The First Phase of the Bohr-Einstein Dialogue
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Einstein and Bohr

The First Phase of the Bohr-Einstein Dialogue

BY MARTIN J. KLEIN*

1. Niels Bohr and Albert Einstein discussed and disagreed about the paradoxes of the quantum theory for a third of a century. The extraordinary personal and intellectual qualities of the two men and the unprecedented difficulty and depth of the issues they debated make these discussions unique in the history of physics. In this paper I want to analyze the first occasion on which Bohr and Einstein differed over a question of fundamental principle. This occurred during the years 1923 to 1925, just before the new quantum mechanics began to appear. The principles at stake were nothing less than the validity of the laws of conservation of energy and momentum and the existence of the wave-particle duality for radiation.

Bohr wrote an account of his long dialogue with Einstein on the occasion of Einstein's seventieth birthday.[1] This is a precious document for anyone interested in the history of twentieth-century physics, but in reading it we must keep in mind that it appeared in 1949 and that it was written by one of the participants in the dialogue. Bohr naturally told the story as he saw it at the time of writing, and his account shows the insight gained by his decades of rich experience with quantum physics. The principal theme of Bohr's essay was the series of critical attacks that Einstein directed over the years against quantum mechanics, and the successive defeat of each of these at-

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1. N. Bohr, "Discussion with Einstein on Epistemological Problems in Atomic Physics," in *Albert Einstein: Philosopher Scientist*, ed. P. A. Schilpp (Evanston, Illinois, 1949), p. 199. Reprinted in N. Bohr, *Atomic Physics and Human Knowledge* (New York, 1958), p. 32.

tacks by Bohr and his collaborators, with each exchange leading to a new and deepened understanding of the fundamentals of the new physics. Einstein was never reconciled to the severe restriction of physical theory to a probabilistic goal which seemed to be an essential feature of quantum mechanics. It is hardly surprising then that he appears in Bohr's essay as the more conservative of the two, concerned about "the lack of firmly laid down principles for the explanation of nature, in which all could agree," while Bohr was the one who thought that "we could hardly trust in any accustomed principles, however broad, apart from the demand of avoiding logical inconsistencies."[2]

Perhaps it had also seemed that way in 1924. For one of the most startling suggestions made during the twenties, that decade of startling suggestions about how the laws of physics should be altered, was that "we abandon any attempt at a causal connection between the transitions in different atoms, and especially a direct application of the principles of conservation of energy and momentum, so characteristic for the classical theories." This extreme measure, abandoning causality and the conservation laws in atomic physics, was proposed by Niels Bohr in a paper written early in 1924 in collaboration with H. A. Kramers and J. C. Slater.[3] It is this paper that will be the focus for what I have to say.

In Bohr's description of his dialogue with Einstein, the Bohr, Kramers, Slater paper is mentioned only very briefly. Bohr and Einstein never met to discuss this work, nor did they correspond about it, so far as I know. Nevertheless, the Bohr, Kramers, Slater proposal is an essential part of the story of the Bohr-Einstein relationship, and this is perhaps its greatest historical interest. Bohr and his collaborators were struggling with the paradox that radiation behaves under some conditions like an electromagnetic wave and under others like a particle of energy. It was, of course, Einstein who had proposed the idea of energy particles or light quanta in 1905, and this idea had just acquired a new respectability almost twenty years later as a result of Arthur Compton's work on the modified wavelength of the X rays scattered by free electrons. If we

2. *Ibid.*, p. 228.
3. N. Bohr, H. A. Kramers, and J. C. Slater, "The Quantum Theory of Radiation," *Phil. Mag.*, *47* (1924), 785. Passage quoted is from p. 791. (A German version of the same paper appeared in *Z. Phys.*, *24* [1924], 69.)

ask *why* Bohr and his co-workers were willing to give up the validity of the conservation laws, except as statistical averages, I think the answer is clear: it was to save physics from an alternative they considered even less acceptable—the admission of light quanta. The Bohr, Kramers, Slater work was an attempt to preserve certain essential features of the wave theory of radiation in the face of an apparent need for Einstein's light quanta, and it was for this conservative reason that they were willing to take the radical step of abandoning the laws of energy and momentum. Their attempt failed, but an analysis of their proposal in the context of its time may add to our understanding of the ideas of both Bohr and Einstein and the interaction between them.[4]

Both men were fully aware of the complexity of the problems they were struggling with, and neither was ever satisfied with easy answers. This account of the first phase of their long dialogue may suggest something of "the years of anxious searching in the dark, with their intense longing, their alternations of confidence and exhaustion" that must precede "the final emergence into the light," when "the happy achievement seems almost a matter of course and any intelligent student can grasp it without too much trouble."[5]

2. In 1905 Albert Einstein suggested, in all seriousness, that light be considered as composed of a collection of independent particles of energy.[6] He made this suggestion, despite all the successes of the wave theory of light throughout the nineteenth century, because he was convinced that this new hypothesis of light quanta offered a more fruitful approach to the understanding of processes involving the emission and absorption of light. The classical theory had failed to account for the existence of an equilibrium distribution of

4. The Bohr, Kramers, Slater paper has been discussed in some detail by Jammer, Meyer-Abich, and van der Waerden. (a) M. Jammer, *The Conceptual Development of Quantum Mechanics* (New York, 1966), pp. 181–188, 345–350. (b) K. M. Meyer-Abich, *Korrespondenz, Individualität und Komplementarität* (Wiesbaden, 1965), pp. 102–133. (c) B. L. van der Waerden, ed., *Sources of Quantum Mechanics* (Amsterdam, 1967), pp. 11–15. The paper itself is also reprinted by van der Waerden, p. 159.

5. A. Einstein, *The World as I See It* (New York, 1934), p. 108.

6. A. Einstein, "Uber einen die Erzeugung und Verwandlung des Lichtes betreffenden heuristischen Gesichtspunkt," *Ann. Phys., 17* (1905), 132. English translation by A. B. Arons and M. P. Peppard in *Amer. Jour. Phys., 33* (1965), 367.

energy in blackbody radiation, and Einstein immediately recognized this as a difficulty in the very foundations of physics.[7] He was led to the hypothesis of light quanta by his study of the entropy of radiation in the high-frequency range, where its spectrum was adequately described by Wien's law. When Einstein analyzed the form of this entropy with the help of Boltzmann's relationship between entropy and probability, he concluded that, when Wien's distribution holds, radiation acts as though it consists of quanta of energy, with the energy quantum proportional to the frequency of the radiation.

This new "heuristic viewpoint" on the nature of light justified itself at once; Einstein used it to account for Stokes's law of fluorescence and the known qualitative, and very puzzling, properties of the photoelectric effect. He also proposed the exact relationship between the frequency of the incident light in the photoelectric effect and the voltage that would stop all photoelectrons produced, a linear equation with a universally constant slope.

One indication of the immediate reaction to Einstein's suggestion can be found by a careful reading of Philipp Lenard's Nobel Lecture, delivered a year later, in May 1906.[8] Lenard received the prize for his work on electrons, including his important experiments on the photoelectric effect. Einstein and his work are simply not mentioned by Lenard, although he asserted that he had "tried hard to put into their historical perspective all the publications which in my opinion have made basic contributions to knowledge," and he actually cited two other papers from the same volume of the *Annalen der Physik* that contained Einstein's work.

By 1909 Einstein's "ceaseless" preoccupation with the "incredibly important and difficult"[9] question of the constitution of radiation had led him to a deeper insight into the theoretical situation. He was now convinced that "the next phase of the development of theoretical physics will bring us a theory of light that can be interpreted as a

7. For further discussion see M. J. Klein, (a) "Einstein's First Paper on Quanta," *The Natural Philosopher*, ed. D. Gershenson and D. Greenberg (New York, 1963), *2*, 57; (b) "Thermodynamics in Einstein's Thought," *Science, 157* (1967), 509.
8. P. E. A. von Lenard, "On Cathode Rays," *Nobel Lectures. Physics. 1901–1921* (Amsterdam, 1967), p. 105.
9. A. Einstein to J. J. Laub, 1909. Quoted in C. Seelig, *Albert Einstein, A Documentary Biography*, trans. M. Savill (London, 1956), p. 87.

kind of fusion of the wave and emission [particle] theories."[10] Einstein had reasons for holding this opinion, and he explained them at a meeting in Salzburg in September of that year. He had analyzed the implications of Planck's law for the blackbody radiation spectrum, using an approach that was peculiarly his own: the study of fluctuations.[11]

The fluctuations ΔE about the average energy E of the blackbody radiation having frequencies between ν and $\nu + d\nu$, and contained in the subvolume V of the enclosure, could be calculated from a basic result of statistical mechanics:

$$\overline{(\Delta E)^2} = kT^2 \left(\frac{\partial E}{\partial T}\right)_V , \tag{1}$$

where k is Boltzmann's constant, and T is the temperature of the walls of the enclosure. Einstein derived this result independently in 1904 and had already used it to great advantage in exploring the significance of fluctuation phenomena. Since the energy E could be expressed in the form

$$E = \rho(\nu, T) V d\nu , \tag{2}$$

where $\rho(\nu, T)$ is the spectral density of the radiation, the fluctuations were determined by the form of the spectral distribution. When Planck's distribution law,

$$\rho(\nu, T) = \frac{8\pi\nu^2}{c^3} \frac{h\nu}{\exp(h\nu/kT) - 1} , \tag{3}$$

was substituted, Einstein obtained the result,

$$\overline{(\Delta E)^2} = (V d\nu)\{h\nu\rho + (c^3/8\pi\nu^2)\rho^2\} . \tag{4}$$

In these equations h is Planck's constant and c is the velocity of light.

While the existence of these energy fluctuations was to be expected, regardless of the wave or particle nature of the radiation, their particular form led Einstein to his new conclusions. The wave theory should lead only to the second term, as one could easily check

10. A. Einstein, "Über die Entwicklung unserer Anschauungen über das Wesen und die Konstitution der Strahlung," *Phys. Z., 10* (1909), 817. See also his earlier paper, "Zum gegenwärtigen Stand des Strahlungsproblems," *Phys. Z., 10* (1909), 185.

11. For further discussion see M. J. Klein, "Einstein and the Wave-Particle Duality," *The Natural Philosopher, 3* (1964), 1.

by using the wave limit of the spectral distribution, that is, the low-frequency classical form first given by Lord Rayleigh. The first term has a natural interpretation as the fluctuation to be expected if the radiation consisted of a collection of independent particles ($E/h\nu$ in number). This term would dominate in the high-frequency or Wien limit of the distribution law.

Einstein concluded that there were two independent causes producing the fluctuations, and that an adequate theory of radiation would have to provide both wave and particle mechanisms. He confirmed this view by a completely independent argument: he calculated the momentum fluctuations of an object suspended in a cavity at a given temperature, an argument closely following his theory of Brownian motion. These fluctuations also had two terms of the same structure, identifiable as wave and particle contributions, again demonstrating that interfering waves alone could not meet the needs of thermodynamic equilibrium. Einstein was certain that the existence of light quanta was a *necessary* result of the fluctuation properties of blackbody radiation, and not just an assumption *sufficient* for deriving Planck's law. Therefore, he thought, "a profound change in our views of the nature and constitution of light is indispensable." He was also confident that such a new fundamental theory, incorporating quanta and interference phenomena, could be constructed.

Einstein's convictions were not shared. Even those who were generally sympathetic to his other work, such men as Max Planck and H. A. Lorentz, had only sharply critical things to say about his light quanta.[12] It was simply not possible to visualize a quantum theory that could account for interference and diffraction phenomena, and physicists were not prepared to sacrifice the electromagnetic-wave theory, which adequately explained these phenomena, on the basis of something so unsubstantial as fluctuation arguments. In 1916 Robert A. Millikan,[13] in the paper reporting his complete experimental confirmation of Einstein's photoelectric equation, could re-

12. See, for example, Planck's remarks after Einstein's paper at Salzburg, *Phys. Z., 10* (1909), 825. See also M. Planck, "Zur Theorie der Wärmestrahlung," *Ann. Phys., 31* (1910), 758, and H. A. Lorentz, "Die Hypothese der Lichtquanten," *Phys. Z., 11* (1910), 349.

13. R. A. Millikan, "A Direct Photoelectric Determination of Planck's 'h'," *Phys. Rev., 7* (1916), 355.

mark on "the astonishing situation that these facts were correctly and exactly predicted . . . by a form of quantum theory which has now been pretty generally abandoned." Millikan was wrong only in suggesting that the idea of light quanta, which he referred to as Einstein's "bold, not to say reckless, hypothesis," had *ever* been accepted.

During that same year, 1916, Einstein published another paper on radiation which showed that he, at least, had not given up his ideas on the quantum structure of radiation.[14] He offered a new derivation of Planck's distribution law, a derivation he described as being "astonishingly simple and general"; he thought it might even properly be called "*the* derivation" of Planck's law.[15] This "purely quantal" treatment was based on statistical assumptions about the processes of emission and absorption, and the basic quantum hypothesis that atomic systems have a discrete set of possible stationary states. The proof turned on the requirement that absorption and emission of radiation suffice to keep a gas of atoms in thermodynamic equilibrium.

The basic idea can be given in a few lines. Suppose that m and n are two atomic states of energies ε_m and ε_n, where $\varepsilon_m > \varepsilon_n$. Guided by the analogy to the behavior of a classical oscillator in the electromagnetic field, Einstein assumed that the probability dW_a that, during the time dt, an atom in state n absorbs energy $(\varepsilon_m - \varepsilon_n)$ from the field, whose spectral radiation density is ρ, and makes a transition to state m, is given by the equation

$$dW_a = B_{mn}\rho dt\,. \tag{5}$$

Similarly, the probability that, during time dt, an atom in the upper state m emits energy $(\varepsilon_m - \varepsilon_n)$ and drops to state n was assumed to have the form

$$dW_e = (B_{nm}\rho + A_{nm})dt\,, \tag{6}$$

where the two terms refer respectively to processes stimulated by the radiation field and processes occurring spontaneously.

These radiation processes must preserve thermodynamic equilib-

14. (a) A. Einstein, "Zur Quantentheorie der Strahlung," *Phys. Gesellschaft, Zürich, Mitteilungen, 16* (1916), 47. Also in *Phys. Z., 18* (1917), 121. (b) English translation in B. L. van der Waerden, ed., *Sources of Quantum Mechanics,* p. 63.
15. A. Einstein to M. Besso, 11 August 1916.

rium among the atoms, so that we must have equal rates of absorption and emission,

$$B_{mn}\rho g_n \exp(-\epsilon_n/kT) = \{B_{nm}\rho + A_{nm}\} g_m \exp(-\epsilon_m/kT)\,, \qquad (7)$$

where g_m and g_n are the statistical weights of the two states. Solving for ρ, assuming that ρ becomes infinite when T does, and using Wien's displacement law, Einstein obtained both the Planck distribution for ρ and Bohr's relationship

$$\epsilon_m - \epsilon_n = h\nu\,, \qquad (8)$$

where ν is the frequency of the radiation associated with this transition.

Einstein's further analysis brought out a new aspect of the radiation problem. To make his theory fully consistent, he had to make explicit use of the completely directional character of energy quanta. Each emitted quantum, for example, must carry away a momentum $h\nu/c$ in a definite direction. The direction would be that of the external radiation if the emission were stimulated, but it would be a direction determined only by chance if the emission were spontaneous. In either case the emission process would be fully directional, and spherical waves would simply not exist. Einstein considered the directional character of quanta to be the main new result of his work. It strengthened his conviction that "a proper quantum theory of radiation" would have to be constructed, even though he had not yet come "any closer to making the connection with the wave theory." But Einstein's argument for this result, a variation of an old favorite of his, based on the Brownian motion of a molecule in the fluctuating radiation field, did not carry the same weight with other theorists that it did with him.

3. Einstein tried hard to find a crucial experiment that would distinguish sharply and directly between the wave and particle theories of radiation. He thought he had found one when he presented a paper to the Academy at Berlin in December 1921, "On an Experiment Concerning the Elementary Process of Light Emission."[16] The question at issue was whether or not the light emitted in one elementary process by a moving atom is monochromatic. On the wave theory the

16. A. Einstein, "Über ein den Elementarprozess der Lichtemission betreffendes Experiment," *Berliner Berichte* (1921), p. 882.

frequency ν emitted by an atom moving with velocity v would vary with the angle θ between the velocity and the direction of observation,

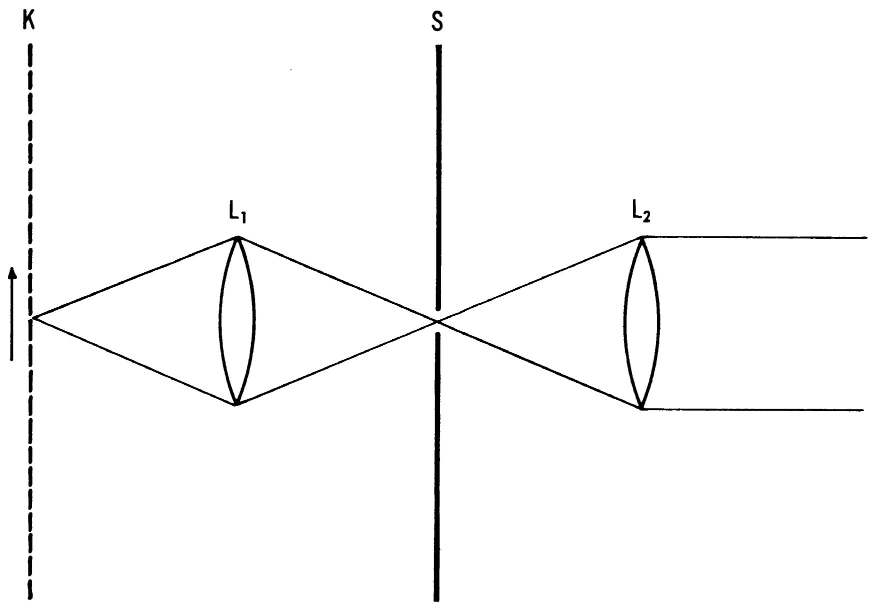
$$\nu = \nu_0\{1 + (v/c) \cos \theta\} , \tag{9}$$

where ν_0 is the frequency emitted by an atom at rest. This is the first-order Doppler effect. On the quantum theory of light emission, however, a consideration of Bohr's fundamental equation, (8), made Einstein "inclined to ascribe one uniform frequency to every elementary act of emission, including emission from a moving atom."[17] Einstein proposed an experiment that should decide between these two alternatives—a frequency varying with direction, as required by the wave theory, and a single fixed frequency, "as suggested though not required by the quantum theory."

The moving light source would consist of the excited atoms in a beam K of canal rays (positive ions). With the help of a lens L_1 and a screen S with a small opening, one could select light coming from a small portion of the beam. A second lens L_2, placed so that the opening in the screen was at its focus, would then make this light into a beam of parallel rays. More precisely, it would make the surfaces of constant phase into planes. These planes would, however, be inclined to one another, in fanlike fashion, if the frequency and wavelength of the emitted light varied with the angle of emission. The light at the top of the beam to the right of L_2 in the figure would be of lower frequency than the light at the bottom. If one were now to allow this beam of light to pass through a dispersive medium, the planes of constant phase would rotate as they traveled, since the lower-frequency light at the top of the beam would travel at a different speed than the higher-frequency light at the bottom of the beam in a dispersive medium. This rotation would manifest itself, according to

17. At the end of his short paper Einstein remarked that although the frequency of the single elementary emission process was independent of direction, this was not inconsistent with the existence of the Doppler effect. I must confess that I simply do not understand this remark. It is true that the Doppler effect had not yet been shown to be valid on the basis of a quantum theory, though Erwin Schrödinger would soon give such a derivation. (E. Schrödinger, "Dopplerprinzip und Bohrsche Frequenzbedingung," *Phys. Z., 23* [1922], 301. See also E. Fermi, "The Quantum Theory of Radiation," *Revs. Modern Phys., 4* [1932], 419.) Since equation (9) would be valid for both wave and quantum theories, the effect Einstein predicted would not have distinguished between them, if it had existed. It seems as though Einstein missed this point, and, so far as I know, no one else ever remarked on it.

Einstein, as a deviation of the light beam from its original direction. He calculated the deviation to be expected and found that it should be possible to obtain an easily measurable deviation of a few degrees under reasonable experimental conditions. No such deviation would be expected if the frequency were independent of the angle of emission, as "suggested" by the quantum theory of emission. If the deviation were not observed, the wave theory would suffer a direct contradiction.



Only a few weeks later Einstein reported to his friend Paul Ehrenfest that the experiment had been tried by Hans Geiger and Walther Bothe in Berlin, and that the outcome was negative: the deviation he had predicted on the basis of the wave theory was not observed.[18] Ehrenfest found this a startling result. "If your light experiment really turns out anticlassically," he commented on a postcard, "—I mean after both theoretical and experimental criticism—then, you know, you will have become really *uncanny* to me. . . . I mean that quite seriously." If the result were genuine, then, Ehrenfest thought,

18. A. Einstein to P. Ehrenfest, 11 January 1922.

Einstein would have discovered "something completely colossal."[19]

Just because this anticlassical result was so disturbing, Ehrenfest could not put it aside. He wrote to Einstein about it again two days later to report a subtlety in the propagation of waves through a dispersive medium which Einstein had missed, and which put his theoretical result in doubt.[20] Einstein had argued as if the wave train were infinitely long, discussing the propagation from the behavior of the phase velocity of the waves. In fact, however, it was a finite wave group or wave packet that was involved in the experimental arrangement, which meant that the group velocity had to be considered. (Ehrenfest was particularly sensitive to the importance of wave packets and the group velocity. Some years earlier he had caught an error in the work of the great authority on wave motion, Lord Rayleigh, an error which arose from overlooking just such a point.[21]) Ehrenfest referred Einstein to an old paper by J. Willard Gibbs[22] in which Gibbs had discussed theoretically the measurement of the velocity of light in a dispersive medium by means of Foucault's rotating mirror experiment. Gibbs had shown that "while the individual wave rotates, the wave-normal of the group remains unchanged, or, in other words, that if we fix our attention on a point moving with the group . . . the successive wave-planes, as they pass through that point, have all the same orientation."

Ehrenfest saw that Gibbs's analysis also applied to Einstein's experiment. The increasing inclination of the planes of constant phase would indeed occur for individual waves as they propagated through the dispersive medium, but these waves would cease to exist physically as soon as they propagated out of the moving group. Inside the moving wave group there would always be just the same range of inclinations of these planes as there was when the group entered the dispersive medium.

Ehrenfest knew his friend well enough to preface his discussion by

19. P. Ehrenfest to A. Einstein, 17 January 1922.
20. P. Ehrenfest to A. Einstein, 19 January 1922.
21. P. Ehrenfest, "Misst der Aberrationswinkel im Fall einer Dispersion des Äthers die Wellengeschwindigkeit?" *Ann. Phys.*, *33* (1910), 1571. See also M. J. Klein, *Paul Ehrenfest. The Making of a Theoretical Physicist* (Amsterdam, 1970), Chapter 7.
22. J. W. Gibbs, "On the Velocity of Light as Determined by Foucault's Revolving Mirror," *Nature*, *33* (1886), 582. Reprinted in *The Scientific Papers of J. Willard Gibbs* (New York, 1906; reprinted 1961), *2*, 253.

saying: "Of course you are such a devil of a fellow that naturally you will finally turn out to be right in the end." He also concluded with the remark: "Don't be annoyed with me if I am wrong; and don't be annoyed with me if I am right."

Einstein answered that everyone was now attacking him for his recently announced result.[23] He and Max von Laue had had "a regular duel" over it at the Berlin colloquium. Nevertheless, Einstein remained convinced that he was right. He had carried out a new and more rigorous calculation based on the wave theory, a calculation too long to include in his letter, and he considered his proof to be "certain," or at least "what a theoretical physicist calls certain." He was repentant over past blunders and curious to see what Ehrenfest would have to say about the new proof. Einstein was particularly fond of this proof over which he had really taken a lot of trouble.

Ehrenfest "wished the new proof well" but refused to believe in the deviation predicted by the wave theory.[24] He, too, was convinced of his position, and saw no way of getting around his wave group argument. Since "the experiment operates *essentially* with wave groups, no *individual* wave ever travels from the left end of the tube [containing the dispersive medium, carbon disulfide] to the right end; the waves entering at the left all die out during their wandering through the desert of carbon disulfide—those that do arrive at the right hand end are *completely different individual waves* which were born in the desert. And they come forth at the right end with the same inclinations as those with which their deceased parents entered at the left."

A week later Einstein wrote: "You were absolutely right."[25] He had discovered an error in his new calculations, and when it was corrected the predicted deviation disappeared. But the whole problem was a deceptive one, so that it was probably worth publishing the detailed calculation to clarify it. The opportunity was provided by the next meeting of the Prussian Academy on 2 February 1922.[26] The calculation Einstein presented was quite different in form from the Ehrenfest-Gibbs discussion, but the central point was the use of a

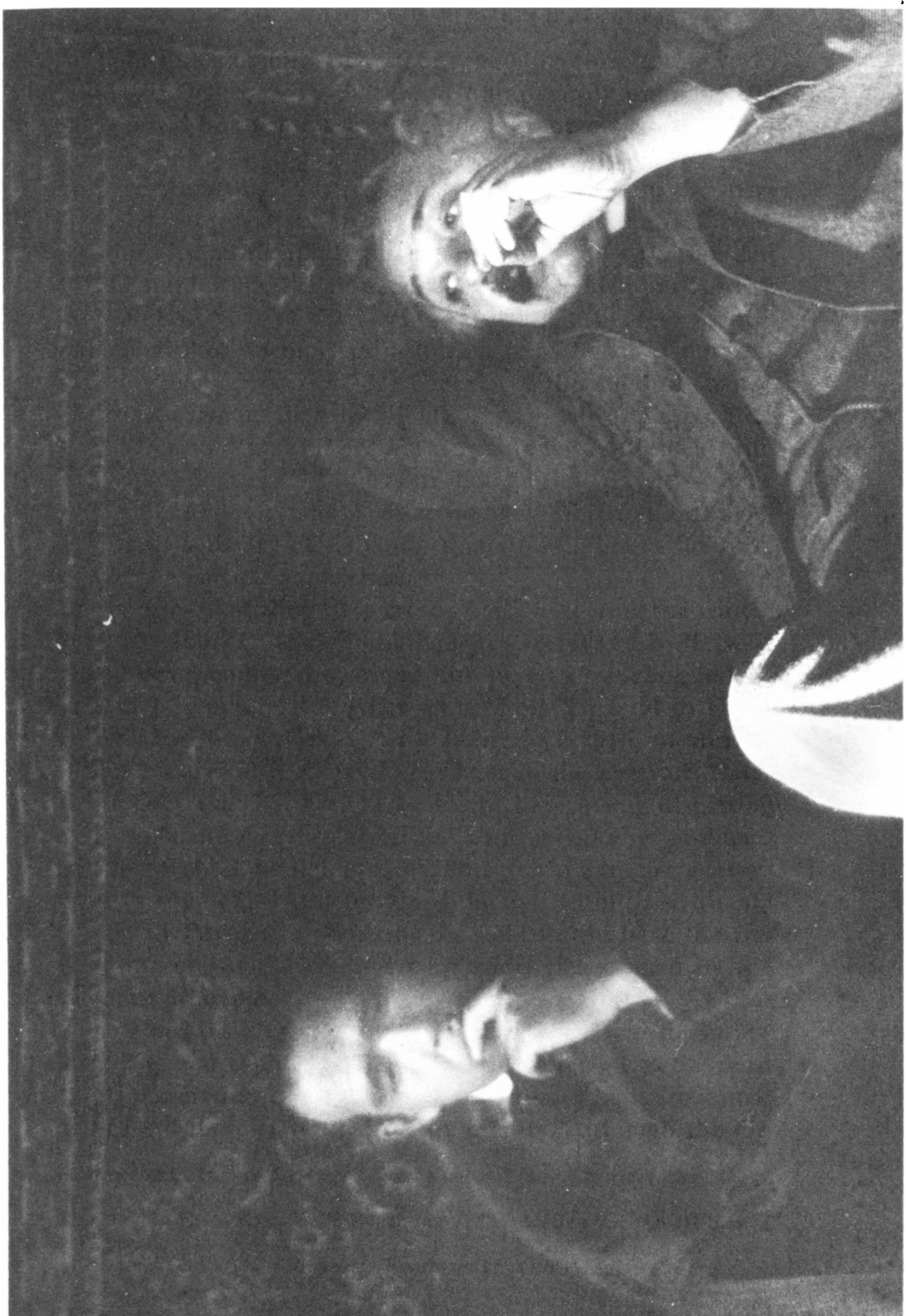
23. A. Einstein to P. Ehrenfest, received 22 January 1922.
24. P. Ehrenfest to A. Einstein, 26 January 1922.
25. A. Einstein to P. Ehrenfest, 30 January 1922.
26. A. Einstein, "Zur Theorie der Lichtfortpflanzung in dispergierenden Medien," *Berliner Berichte* (1922), p. 18.

wave group rather than an infinite train of waves. Einstein had to conclude that the negative result obtained by Geiger and Bothe did not allow one to infer anything about the wave or quantum nature of light emission.

4. It was, in any case, difficult for physicists to escape completely from Einstein's light quanta by the early 1920's, since the quanta provided such a successful way of accounting for phenomena like the photoelectric effect. Most physicists would, however, probably have agreed that, even though radiation must have quantal features, these features appeared only when light was emitted or absorbed, and that the free propagation of radiation had to be described by the classical wave theory.[27]

It became impossible to maintain this separation of the domains in which the wave and particle theories of light were applicable after the discovery of the Compton effect.[28] Arthur Compton found that the wavelength of X rays increased when they were scattered by free electrons. Both Compton[29] and Peter Debye[30] independently worked out the now familiar equations for this Compton scattering by treating the X ray as a particle, a quantum of energy $h\nu$ and momentum $h\nu/c$, and applying the conservation laws for momentum and energy to the collision between this quantum and an electron at rest. Since this approach led to a successful description of the Compton effect, most physicists considered the agreement between theory and experiment to be "definite evidence for the existence of light quanta," as J. H. Van Vleck put it.[31] Some were willing to go further. Arnold Sommerfeld, for example, wrote Compton that his discovery sounded "the death knell of the wave theory of radiation."[32] For now even freely propagating radiation seemed to have particle properties, as

27. See, for example, A. Sommerfeld, *Atomic Structure and Spectral Lines*, trans. by H. L. Brose from third German edition of 1922 (New York, 1923), p. 253.
28. A. H. Compton, "Secondary Radiations Produced by X-Rays, and Some of Their Applications to Physical Problems," *Bull. Nat. Res. Council, No. 20* (1922), p. 16.
29. A. H. Compton, "A Quantum Theory of the Scattering of X-Rays by Light Elements," *Phys. Rev., 21* (1923), 483.
30. P. Debye, "Zerstreuung von Röntgenstrahlen und Quantentheorie," *Phys. Z., 24* (1923), 161.
31. J. H. Van Vleck, "Quantum Principles and Line Spectra," *Bull. Nat. Res. Council, No. 54* (1926), p. 270.
32. Quoted by A. H. Compton in *J. Franklin Inst., 198* (1924), 70.



shown by the simple quantum theory of the Compton effect based solely on the conservation laws. The Compton effect was generally taken to be the kind of crucial experiment that Einstein had been looking for.

The quantal explanation of the Compton effect had other consequences. It made it possible to treat the old, perplexing, unsolved problem of the thermal equilibrium between free electrons and blackbody radiation. H. A. Lorentz and A. D. Fokker had worked on this problem a decade earlier and had been unable to construct a plausible theory that would account for a Maxwellian velocity distribution for the electrons and a Planck distribution for the radiation.[33] Wolfgang Pauli took up the problem again in 1923 and showed that Compton scattering provided a mechanism that would achieve this aim.[34] Pauli's method was a modification of the one used by Einstein in his 1916 theory of radiation. A few months after Pauli's paper appeared, Einstein and Ehrenfest showed that his argument could be clarified and made more intelligible if one looked at Compton scattering as a two-step process: the absorption of a quantum of frequency ν by an electron, and the emission of a quantum of frequency ν', both quanta being appropriately specified as to direction, and the entire process subjected to the energy and momentum conservation laws.[35] Once again Einstein's free light quanta seemed to be essential to the understanding of the whole effect.

Nevertheless, not quite everyone was persuaded that the Compton effect proved the existence of light quanta. The most important nonbeliever was Niels Bohr, and we must now try to see why he took up an opposing position.

5. Bohr's first famous series of papers, written in 1913, was concerned with the problem indicated by its title, "On the Constitution

33. See H. A. Lorentz, "Sur l'application au rayonnement du théorème de l'équipartition de l'énergie," in *La théorie du rayonnement et les quanta,* ed. P. Langevin and M. de Broglie (Paris, 1912), p. 35. See also W. Pauli, "Quantentheorie" in *Handbuch der Physik, 23, Quanten,* ed. H. Geiger (Berlin, 1926), p. 18. Reprinted in W. Pauli, *Collected Scientific Papers,* ed. R. Kronig and V. F. Weisskopf (New York, 1964), *1,* 288.

34. W. Pauli, "Über das thermische Gleichewicht zwischen Strahlung und freien Elektronen," *Z. Phys., 18* (1923), 272.

35. A. Einstein and P. Ehrenfest, "Zur Quantentheorie des Strahlungsgleichgewichts," *Z. Phys., 19* (1923), 301. See also M. J. Klein, *op. cit.* (note 11).

of Atoms and Molecules."[36] This research was not originally undertaken to explain atomic spectra, and Bohr's highly successful theory of the hydrogen spectrum, worked out only a few weeks before he sent the first paper of the series off for publication, seemed almost a distraction from what he described as the "main object of this paper —the discussion of the permanent state of a system consisting of nuclei and bound electrons."[37] As Bohr's ideas developed over the years, the analysis of spectra took on a more and more significant role, just because this analysis proved to be the best way to study atomic structure. Bohr's writings, and the old quantum theory generally, dealt with two basic problems: what is the nature of the stationary states of atomic systems, and how does the structure of the atom, defined through these stationary states, determine the physical and chemical properties of the corresponding element? (The character of the atomic spectrum was the most important of these physical properties.)

In none of Bohr's writings before 1922, so far as I know, did he concern himself at any length with the problem of the nature of radiation.[38] He did make a remark on this problem in a lecture in Berlin in 1920, with Einstein present in the audience, but it was only to put it aside for the time being. "I shall not," he had said, "here discuss the familiar difficulties to which the 'hypothesis of light quanta' leads in connection with the phenomenon of interference, for the explanation of which the classical theory of radiation has shown itself to be so remarkably suited. Above all I shall not consider the problem of the nature of radiation."[39]

Although Bohr did not write on the radiation problem, he certainly did think about it. He was deeply impressed by Einstein's new statistical derivation of the Planck distribution, which also supported his own characteristic postulate, and he incorporated the idea of transition probabilities, both spontaneous and induced, into his sub-

36. (a) N. Bohr, "On the Constitution of Atoms and Molecules," *Phil. Mag., 26* (1913), 1, 476, 857. (b) The three papers have been reprinted in a book with the same title (Copenhagen, 1963), with a long and informative introduction by L. Rosenfeld.

37. See Bohr, *op. cit.* (note 36[b]), p. 20.

38. The same comment was made by K. M. Meyer-Abich in his book, *op. cit.* (note 4[b]), p. 108.

39. N. Bohr, *The Theory of Spectra and Atomic Constitution* (Cambridge, 1922), p. 22.

sequent work. Bohr called particular attention to the fact that Einstein had made his basic assumptions in analogy to the classical theory of radiation.[40] The connection of new work to the classical theory was always a matter of concern to Bohr.

In his 1913 paper on the hydrogen spectrum Bohr had assumed that the frequency of a spectral line emitted by an atom was proportional to the difference between the energies of the initial and final stationary states of the atom. This, as Bohr said, was "in obvious contrast to the ordinary ideas of electrodynamics," since it destroyed the classical idea that the frequency emitted was the frequency of some internal motion in the atom. This assumption had been the most puzzling and disturbing feature of Bohr's theory. (Years later Erwin Schrödinger still described it as "monstrous" and "inconceivable."[41]) But Bohr had also shown that his new assumption led to frequencies that were completely consistent with what classical theory predicted for the emission in the long wavelength limit, the same limit in which Planck's distribution law reduced to the classical Rayleigh form.

Bohr took this result very seriously, so seriously indeed that it became a central theme and guiding principle in his work. In the region of large quantum numbers, where the stationary states are closely spaced and emitted wavelengths are long, the frequencies calculated from the quantum theory must agree with those calculated classically, and the transition probabilities are simply related to the amplitudes of the corresponding harmonic components in the motion. This principle of correspondence became a very powerful method for treating specific problems in the theory of spectra, but Bohr saw its real significance as being more than that: it made it possible, he wrote, "in a certain sense to regard this theory [the quantum theory of spectra] as a natural generalization of our ordinary ideas of radiation."[42] In his search for a new theory, the correspondence principle was one of the few sure guides; it gave Bohr a way of keeping in contact with the solid results of classical electro-

40. N. Bohr, "The Quantum Theory of Line-Spectra," *D. Kgl. Danske Vidensk. Selsk. Skrifter. Naturvidensk. og Mathem. Afd.*, (8) *4*, 1 (1918), 7.

41. E. Schrödinger, A. Einstein, M. Planck, and H. A. Lorentz, *Letters on Wave Mechanics*, ed. K. Przibram, trans. M. J. Klein (New York, 1967), p. 61.

42. N. Bohr, "The Effect of Electric and Magnetic Fields on Spectral Lines," *Proc. Phys. Soc.*, *35* (1923), 279.

magnetic theory, while seeking the quantum theory which would be its "natural generalization."

6. The third Solvay Congress, on "Atoms and Electrons," was held in April 1921. Bohr was to have been one of the principal speakers, but illness kept him from going to Brussels for the meeting. He never completed the report on the application of the quantum theory to atomic problems which he had planned to deliver, but a part of it was written. This was presented for him by Ehrenfest, and it appeared in the proceedings. This portion included a brief discussion of the radiation problem, emphasizing the importance of the correspondence principle.[43] Bohr devoted only a few sentences to the hypothesis of free light quanta, but one of his remarks is worth noticing here. "Such a concept," he wrote, "seems, on the one hand, to offer the only possibility of accounting for the photoelectric effect, *if we stick to the unrestricted applicability of the ideas of energy and momentum conservation*. On the other hand, however, it presents apparently insurmountable difficulties from the point of view of the phenomena of optical interference. . . ." The words I have emphasized suggest that Bohr was already considering the possibility that the conservation laws might *not* be universally valid.

That Bohr was indeed thinking along these lines is confirmed by Paul Ehrenfest's description of his friend's ideas some months later. In a postcard to Einstein which has already been quoted above, Ehrenfest wrote that he was very curious to know Bohr's reaction to Einstein's proposed crucial experiment. Ehrenfest had recently been in Copenhagen to visit Bohr, and he tried to give Einstein an idea of Bohr's current thinking. "If I am able to reproduce his opinion on these matters correctly, I might formulate it this way. He is much more willing to give up the energy and momentum theorems (in their classical form) for elementary atomic processes, and to maintain them only statistically, than to 'lay the blame on the aether.' "[44]

The idea that a restriction on the validity of the conservation laws might provide a way of reconciling the wave and quantum aspects of light seems to have occurred to several people in this period. As

43. N. Bohr, "L'application de la théorie des quanta aux problèmes atomiques," in *Atomes et électrons* (Paris, 1923), pp. 241–242.
44. P. Ehrenfest to A. Einstein, 17 January 1922.

early as the summer of 1919 Charles Darwin wrote to Bohr, sending him his general views on the problems of the quantum theory.[45] Darwin considered "the case against conservation quite overwhelming." He remarked that Frederick Lindemann had told him of a conversation with Einstein on this subject: Einstein "had tried without conservation," but found "it was no better than with."

Bohr immediately began to compose a long answer to Darwin's letter, in which he tried to formulate his own views on some of the basic questions of principle in the quantum theory. This proved to be too big a problem to be solved in one letter—it was Bohr's lifework—and the answer to Darwin was never sent.[46] The unfinished draft of Bohr's ideas was preserved, however, and it presents a remarkable sketch of "the scientific conscience (bad or good?) of a Quanticist," as Bohr described it almost three years later, when he finally did write to Darwin.[47]

Bohr started with some general remarks about the nature of scientific reasoning, presumably prompted by Darwin's criticism of the proofs of the necessity of a quantum theory, such as that given by Poincaré.[48] "All progress in science emphasizes difficulties," he wrote. "All progress in physics no proofs whatever but only simple connections of different conceptions." Bohr even went so far as to describe "most general reasoning in science as opportunistic." He agreed with Darwin that the photoelectric effect was "by far the central evidence" for the applicability of quanta to nonstatistical phenomena. Bohr also thought, however, that the "wonderful inverse of the photoelectric effect which we see in the phenomena of excitation of spectral lines" would "make the case very hard" for any attempt to explain such things statistically. He was inclined to accept the rather widespread view that the wave theory of light was valid for freely propagating radiation and "that all difficulties are concentrated on the interaction between the electromagnetic forces and matter." With respect to these interactions, however, Bohr was "inclined to take

45. C. Darwin to N. Bohr, 20 July 1919. (The documents referred to in notes 45–47 are part of the Archive for the History of Quantum Physics. They were pointed out to me by Professor Roger Stuewer.)

46. N. Bohr, undated draft of a letter to C. Darwin.

47. N. Bohr to C. Darwin, 14 February 1922.

48. H. Poincaré, "Sur la théorie des quanta," *Journal de Physique*, *2* (1912), 5. See R. McCormmach, "Henri Poincaré and the Quantum Theory," *Isis*, *58* (1967), 37.

the most radical or rather mystical views imaginable." He thought that conservation of energy was "quite out of question," and wondered if the frequency of the incident light were just "the key to the lock which controls the starting of the interatomic [intra-atomic?] process." Even the definition of energy was not a trivial problem in the quantum theory, and required the use of "the principle of mechanical transformability," Bohr's name for Ehrenfest's adiabatic principle. Only this kept it from being "so criminal as it looks at first sight to speak with such light heart of the fundamental difficulties touched upon above and still to attempt to be a serious worker in the present cribbled [crippled?] field of physics."

These comments were only part of the preliminary draft of a letter that was never sent, but Bohr expressed himself in much the same vein in a major paper several years later.[49] This work, completed in November 1922, was intended as the first of a series of essays on the application of the quantum theory to atomic structure. It was actually the only one of the series to be published, and it dealt with the fundamental postulates of the theory. Bohr tried to state and explain the principles which formed the basis for the applications of the theory to atomic structure, applications which had already included a wide range of phenomena and were constantly being extended. But even more than that, Bohr wanted to treat the question of "whether it is possible to present the principles of the quantum theory in such a way that their application appears free from contradiction."

It was in connection with this last question that Bohr finally confronted Einstein's hypothesis of light quanta, in the concluding chapter of his long paper. This chapter was entitled, "On the formal nature of the quantum theory," and Bohr emphasized that the hypothesis of light quanta should be considered as being only *formal*. The view that light propagated as localized and indivisible packets of energy had "placed certain classes of phenomena, such as the photoelectric effect, in a clear light in relation to the quantum theory," but it could "in no wise be regarded as a satisfactory solution." The light quantum hypothesis gave rise to "insuperable difficulties when applied to the explanation of the phenomena of

49. N. Bohr, "On the Application of the Quantum Theory to Atomic Structure, Part I, The Fundamental Postulates of the Quantum Theory," *Proc. Cambr. Phil. Soc. (Supplement)* (1924). (Originally published in German in *Z. Phys., 13* [1923], 117.)

interference," and it even "excluded in principle the possibility of a rational definition of the conception of a frequency ν, which plays a principal part in this theory." One could not make an adequate picture of the processes involved, if one started with Einstein's hypothesis. In addition, the success of the hypothesis of light quanta in accounting for "certain aspects of the phenomena" supported the view that "a description of atomic processes in terms of space and time cannot be carried through in a manner free from contradiction by the use of conceptions borrowed from classical electrodynamics, which, up to this time, have been our only means of formulating the principles which form the basis of the actual applications of the quantum theory."[50]

Bohr also emphasized the great difficulties involved in trying to make a quantum theory of dispersion. The classical theory assumed that the illuminated atom produced secondary waves coherently related to the incident waves. The characteristic frequencies of the equivalent oscillators would, however, have to be those observed in the absorption spectrum, and one of the cardinal assumptions of the quantum theory was that the absorption frequencies were not the frequencies of any actual electronic motions in the atom.[51]

One conclusion could be drawn from all the difficulties: "A general description of the phenomena, in which the laws of the conservation of energy and momentum retain in detail their validity in their classical formulation, cannot be carried through." As a result, Bohr warned, "We must be prepared for the fact that deductions from these laws will not possess unlimited validity."[52] It was not the conservation laws but rather the correspondence principle and Ehrenfest's adiabatic principle to which Bohr looked for guidance. They were "suited, in a higher degree, to point out new ways for further extensions of the quantum theory of atomic structure," and they offered "a hope in the future of a consistent theory, which at the same time reproduces the characteristic features of the quantum theory . . . and, nevertheless, can be regarded as a rational generalisation of classical electrodynamics."[53]

Bohr's paper was published in the spring of 1923, so that his re-

50. *Ibid.*, pp. 34–35.
51. *Ibid.*, p. 38.
52. *Ibid.*, p. 40.
53. *Ibid.*, p. 42.

jection of light quanta appeared at about the same time as the works of Compton and Debye, works that showed how simply and naturally light quanta could be used to account for the Compton effect.

7. There were other physicists ready to consider giving up the strict validity of the conservation laws by this time. Sommerfeld commented on this idea in his influential treatise on atomic structure and spectra: "The mildest modification that must be applied to the wave theory is, therefore, that of disavowing the energy theorem for the single radiation phenomenon and allowing it to be valid only on the average for many processes." He considered this kind of change to be much less extreme than taking the light quantum hypothesis as valid, which would make Maxwell's equations for the field into "statistical approximations."[54] But even though some physicists were willing to deny the conservation laws, nobody was able to use this denial to construct a theory of radiation that could account for both the wave and the particle properties.

One of the people puzzling over these questions during the winter of 1923/24 was John C. Slater, who had just received his Ph.D. at Harvard and was spending the year in Europe on a traveling fellowship. Early in December 1923 Slater wrote from England to H. A. Kramers, who had been working with Bohr since 1916, to arrange the details of his arrival in Copenhagen later in the month.[55] Slater mentioned briefly that he thought he might have a way of getting a consistent explanation of dispersion and a variety of other problems by putting the emphasis on light quanta. One would have to construct an electromagnetic field that determined the motion of these quanta with the help of Poynting's theorem; the field would have the frequencies of the emission lines and amplitudes determined by the correspondence principle.

Slater explained his idea more clearly in a letter he sent to *Nature* from Copenhagen at the end of January.[56] He referred to the need for achieving consistency between those properties of light accounted for by waves and those accounted for by quanta. Although the dis-

54. A Sommerfeld, *Atomic Structure and Spectral Lines, op. cit.* (note 27), p. 253.

55. J. C. Slater to H. A. Kramers, 8 December 1923. (Archive for the History of Quantum Physics.)

56. J. C. Slater, "Radiation and Atoms," *Nature, 113* (1924), 307.

continuous side of the story was "apparently the more fundamental," Slater thought he could make progress "by associating the essentially continuous radiation field with the continuity of existence in stationary states, and the discontinuous changes of energy and momentum with the discontinuous transitions from one state to another."

He assumed that an atom in one of its stationary states was surrounded by "a virtual field of radiation, originating from oscillators having the frequencies of possible quantum transitions." This virtual radiation field would provide for statistical conservation of energy and momentum by determining the probabilities of the possible transitions. The virtual field of a given frequency produced by the atom itself would determine the spontaneous transition probability, while the virtual fields due to other atoms would determine the probabilities for induced emission or absorption, much as Einstein had suggested. When an atomic transition occurred, the virtual radiation field would have to change character abruptly, so that the frequencies it then contained would be those appropriate to the new stationary state of the atom.

Slater remarked that although his original goal had been to construct a field that would serve to guide light quanta, he had been persuaded by Kramers that his new approach really implied "a much greater independence between transition processes in distant atoms than [he] had perceived." Many years later Slater explained that the statistical version of the conservation laws was, in his words, "put into the theory by Bohr and Kramers, quite against my better judgment." He would have preferred to keep the light quanta "as real entities" and to have them satisfy the conservation laws exactly, but "Bohr and Kramers opposed this view so vigorously" that he went along with them "to keep peace and get the main part of the suggestion published."[57]

In his letter to *Nature,* Slater referred to a forthcoming paper, written jointly with Bohr and Kramers, for more details. Although the paper did appear under the names of all three men, Slater wrote to a friend that Bohr and Kramers had actually written all of it,[58] and the paper is certainly in Bohr's unmistakable style. Slater's vir-

57. J. C. Slater to B. L. van der Waerden, 4 November 1964. Quoted in B. L. van der Waerden, *op. cit.* (note 4[c]), p. 13.

58. J. C. Slater to J. H. Van Vleck, 27 July 1924. (Archive for the History of Quantum Physics.)

tual radiation field—associated with an atom in one of its stationary states and orginally intended as part of a theory of light quanta—had now become the core of a program for a new theory of radiation, a theory that would have no use for light quanta.

Bohr began with a long introduction, emphasizing both the difficulties in the basic ideas of the quantum theory and the progress that had nevertheless been made in atomic physics by using some of these ideas.[59] He pointed out once again that the quantum theory had only a "formal character" since it did not provide "a description of the mechanism of the discontinuous processes" that it used. Bohr mentioned Einstein's arguments based on the conditions for thermodynamic equilibrium between matter and radiation, and Einstein's conclusion from these arguments that light quanta must carry momentum $h\nu/c$ in definite directions. He admitted that this conclusion was "considered as an argument for ascribing a certain physical reality to the theory of light quanta," and that it had recently been used with great success to explain the Compton effect and to clarify other problems.[60] Bohr's next paragraph, however, showed why he was not satisfied with this apparent progress. The quantum theory of atomic processes "must in a certain sense ultimately appear as a natural generalization" of classical electrodynamics. This principle, the correspondence principle in the broadest sense, was central for Bohr; it was his only guidance in the attempt to create a new physics. Einstein's light quanta could not be understood on correspondence terms, and for that reason the radiation theory would have to be constructed in some other way. Slater's idea seemed to provide the necessary starting point. This new approach did not "in any way remove the formal character of the theory"—it did not give the mechanism of transitions or avoid their probabilistic description—but it was "a definite advance" in its reinterpretation of radiation phenomena.[61]

An atom in a stationary state was to be thought of as "communicating continually with other atoms" through a virtual radiation field whose frequencies were those of all transitions from this state allowed by the earlier Bohr theory. In each stationary state the atom would

59. N. Bohr, H. A. Kramers, and J. C. Slater, "The Quantum Theory of Radiation," *Phil. Mag.*, *47* (1924), 785.
60. *Ibid.*, p. 789.
61. *Ibid.*, p. 790.

then be equivalent to a set of virtual harmonic oscillators, an approach already used a few years before by R. Ladenburg in his work on dispersion.[62] The frequency, intensity, and polarization of spectral lines would be related to the structure of the atom exactly as before, but the occurrence of transitions would be determined in quite a different way.

According to the Bohr, Kramers, Slater theory the occurrence of a transition in an atom would depend on the initial stationary state of that atom and on the states of those other atoms that produce the virtual radiation field at its location; it would not, however, depend on the occurrence of a transition in one of the latter atoms. This meant abandoning "any attempt at a causal connection between the transitions in distant atoms, and especially a direct application of the principles of conservation of energy and momentum, so characteristic for the classical theories."[63] Thus the light quantum theory would say that if an atom absorbed a quantum $h\nu$ in making a transition from state 1 to state 2 (where $h\nu = E_2 - E_1$), then some other atom must have previously emitted this quantum by making a transition from state 2 to state 1. The new theory, on the other hand, would say that the atom in state 1, which absorbs the radiation, must be subject to a virtual field of frequency ν, produced by another atom in state 2, but that no actual transition of this second atom would be required for the absorption to occur. This second atom could remain in state 2, and yet its virtual radiation field could produce transitions in which any number of other atoms gain the energy $E_2 - E_1$.

Although energy would not be conserved in the individual process of emission or absorption according to this point of view, there would still have to be conservation on the average over many such events. This was provided for by "the peculiarities of the interaction between the virtual field of radiation and the illuminated atoms."[64] The authors contemplated a mechanism much like that of the classical wave theory. An atom illuminated by virtual radiation would act as a source of secondary virtual radiation of the same frequency.

62. R. Ladenburg, "Die quantentheoretische Deutung der Zahl der Dispersionselektronen," *Z. Phys.*, *4* (1921), 451. English translation in B. L. van der Waerden, *op. cit.* (note 4[c]), p. 139.
63. Bohr, Kramers, and Slater, *op. cit.* (note 59), p. 791.
64. *Ibid.*, p. 793.

The amplitude of these secondary virtual waves would be large when and only when the incident frequency was very near the frequency of one of the virtual oscillators associated with the stationary state in which the atom happened to be. The relative phase of secondary and incident waves would determine whether the interference between the two would lead to a decrease or an increase in the intensity of the virtual radiation field. If this intensity were decreased, for example, the virtual field would be less capable of inducing transitions in other atoms. (The probability of such an induced transition, the probability that Einstein had introduced, was assumed to be determined by the intensity of the virtual radiation field at the frequency corresponding to the transition.) When the virtual oscillator corresponded to a transition that increased the atom's energy, the phase relations would have to be such as to decrease the intensity of the virtual radiation field, thereby ensuring the statistical validity of the conservation of energy. Similar remarks could be made for the momentum law, since a transition involving an energy change $h\nu$ would also produce a momentum change $h\nu/c$ in some direction.

Bohr, Kramers, and Slater discussed a variety of physical phenomena in their paper, showing how they were to be interpreted according to the new radiation theory. In some cases the new theory led to experimental consequences that differed sharply from those of the old. If, for example, one had a beam of atoms emerging from a luminescent discharge into a vacuum and one asked for the duration of the luminosity in the beam, the old and new theories gave different answers. According to the old theory, even if all the emerging atoms were to be in the same excited state and to have the same speed, the decay of the luminosity would vary as a superposition of exponential decays, one exponential for each different transition probability corresponding to each of the emitted lines. On the new theory, however, all of the spectral lines starting in the given state would decay at one and the same rate. The experimental data available did not allow one to distinguish between these two distinctly different predictions.[65]

Another experiment which Bohr and his collaborators could hardly avoid discussing was the Compton effect, especially as it was generally considered to be direct evidence for the existence of light quanta. Compton himself had already shown that the modified radiation with

65. *Ibid.*, p. 794.

its increased wavelength could be interpreted formally as secondary radiation coming from an imaginary recoiling source which produced a Doppler effect. This imaginary source could not be identified with the actual illuminated electron, since the velocity one had to ascribe to it differed from that of the electron. Bohr had to admit that this feature of the virtual wave interpretation was "strikingly unfamiliar to the classical conceptions," but he thought that this was no reason to reject it as inadequate, since it was only a "formal interpretation." Although the emission of scattered radiation of the proper modified wavelength could be accounted for in this way on the continuous virtual wave theory, Bohr and his collaborators still had to admit a discontinuous element into their analysis. They had to assume "that the illuminated electron possesses a certain probability of taking up in unit time a finite amount of momentum in any given direction."[66] The recoil of an electron and the production of scattered radiation of modified wavelength would be uncorrelated events on this view, a conclusion in sharp contrast to the results of the Compton-Debye analysis based on light quanta.

8. No paper by Bohr would have been ignored in 1924, and certainly not a paper dealing with such fundamental issues as this one did. But it was never easy to grasp Bohr's meaning, and this time there was no structure of equations to help guide the reader through Bohr's dense and difficult prose. The complete mathematical content of the seventeen-page paper was, in fact, the single equation, $h\nu = E_1 - E_2$.

One of the first to react in print was Erwin Schrödinger.[67] He was much taken with the new way of looking at radiation, partly because it emphasized the continuous rather than the discontinuous aspects of the phenomena. Schrödinger liked this idea of a return to a wave theory which could dispense with light quanta. (He never even mentioned, however, that the radiation fields produced by atoms in their stationary states were only virtual fields.) What particularly attracted Schrödinger was the proposal that energy conservation is only a statistical law. He was familiar with this conjecture through the work

66. *Ibid.*, p. 799.

67. E. Schrödinger, "Bohrs neue Strahlungshypothese und der Energiesatz," *Naturwissenschaften, 12* (1924), 720. See the discussion in W. T. Scott, *Erwin Schrödinger. An Introduction to His Writings* (Amherst, 1967), pp. 30, 48.

of his teacher, Franz Exner, and had already speculated in his Zürich inaugural lecture about the possibility that the world was fundamentally acausal and that the conservation laws were only statistically valid. The Bohr, Kramers, Slater theory put these old conjectures and speculations "for the first time into a form one could grasp," and Schrödinger proceeded to work out some of its consequences, after giving a brief exposition of the theory in his own crisp style. Schrödinger emphasized one implication of statistical energy conservation: there must be corresponding fluctuations in the energy of an isolated system, "true" energy fluctuations in the sense that they were not due to contact with another system such as a heat bath. Although these intrinsic fluctuations would not have the disastrous consequences that a first inspection of the problem suggested (as Schrödinger demonstrated with the help of an example based on the runaway inflation of the times), they would have some peculiar features. While they would normally be much too small to be detected, they would necessarily grow arbitrarily large as the time of observation increased, a very puzzling result at best.

Other physicists attempted to work out the details of the program suggested by Bohr and his co-workers. Richard Becker in Berlin tried to construct a unified theory of absorption and dispersion on the basis of the new approach to radiation.[68] These phenomena had previously been treated quite separately, by the quantum and wave methods respectively. Becker tried to specify the nature of the spherical waves radiated by an atom in a stationary state in such a way that both phenomena could be understood together. In Amsterdam, J. D. van der Waals, Jr., pointed to an inconsistency in the Bohr, Kramers, Slater theory.[69] He saw no reason why one should not be able to treat the absorption of momentum continuously in this theory, just as one treated the absorption of energy. He thought this way of treating momentum was necessary for the consistency of the theory, and questioned the cogency of Einstein's old arguments that purported to make discontinuous momentum changes $h\nu/c$ into a thermodynamic necessity. This last point was also made at greater length by Pascual

68. R. Becker, "Über Absorption und Dispersion in Bohrs Quantentheorie," *Z. Phys.*, *27* (1924), 173.

69. J. D. van der Waals, Jr., "Remarques relatives à des questions du domaine de la théorie des quanta," *Arch. Néerl.*, *8* (1925), 300. This paper was pointed out to me by Professor Paul Forman.

Jordan in his Göttingen dissertation.[70] Jordan went on to suggest a generalization of Einstein's statistical arguments that would avoid the necessity for "needle radiation" but would still lead to Planck's radiation law. Einstein soon pointed out, however, that despite the "ingenious" nature of Jordan's reasoning, his generalization went too far; it would prevent one from defining an absorption coefficient at all, because Jordan did not treat the absorption of radiation coming from different directions as completely independent processes.[71]

Experimentalists also responded to the suggestions of Bohr, Kramers, and Slater. Within a month or so of the appearance of their paper, Walther Bothe and Hans Geiger proposed an experiment that would test one crucial prediction of the theory.[72] When one interpreted the Compton effect on the basis of the new theory, one had to conclude that the scattering of X radiation with an increase in wavelength was not necessarily correlated with the recoil of an electron. Bothe and Geiger announced that they were preparing to search for coincidences between Compton recoil electrons and scattered X rays (or, more precisely, the photoelectrons produced by the absorption of the scattered X rays) in an experiment using counters.

At about the same time Arthur Compton undertook an experiment, in collaboration with Alfred W. Simon, to test the same basic point.[73] Compton and Simon used the Wilson cloud chamber, rather than counters, and tried to check the relationship between the angle of scattering of the X rays (observed by means of secondary photoelectrons) and the angle of recoil of the Compton electron. The light quantum theory predicted a unique relationship between these two angles, while the new theory of Bohr, Kramers, and Slater called for no correlation between them. Both this experiment and Bothe and Geiger's were difficult, and their results were not available until the summer of 1925.

At Copenhagen the ideas of the Bohr, Kramers, Slater theory were developed in a variety of directions. Kramers took up the idea that

70. P. Jordan, "Zur Theorie der Quantenstrahlung," *Z. Phys., 30* (1924), 297. Similar criticisms were made by others. See J. H. Van Vleck, *op. cit.* (note 31), p. 269 for references. Also see W. Pauli, *op. cit.* (note 33), p. 16.

71. A. Einstein, "Bemerkung zu P. Jordans Abhandlung 'Zur Theorie der Quantenstrahlung,'" *Z. Phys., 31* (1925), 784.

72. W. Bothe and H. Geiger, "Ein Weg zur experimentellen Nachprüfung der Theorie von Bohr, Kramers und Slater," *Z. Phys., 26* (1924), 44.

73. A. H. Compton and A. W. Simon, "Directed Quanta of Scattered X-Rays," *Phys. Rev., 26* (1925), 289.

an atom in a stationary state acts like a set of virtual oscillators having the frequencies of all absorption and emission lines starting in that state.[74] The theory of dispersion that he developed on this basis made no use of the more controversial aspects of the joint paper. Kramers continued the work on dispersion in collaboration with Werner Heisenberg, and this work was the immediate predecessor of Heisenberg's new quantum mechanics.[75]

Bohr himself struggled to understand the full implications of his new ideas by examining their consequences for other kinds of atomic processes.[76] He made an analysis of the whole problem of collisions of atoms with charged particles. This problem dealt with a range of phenomena that showed the same kind of duality between classical continuity and quantal discreteness that one had in the domain of radiation. At one extreme was a process like the scattering of very fast alpha particles by atoms. It was here that a purely classical analysis had led Rutherford to the concept of the nuclear atom, on which all later developments in atomic physics were based. At the other extreme was the Franck-Hertz experiment, as clear a demonstration of the effects of discrete, quantized atomic states as one could ask for. In between one had a whole range of situations, including the loss of energy by charged particles passing through matter, a phenomenon Bohr had worked on years before.[77] Bohr argued that one could classify atomic collision phenomena according to whether or not they exhibited "reciprocity," by which he meant "a mutual coupling of the participating systems of such a nature that the collision would only be considered as completed, from the standpoint of either system, when the other system is brought into that stationary state which is taken to be the final result of the interaction."[78] Reciprocal collision processes were those that had inverse processes, like the

74. H. A. Kramers, "The Law of Dispersion and Bohr's Theory of Spectra," *Nature, 113* (1924), 673; "The Quantum Theory of Dispersion," *Nature, 114* (1924), 310. Both reprinted in van der Waerden, pp. 177, 199. There are also several unpublished manuscripts by Kramers from this period in the Archive for the History of Quantum Physics, which deal with possible extensions of the Bohr, Kramers, Slater work.

75. H. A. Kramers and W. Heisenberg, "Über die Streuung von Strahlen durch Atome," *Z. Phys., 31* (1925), 681. English trans. in van der Waerden, p. 223.

76. N. Bohr, "Über die Wirkung von Atomen bei Stössen," *Z. Phys., 34* (1925), 142.

77. N. Bohr, "On the Decrease of Velocity of Swiftly Moving Electrified Particles in Passing through Matter," *Phil. Mag., 30* (1915), 581.

78. N. Bohr, *op. cit.* (note 76), p. 143.

Franck-Hertz collisions. The absence of such inverse processes for nonreciprocal ones led, in Bohr's view, to difficulties in interpretation when one applied the exact conservation laws to them. Nonreciprocal collisions, such as those between a fast α particle and an atom, in which the duration of the collision is short compared to the periods of electronic motions within the atom, would be analogous to radiation processes: the conservation laws could be expected to be valid only on the average for such collisions.

Bohr proposed various ways in which his suggested distinction between reciprocal and nonreciprocal interactions might be tested experimentally. In this paper Bohr's characteristic expression, "We must be prepared to find . . . ," is used a number of times, warning the reader that nature is likely to be harder to understand and less adaptable to existing categories than he expects. Bohr concluded by emphasizing the tentative character of his proposed new viewpoint, although it did seem to offer a way out of the difficulties of maintaining both the quantum theory of atomic phenomena and the conservation laws.

9. Bohr had been reluctant to accept light quanta. Einstein was even more reluctant to accept the alternative Bohr proposed. "Bohr's views on radiation interest me very much," he wrote to Hedwig Born, Max Born's wife, in April 1924. "But I shouldn't let myself be pushed into renouncing strict causality before it had been defended altogether differently from anything done up to now. The idea that an electron ejected by a light ray can choose *of its own free will* the moment and direction in which it will fly off, is intolerable to me. If it comes to that, I would rather be a shoemaker or even an employee in a gambling casino than a physicist. My attempts to give quanta a form one can grasp have failed again and again, it is true, but I am far from giving up hope."[79]

Einstein had some more specific things to say in a letter to Ehrenfest at the end of May:

> I reviewed the Bohr, Kramers, Slater paper at our colloquium the other day. This idea is an old acquaintance of mine, but I don't consider it to be the real thing. Principal reasons:

79. A. Einstein to H. Born, 29 April 1924. Quoted in M. Born, *Physik im Wandel meiner Zeit,* 4th ed. (Braunschweig, 1966), p. 294.

(1) Nature seems to adhere strictly to the conservation laws (Franck-Hertz, Stokes's rule). Why should action at a distance be an exception?

(2) A box with reflecting walls containing radiation, in empty space that is free of radiation, would have to carry out an ever increasing Brownian motion.

(3) A final abandonment of strict causality is very hard for me to tolerate.

(4) One would also almost have to require the existence of a *virtual* acoustic (elastic) radiation field for solids. For it is not easy to believe that quantum *mechanics* necessarily requires an electrical theory of matter as its foundation.

(5) The occurrence of ordinary scattering (not at the proper frequency of the molecules), which is above all standard for the optical behavior of bodies, fits badly into the scheme. . . .[80]

In the brief notes he wrote out for his colloquium talk, Einstein had included several other criticisms of the Bohr, Kramers, Slater idea.[81] He found the preordained harmony between the probabilities of absorption and emission and the intensities of the virtual radiation to be unsatisfactory. He wondered how the virtual field was to be arranged so that the return of a formerly free electron would correspond to a Bohr orbit, and considered this point "very suspicious."

A few months later Einstein mentioned the subject again in another letter to Ehrenfest, saying that Bohr and his collaborators had "abolished free quanta," but adding that free quanta "would not allow themselves to be dispensed with."[82] Ehrenfest, who would be more and more torn between the conflicting views of his two close friends as the years went by, wrote back: "If Bothe and Geiger find 'statistical independence' of the electron and the scattered light quantum it will prove *nothing*. But if they find a *correlation* it will be a triumph of Einstein over Bohr. This time, as an exception, I firmly believe you are right, and I would therefore be happy if the correlation were to be demonstrated."[83]

Even the newspapers were aware of the difference of opinion between Bohr and Einstein on a question of fundamental principle. At

80. A. Einstein to P. Ehrenfest, 31 May 1924.
81. Unpublished note in the Einstein Archive.
82. A. Einstein to P. Ehrenfest, 12 July 1924.
83. P. Ehrenfest to A. Einstein, 9 January 1925.

the end of October 1924 Einstein received a letter from Kurt Joel, a member of the editorial staff of the *Vossische Zeitung* in Berlin.[84] Joel wrote that reports and dispatches from Copenhagen indicated that there was a controversy over the nature of light and the conservation of energy, and that the outcome was likely to be decided by an experiment being performed by Geiger and Bothe in Berlin. He asked if Einstein would be kind enough to supply more information. Einstein had had some experience with the ways of the press by this time, and his answer was very brief.[85] Yes, there was a real difference between him and Bohr over the nature of light, but the reports Joel had forwarded were evidently from a not very well informed source. And, he added, there had been no written exchange of views with Bohr on this subject. (The two men never did correspond much with each other.)

When the results of the experiment by Bothe and Geiger were announced, after months of rumors, they seemed quite unambiguous. The observed counter coincidences between the Compton recoil electrons and the scattered X rays were orders of magnitude greater than the purely chance coincidences predicted by Bohr, Kramers, and Slater. "The experiments described are incompatible with Bohr's interpretation of the Compton effect," Bothe and Geiger wrote. The conclusion was clear: "One must therefore admit that the concept of light quanta possesses more reality than is supposed in this theory."[86]

Compton and Simon came to the same conclusion as a result of their cloud chamber test of the relationship between the angles of scattering and recoil. "These results do not appear to be reconcilable with the view of the statistical production of recoil and photoelectrons proposed by Bohr, Kramers, and Slater. They are, on the other hand, in direct support of the view that energy and momentum are conserved during the interaction between radiation and individual electrons." They also saw their results as directly supporting Einstein's picture of "directed quanta of radiant energy."[87]

84. K. Joel to A. Einstein, 28 October 1924.
85. A. Einstein to K. Joel, 3 November 1924.
86. W. Bothe and H. Geiger, "Über das Wesen des Comptoneffekts; ein experimenteller Beitrag zur Theorie der Strahlung," *Z. Phys., 32* (1925), 639. See also W. Bothe, "Absorption und Zerstreuung von Röntgenstrahlen" in *Handbuch der Physik, 23, Quanten*, ed. H. Geiger (Berlin, 1926), pp. 423–424.
87. A. H. Compton and A. W. Simon, *op. cit.* (note 73), p. 299.

Einstein remarked simply, in a letter to Ehrenfest: "We both had no doubts about it."[88]

10. The experimental refutation of the Bohr, Kramers, Slater theory did not solve any of the perplexing problems of radiation. The development of physics seemed to have produced insoluble difficulties. To one physicist, O. D. Chwolson in Leningrad, Bohr's proposal served as an instance of "what peculiar things the current efforts, one may well say the current desperate efforts, of physicists lead them to, as they strive to get physics out of the blind alley it is in now."[89] And J. H. Van Vleck, who was struggling with the problems himself, commented in a similar vein in the summer of 1925: "Modern physics certainly is passing through contortions in its attempt to explain the simultaneous appearance of quantum and classical phenomena; but it is not surprising that paradoxical theories are required to explain paradoxical phenomena."[90]

Bohr's response to the results of the Bothe-Geiger experiment came in a long "Postscript" that he added in July 1925 to his paper on atomic collisions, which was already in proof.[91] Bothe and Geiger had proved that the individual processes involved in the Compton effect were really coupled and not statistically independent as Bohr and his collaborators had proposed. The question now was, what did this mean? Which alternatives were now ruled out and which were still open? Bohr emphasized that the outcome of the Bothe-Geiger experiment "could not be looked at as simply distinguishing between two well-defined ways of describing the propagation of light in empty space, which would correspond to either a corpuscular or a wave theory of light." The problem lay deeper: what were the limits within which one could apply to atomic processes the kind of space-time picture that had previously served for the description of natural phenomena? The Bohr, Kramers, Slater theory had tried giving up the strict validity of the conservation laws just because there seemed to be no imaginable space-time mechanism which maintained the causal connections between individual atomic radiative processes and also managed to preserve a sufficiently close tie with the ideas of

88. A. Einstein to P. Ehrenfest, 18 August 1925.
89. O. D. Chwolson, *Die Physik 1914–1926* (Braunschweig, 1927), p. 392.
90. J. H. Van Vleck, *op. cit.* (note 31), p. 287.
91. N. Bohr, *op. cit.* (note 76), pp. 154–157.

classical electrodynamics. Now, despite the successful development of some of the ideas of Bohr, Kramers, and Slater in the theory of dispersion, the experimental results had closed off that way out of the difficulties. Since these results seemed to argue for the kind of corpuscular theory of light associated with Einstein's light quanta, Bohr warned that "one must be prepared to find that the generalization of classical electrodynamic theory that we are striving after will require a sweeping revolution in the concepts on which the description of nature has been based up to now."

That most critical of physicists, Wolfgang Pauli, agreed with Bohr's harsh conclusion.[92] He was convinced that light quanta must be assigned no less reality than electrons. Pauli thought that what needed thoroughgoing revision was not the energy and momentum laws but rather the classical concepts of force and motion, and especially the classical concept of the electromagnetic field. Pauli had been more than usually skeptical of the Bohr, Kramers, Slater theory anyway, and was happy to see it so quickly discredited by the experiments. He suggested to Kramers that it might otherwise have soon become a hindrance to the development of theoretical physics, particularly for those physicists whose sense of reality was not so strong as Bohr's.[93]

Einstein was as convinced as Bohr that there would be no easy answer to the riddle of radiation. He had never imagined that his light quantum hypothesis constituted a real theory, nor did he ever give up his efforts to construct such a theory, one that would unify the disparate concepts of particle mechanics and field electrodynamics. In December 1923—when the Compton effect finally persuaded many physicists that radiation did have the corpuscular features that Einstein had pointed out almost twenty years earlier, and when Bohr was ready to consider giving up causality, conservation, and detailed space-time descriptions of atomic phenomena—Einstein was pointing his researches in another direction. He read a paper to the Prussian Academy on the question, "Does field theory offer any possibilities for the solution of the quantum problem?"[94] This time it was Einstein who emphasized the "wonderful certainty" with which

92. W. Pauli, *op. cit.* (note 33), p. 86.

93. W. Pauli to H. A. Kramers, 27 July 1925. (Pauli Collection, Zürich. Professor Paul Forman was kind enough to provide me with a copy of this letter.)

94. A. Einstein, "Bietet die Feldtheorie Möglichkeiten für die Lösung des Quantenproblems?" *Berliner Berichte* (1923), p. 359.

the wave theory of light accounted for the complicated phenomena of optical interference and diffraction. No one who fully appreciated this "wonderful certainty" would find it hard to believe that a causal description in space and time by means of partial differential equations—a field theory—was well suited to do justice to the facts. Einstein was convinced that field theory did offer many unexplored possibilities which might allow one to put the quantum rules on a firm foundation, and that it would be unwise to abandon the goal of causal space-time description before these possibilities had all been explored. He had already begun to study overdetermined systems of equations in the hope that these would lead to laws that restricted the initial conditions in the manner of quantum conditions.

It was Einstein who was ready to devote himself to the exploration and development of the unheard-of idea that material particles should show wave properties, even as electromagnetic radiation showed corpuscular properties, when that idea was put forward by Louis de Broglie.[95] Einstein seized upon de Broglie's suggestion of matter waves, testing it, searching out its experimental consequences, and serving as its great advocate. Bohr's first comment in print on de Broglie's work came in his "Postscript" of July 1925, where he mentioned both de Broglie's thesis and Einstein's subsequent papers as examples of work that renounced the goal of space-time description.[96] One may doubt that de Broglie or Einstein viewed their work this way.

11. Werner Heisenberg once referred to the Bohr, Kramers, Slater theory as "the first serious attempt to resolve the paradoxes of radiation into rational physics."[97] That theory would certainly not have been recognized as falling under the label "rational physics" when it appeared. Even Bohr, who was striving to develop the new physics whose necessity he had been persuaded of for years, made no such claims for the work. He admitted that it had not "in any way removed the formal character of the [quantum] theory," so that even if it

95. For a detailed discussion see M. J. Klein, *op. cit.* (note 11).
96. N. Bohr, *op. cit.* (note 76), p. 157.
97. W. Heisenberg, "The Development of the Interpretation of the Quantum Theory," in *Niels Bohr and the Development of Physics,* ed. W. Pauli (London, 1955), p. 12.

were successful, it would serve only as an indication of a new line to follow.

Heisenberg's remark is also extraordinary for its suggestion that no one before Bohr, Kramers, and Slater had tried to resolve the wave-particle paradoxes into "rational physics." In April 1924 Einstein had described the current situation in an article on the Compton effect, written for a Berlin newspaper: "We now have two theories of light, both indispensable, but, it must be admitted, without any logical connection between them, despite twenty years of colossal effort by theoretical physicists."[98] Einstein's phrase "twenty years of colossal effort" was no exaggeration, though he was too modest even to hint that most of that effort was his own. He had been struggling to construct a "rational physics" that would resolve the paradoxes of radiation long before his colleagues recognized the existence of the problem, and he would go on with the struggle long after almost all of them were satisfied that the problem had been solved.

Einstein once wrote that what made Bohr "so marvelously attractive as a scientific thinker" was "his rare blend of boldness and caution."[99] (He could, of course, speak with some authority on these subjects.) Einstein's own blend of boldness and caution was complementary to Bohr's, to use the exactly appropriate term. He could never share Bohr's view that the new quantum physics constituted the long sought-for "rational generalization of classical physics," and he never stopped criticizing what he considered to be its inadequacies. Einstein had no illusions about the path he chose for himself. He knew that it subjected him to the accusation of "rigid adherence to classical theory," an accusation not always made, as he thought Bohr did make it, "in the friendliest of fashion." Einstein felt that his lonely efforts were demanded by "a coercion which I cannot evade,"[100] and he knew better than anyone else the price that they exacted. In 1951 he wrote to his old friend Michele Besso: "All the fifty years of conscious brooding have brought me no closer to the answer to the question, 'What are light quanta?'" But he added, "Of course today every rascal thinks he knows the answer, but he is

98. A. Einstein, "Das Comptonsche Experiment," *Berliner Tageblatt,* 20 April 1924, 1. Beiblatt.

99. A. Einstein, *The World as I See It,* p. 68.

100. A. Einstein, "Remarks Concerning the Essays Brought Together in This Cooperative Volume," in *Albert Einstein: Philosopher Scientist,* pp. 675–676.

deluding himself."[101] And when, just a few weeks before his death, Einstein wrote to Bohr to enlist his support for a public declaration warning the world about the hazards of an atomic arms race, he began with the remark: "Don't frown like that! This has nothing to do with our old controversy on physics, but rather concerns a matter on which we are in complete agreement."[102]

Bohr has written of the "deep and lasting impression" that his discussions with Einstein made on him. How deep and lasting they were was made clear by Abraham Pais, when he described the way in which Bohr would daily relive the struggles that went into the understanding of quantum mechanics. "This," Pais added, "I am convinced, was Bohr's inexhaustible source of identity. Einstein appeared forever as his leading spiritual sparring partner—even after the latter's death he would argue with him as if Einstein were still alive."[103]

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101. A. Einstein to M. Besso, 12 December 1951.
102. A. Einstein to N. Bohr, 2 March 1955. Reprinted in *Einstein on Peace*, ed. O. Nathan and H. Norden (New York, 1960), pp. 629–630.
103. A. Pais, "Reminiscences from the Post-war Years" in *Niels Bohr*, ed. S. Rozental (Amsterdam, 1967), p. 219.