Folgerungen der Undulationstheorie nicht vereinigen zu lassen scheint" (Discussion 1911, p. 359).

[13] Einstein to Lorentz, 17 January 1916, Einstein 1998, Doc. 184, p. 247; cf. also Lorentz to Ehrenfest, 18 January 1916 (Archive for the History of Quantum Physics).

[14] This is a reference to the theory of emission of gravitational waves from a massive body in general relativity.

[15] "Mit der Theorie der Ausstrahlung materialler Systeme bin ich noch nicht ganz fertig. Aber soviel ist mir klar, dass die Quanten—Schwierigkeiten auch die neue Gravitationstheorie treffen, ebenso gut wie die Maxwell'sche Theorie. Es hat mich sehr gefreut, dass Sie in Ihren Pariser Vorträgen die Schwankungs—Eigenschaften der Strahlung einer eingehenden Behandlung gewürdigt haben; hier treten die Unrichtigkeiten der Theorieen am reinsten zutage" (Einstein to Lorentz, 17 June 1916, Einstein 1998, Doc. 226, p. 300). Pais has already suggested that the study of the emission of gravitational waves could have spurred Einstein's return to quantum electrodynamics in 1916 (Pais 1982, p. 280), but he did not pay attention to the reference to Lorentz's book in the same paragraph of Einstein's letter.

[16] For a discussion of Planck's combinatorics, see Bergia 1987 and Darrigol 1991.

[17] Early quantum theorists would say that quanta in the oscillator lose their individuality or that they are not independent from one another. I am using all three

words, "individuality," "independency," and "distinguishability," as synonyms, as they often were in those days.

[18] In Darrigol 1991, they are referred to by symbols W_2 and W_4 correspondingly.

[19] In 1923 Planck developed this method a bit further: Laue had given the expression for the probability $w(N)$ of one oscillator to have the energy value $Nh\nu$, Planck then calculated the probability $W_m(N)$ for a system of m oscillators to have the total energy $Nh\nu$ (Planck 1923b).

[20] For a detailed discussion of these papers and the history of the concept of indistinguishability, see Darrigol 1991.

[21] Ehrenfest lived in St. Petersburg in 1907–1912 and initiated the tradition of theoretical physics in Russia.

[22] See the citation analysis in Small 1986. On the history of experimental aspects of the wave-particle dilemma, see Wheaton 1983.

[23] On the role of thermodynamics in Einstein's thought, see Klein 1967.

[24] Bergia (1987) seems to have overlooked this difference.

[25] An undocumented later interview, reported by J. Mehra. Quoted from Bergia 1987, p. 226.

[26] Bose to Einstein, 4 June 1924, EA 6-127. As Mehra and Rechenberg (1982, vol. 1, p. 565) have already noted, de Broglie had derived this coefficient earlier in (de Broglie 1922a) also from the entirely corpuscular perspective. De Broglie also divided phase space into quantum cells, but he used this to derive the Wien law only, proceeding then to the hypothesis of light molecules in order to obtain the Planck law. Another part of Bose's calculations, the expression for the number of cells with r particles (Equation (13)) is exactly the same as the expression for the number of oscillators with r quanta in Schrödinger 1924. Schrödinger was aware that such quanta were indistinguishable.

[27] "Bohr, Kramers und noch einer haben die "losen" Quanten abgeschaft. Werden sich aber nicht entbehren lassen. Der Inder Bose hat eine schöne Ableitung des Planckschen Gesetzes samt Konstante auf Grund der losen Lichtquanten gegeben. Ableitung elegant, aber Wesen bleibt dunkel. Ich habe auf Grund seine Theorie auf ideales Gas angewendet. Strenge Theorie der "Entartung." Keine Nullpunktsenergie und oben kein Energiedefekt. Gott weiss ob es so ist" (Einstein to Ehrenfest, 12 July 1924, EA 10-090).

[28] Bose attempted to calculate these fluctuations in his manuscript "Fluctuations in density," undated, but evidently written in 1924–1925 (EA 6-133).

[29] On de Broglie's work leading to his dualistic model and the introduction of matter waves, see Kubli 1971 and Darrigol 1993.

[30] Einstein to Jakob Laub, 4 November 1910, Einstein 1993b, Doc. 231, pp. 260–262. Einstein once mentioned the idea of energy non-conservation in print (1911, p. 348).

[31] "lassen die Aufstellung einer eigentlich quantenhaften Theorie der Strahlung fast unvermeidlich erscheinen. . . . Die Schwache der Theorie liegt einerseits darin,

daβ sie uns dem Anschluβ an die Undulationstheorie nicht näher bringt" (Einstein 1917, pp. 127–128).

[32] He also rejected the proposal that energy is not conserved, when it appeared in print in the 1924 theory of Bohr, Kramers, and Slater: "This idea is an old acquaintance of mine, whom I don't consider as a real guy." ["Diese Idee ist ein alter Bekannter von mir, den ich aber fur keinen reelen Kerl halte."] (Einstein to Ehrenfest, 31 May 1924, EA 10-088)

[33] "die Fragestellung prinzipieller Natur, die wir nun auf dem Gebiete der Lichterscheinungen haben, gipfelt darin, entweder, zu zeigen, daß die Korpuskulartheorie das wahre Wesen des Lichtes erfaßt, oder, daß die Undulationstheorie richtig ist und das Quantenhafte nur scheinbar ist, oder endlich, daß beide Auffassungen dem wahren Wesen des Lichts entsprechen und das Licht sowohl Quanteneigenschaften als undulatorische Eigenschaften hat. Man suchte nun eine Synthese dieser beiden Eigenschaften zu finden, was bisher mathematisch noch nicht gelungen ist. Der letzte große Fortschritt, der in der Physik des Lichts gemacht wurde, ist dadurch erreicht worden, daß man sich wieder von der Korpuskularauffassung entfernt hat und wieder einen Schritt gemacht hat, der umgekehrt ist demjenigen, der von der Undulationstheorie zur Korpuskulartheorie geführt hat. Einstein verweist hier auf die Arbeiten von De Broglie und Schrödinger. . . . die Natur fordert von uns eine Synthese beider Auffassungen, die bis jetzt allerdings noch über die Denkkräfte der Physiker hinausgegangen ist" (Einstein 1927, p. 546). On Einstein's reluctance to accept duality in the sense of quantum mechanics, see also Bach 1989, p. 174.

[34] "Dein Gehirn sieht, weiß der Himmel, reinlicher aus. Seine Produkte sind klar, einfach und treffen die Sache. Wir kapieren es dann zur Not ein paar Jahre später. So ist es uns auch mit Deiner Gasentartung und der Boseschen Statistik gegangen. Glücklicherweise erschien Ehrenfest hier und hat uns ein Licht aufgesteckt. Darauf habe ich die Arbeit von Louis de Broglie gelesen und bin allmählich auch hinter Deine Schliche gekommen. Jetzt glaube ich, daß die 'Wellentheorie der Materie' eine sehr gewichtige Sache werden kann" (Born and Einstein 1969, p. 120). [English translation from Born and Einstein 1971, p. 83**]**

[35] The following discussion is restricted to the problem of fluctuations only. For a detailed analysis of these papers in the context of matrix mechanics, see Mehra and Rechenberg 1982, vol. 3, and van der Waerden 1967, and in the context of the emerging quantum electrodynamics, Darrigol 1986 and Pais 1986.

[36] Jordan also criticized in passing the idea of light molecules, calling it "Debye statistics." The mistaken attribution of the idea to Debye is due to a wrongly interpreted obscure footnote in Schrödinger 1924.

[37] "Mit Heisenberg–Born habe ich mich noch viel beschäftigt. Ich neige mehr und mehr dazu, bei aller Bewunderung des Gedankens diesen für unzutreffend zu halten. Nullpunkts-Energie der Hohlraumstrahlung kann es nich geben. Das diesbezügliche Argument von Heisenberg, Born und Jordan (Schwankungen) halte ich für hinfällig, schon deshalb weil die Wahrscheinlichkeit für grosse Schwankungen (zum Beispiel Antreffen der *ganzen* Energie in Teilvolumen *U* von *V*) so

gewiss nicht richtig herauskommt." (Einstein to Ehrenfest, 12 February 1926, EA 10-131)

[38] "Ich will, sobald ich Zeit habe, eine systematische Matrizentheorie des elektromagnetischen Feldes zu überlegen versuchen" (Jordan to Einstein, 15 December 1925, EA 13-474).

[39] He also made a mathematical mistake on the way, multiplying the operators by $-i$ and i correspondingly, but this did not affect the final result.

[40] ". . . zumal Pauli und Heisenberg nicht davon wissen wollten, während Born zwar anfanglich sehr zustimmte, aber später auch nichts mehr davon hielt" (Jordan to Schrödinger, undated, probably autumn 1927, Archive for History of Quantum Physics).

[41] See Corinaldesi 1953 for further references. There was also an attempt to substitute the dualistic interpretation of the fluctuation formula with another one based on classical statistics in Bach 1989 and literature cited there.

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