Einstein and the Wave-Particle Duality

I. Introduction

Niels Bohr, who was in a unique position to know, always insisted on the variety and complexity that characterize the history of quantum mechanics. In his last published work[1] he described "the 'heroic era' of quantum physics" as involving "a unique cooperation of a whole generation of theoretical physicists from many countries," and "the combination of different lines of approach and the introduction of appropriate mathematical methods." Bohr's words should serve as a warning against attempts to oversimplify this history by making some particular one of the lines of approach appear to be *the* principal way in which quantum physics developed. Understandably, but nonetheless unfortunately, many of the accounts of the period, including some of the memoirs written by those who played leading parts in it, suffer from this kind of oversimplification. The picture of the development that one gets from such accounts, interesting though they may be for their personal, first-hand details, lacks just that variety and complexity peculiar to the principal achievement of twentieth century physics.

The most common form that the oversimplification takes is an almost exclusive concentration on the problems of atomic structure and atomic spectra from Bohr's work in 1913 to the new quantum mechanics of 1925–26. Such a concentration necessarily implies a neglect of other problems, problems that may have concerned a smaller number of theorists but that figured in a major way in the synthesis of the nineteen twenties. In this essay I shall try to follow another of the lines of approach to quantum mechanics. This is a line that can be clearly traced by the problems struggled with along the way and by the methods used in the struggle. The problems were not those of atomic structure but those of the dual nature of radiation and the properties of gases. The methods were not so much those of the "old quantum theory" as those of statistical mechanics. And the presiding genius and principal guide was not Bohr, but Einstein. It is the line of approach that led up to Schrödinger's wave mechanics.

As point of departure consider a remark from Erwin Schrödinger's paper, *On the Relationship of the Heisenberg-Born-Jordan Quantum Mechanics to Mine.*[2] In this paper, written in March, 1926, Schrödinger demonstrated the mathematical equivalence of these two theories, so very different in their starting points, their mathematical methods, and their general approaches. In contrasting the origins of his wave mechanics with those of the Göttingen physicists' matrix mechanics, Schrödinger wrote that he was unaware of any genetic connection between his work and Heisenberg's, which had appeared some months earlier. He knew of Heisenberg's work, but he had been "frightened off, not to say repulsed" by its formidable looking algebra and its lack of intuitive clarity. "My theory," he wrote, "was stimulated by de Broglie's thesis and by short but infinitely far-seeing remarks by Einstein."

Those "short but infinitely far-seeing remarks" that Einstein made late in 1924 form the focal point of this essay. They consisted of a forceful restatement of de Broglie's idea that waves must be associated with material particles, backed by cogent arguments based on Einstein's quantum theory of the ideal gas. Both de Broglie's idea and the work of Bose that Einstein applied in his theory of the gas can, in turn, be properly considered as arising from Einstein's revolutionary studies of the nature of radiation, carried on since the beginning of his career. Einstein had established the existence of a dual character in radiation, the wave-particle duality, and had long been emphasizing its fundamental importance for the future of physics. He was, therefore, the one physicist best fitted to see the significance of de Broglie's work and to explore its implications. At the very time that Compton's

experiments had finally convinced a good many physicists of the reality of light quanta, particles of radiation, Einstein joined de Broglie in his proposal that this same wave-particle duality must hold for matter as well as radiation. Little wonder that his influence was felt quickly and decisively, and not least by Schrödinger.

II. Einstein's Work on the Structure of Radiation

A. "It is undeniable that there is an extensive group of data concerning radiation which show that light has certain fundamental properties that can be understood much more readily from the standpoint of the Newtonian emission theory than from the standpoint of the wave theory. It is my opinion, therefore, that the next phase of the development of theoretical physics will bring us a theory of light that can be interpreted as a kind of fusion of the wave and emission theories. The purpose of the following arguments is to give a foundation for this opinion, and to show that a profound change in our views of the nature and constitution of light is indispensable."

These are the words that Einstein addressed to the scientists assembled in Salzburg in September 1909 to announce his view of the direction in which physics would inevitably be forced to move.[3] Einstein had no doubt that the granular, quantum structure of radiation, first pointed out by him four years earlier, was real and would have to be reckoned with seriously in any future theory. This view was unique to Einstein; just how unique it was can be gauged from the fact that Max Planck, the one physicist besides himself whose work was used in Einstein's arguments, was the first to rise in the discussion period and to remark, apropos of Einstein's idea of free quanta, "That seems to me to be a step that, in my opinion, is not yet called for."[4] Einstein, however, had a structure of arguments to support his view, a strong and beautiful structure that demonstrates the unity as well as the profundity of his thought. The structure was built on the ideas and methods of his three masterworks of 1905 and brought out the interconnections and common concerns behind these apparently very diverse papers.[5]

Einstein was arguing the need for a profound change in the theory of radiation, and he began his arguments with a review of the successes and the difficulties of the electromagnetic theory of light. He stressed the importance of the work that H. A. Lorentz had done in making a clear separation between aether and matter, so that the electromagnetic field was connected with ponderable matter only insofar as the charges and currents that serve as field sources are always found tied to matter. Lorentz's theory, with its assumption of an absolutely stationary aether, had been able to account for all of the previously paradoxical experimental results with one exception—the null result of the Michelson-Morley experiment. Einstein described his own "so-called relativity theory" as the outcome of insisting on two requirements: the principle of relativity (asserting the equivalence of reference systems in uniform relative motion and, by implication, the nonexistence of the aether as an absolute frame of reference), and the constancy of the velocity of light. It is especially interesting to notice that he considered the latter postulate as preserving the essential feature of Lorentz's aether theory, in which the velocity of light was, of course, independent of the velocity of its source.

The only aspect of the theory of relativity itself that Einstein found relevant to his discussion was one of its consequences that suggested a departure from previous views on the structure of radiation. This was the equivalence of mass and energy. By means of a simple relativistic calculation he showed that the inertial mass of a body that emits energy E in the form of radiation is reduced by the amount E/c^2, where c is the velocity of light. The relativity theory therefore shared two features with an emission or corpuscular theory of light, in contrast with the wave theory: light was to be considered as an independently existing entity, and not as something depending on the state of a hypothetical medium; and the emission of light by one body followed by its absorption by another involved a transfer of inertial mass. To this extent, but only to this extent, did the theory of relativity modify one's views of the structure of radiation. The principal support for Einstein's

opinion that basic change in those views was necessary came from other quarters, and he went on to marshal that support.

He pointed to a variety of properties of light, well known and easily stated, that just could not be explained on the basis of the wave theory. Why, for example, did the occurrence or nonoccurrence of a particular photochemical reaction depend on the color of the incident light, and not at all on its intensity, which ought to determine the available energy? Why is light of short wavelengths more effective in producing chemical reactions than light of longer wavelengths? How does a single photoelectron acquire so much energy from a light source whose energy is distributed at a very low density, and why is the energy of such a photoelectron independent of the light intensity? Einstein conjectured that all of these difficulties had a common origin in one essential feature of the wave theory of light: the basic emission process did not have a simple inverse. If one considered the emission of light as consisting of the production of an expanding spherical wave by an oscillating electric charge, then the inverse process, the absorption of a contracting spherical wave by a charge, while allowed by Maxwell's equations, was surely not an elementary process. This formal defect of the wave theory—the absence of a symmetry that was present in, for example, the kinetic theory of gases—Einstein took to be a basic flaw.

In this respect a corpuscular theory of light would be superior, since it was not subject to the same kind of criticism. If the emission of light could be viewed as the ejection of a "particle of light," then absorption would consist of exactly the inverse process. Since the energy emitted would not be dissipated over an infinite volume, all of it would be available at absorption. The basic features of all processes, such as the photoelectric effect, that seemed to suggest directed rather than spherically symmetrical emission of light would then become intelligible. (It was characteristic of Einstein's approach to point to a formal asymmetry in the underlying theory as the root of physical problems. He had already done just that in his 1905 papers on quanta and relativity. Reasoning based on such general, formal grounds has become a dominant feature of theoretical physics, but it was rare before Einstein, and it has not often been used so effectively since.)

Einstein obtained the strongest support for his opinions from what he found in Planck's theory of black-body radiation, whose essentials he had to repeat for his audience because, even in 1909, it was a theory that he felt he "probably should not assume to be generally familiar." He pointed out that the key step in Planck's reasoning had been his evaluation of the entropy of a collection of oscillators of frequency ν by counting the number of ways W in which they could share a fixed amount of energy.[6] To do this counting Planck had considered the total energy as divided into "particles," each of energy ϵ; and to make his result consistent with Wien's displacement law, itself a consequence of the second law of thermodynamics, he had set ϵ proportional to the frequency ν,

$$\epsilon = h\nu, \tag{1}$$

with h a new natural constant. These arguments had led Planck to an expression for $\rho(\nu,T)$, the energy of thermal radiation of frequency ν at temperature T, per unit volume and per unit frequency interval,

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

which was in agreement with all available data. (The constant k in this equation is Boltzmann's constant.)

The first point that Einstein made concerning Planck's work was its essential departure from classical ideas. This departure was most clearly in evidence wherever the average energy of an oscillator of a particular frequency was small compared to the energy of one quantum of that frequency, a situation that would often occur. This deviation from ordinary statistical ideas was not a reason for rejecting Planck's work: indeed, one of Planck's deductions from his theory, a value for the fundamental unit of electric charge, had just been strikingly confirmed by the experiments of Rutherford and Geiger. Quite the contrary, Einstein had already seen the powerful way in which a suitable generalization of Planck's ideas could handle just the questions he had posed earlier in his lecture. In 1905, as the result of an independent line of argument, Einstein had proposed that radiation consists of

energy quanta of magnitude $h\nu$, and he considered Planck's successful radiation theory as supporting his views, as, in fact, implicitly based on his own hypothesis of light quanta.[7] Einstein did not trouble to repeat the list of phenomena he had already explained on the basis of the quantum theory, phenomena as diverse as Stokes's law for fluorescence and the behavior of the specific heats of solids at low temperatures. He had newer arguments that he considered even more convincing.

He introduced these new arguments by raising a question, a doubt about Planck's quantum theory, that had already occurred to some and that would provide the theme for much of Planck's work during the next few years as he tried to heal his break with classical theory. "Would it not be conceivable that Planck's radiation formula was indeed correct, but that it could be derived by some method that was not based on such an apparently monstrous assumption as Planck had used? Would it not be possible to replace the hypothesis of light quanta by some other hypothesis by means of which one could do equal justice to the familiar phenomena? If it is necessary to modify the principles of the theory could one not at least retain the equations for the propagation of radiation and interpret only the elementary events of emission and absorption in a way different from that used previously?"

Einstein's answer to all these questions was negative, and his reasons for so answering lay in the structure of Planck's radiation law itself. The key to his reasoning was his reversal of Planck's procedure. Instead of trying to derive the distribution law from some more fundamental starting point, he turned the argument around. Planck's law had the solid backing of experiment; why not assume its correctness and see what conclusions it implied as to the structure of radiation? Einstein had already done just this sort of thing in 1905 when he based his argument for the granular structure of radiation on the consequences of Wien's radiation law, the predecessor and high frequency limit of Planck's law. This time he applied the method to Planck's law itself with equally impressive results.

B. There were two independent arguments that Einstein referred to in his report, leading to closely related results by distinctly different methods. He gave neither argument in detail, since he had already sketched both in a paper that had appeared earlier that year, and covered some of the same ground.[8] The first was closely related to both his 1905 paper on quanta and his earlier work on the foundations of statistical mechanics,[9] and consisted of a calculation of the energy fluctuations in black-body radiation. Einstein's calculation went directly back to the second law of thermodynamics and its statistical interpretation. Its major steps are readily indicated.

Let us consider a system composed of two parts, coupled so that they can exchange energy freely, subject only to the condition that the total energy is fixed. If the two parts have fixed volumes, then the equilibrium condition of the second law is the requirement that the entropy be a maximum with respect to the energy exchange. If we denote the entropies of the two parts by S_1 and S_2, with $S^0{}_1$ and $S^0{}_2$ the corresponding equilibrium values, and the energies by E_1 and E_2, et cetera, then we can expand the total entropy in terms of the departure from equilibrium:

$$S_1 + S_2 = S_1^0 + S_2^0 + \left\{\left(\frac{\partial S_1}{\partial E_1}\right)_0 - \left(\frac{\partial S_2}{\partial E_2}\right)_0\right\}\epsilon + \frac{1}{2}\left\{\left(\frac{\partial^2 S_1}{\partial E_1{}^2}\right)_0 + \left(\frac{\partial^2 S_2}{\partial E_2{}^2}\right)_0\right\}\epsilon^2 + \cdots \quad (3)$$

We have used the fact that ϵ, defined as $E_1 - E_1^0$, is also equal to the negative of $E_2 - E_2^0$, because the system is closed. Since the entropy is maximum at equilibrium, the coefficient of ϵ must vanish, expressing the equality of the temperatures. If we make the additional assumption that the second volume is large compared to the first, then the deviation of the entropy from its equilibrium value, to second order in ϵ, is simply

$$\Delta S = (S_1 + S_2) - (S_1^0 + S_2^0) = \frac{1}{2}\left(\frac{\partial^2 S_1}{\partial E_1{}^2}\right)_0 \epsilon^2 = -\frac{1}{2C_vT^2}\epsilon^2, \quad (4)$$

where C_v is the heat capacity at constant volume, $(\partial E/\partial T)_v$, and the subscripts have been dropped as no longer necessary.

Einstein combined this result with his own reinterpretation of Boltzmann's principle (another of his eminently successful "reversals" of procedure), to obtain the probability that such an energy fluctuation ϵ would occur. If $W(\epsilon)d\epsilon$ is the probability of a fluctuation between ϵ and $\epsilon + d\epsilon$ in magnitude, then the Boltzmann-Einstein equation for $W(\epsilon)$ reads,

$$\begin{aligned} W(\epsilon)d\epsilon &= \alpha \exp\{(\Delta S)(N_0/R)\}d\epsilon \\ &= \alpha \exp\{-(N_0\epsilon^2/2RC_vT^2)\}d\epsilon, \end{aligned} \quad (5)$$

where α is simply a normalization constant, R is the gas constant, and N_0 is Avogadro's number. The mean square energy fluctuation $\langle\epsilon^2\rangle$ could now be calculated directly from its defining equation,

$$\langle\epsilon^2\rangle = \int_{-\infty}^{\infty} \epsilon^2 W(\epsilon)d\epsilon \Big/ \int_{-\infty}^{\infty} W(\epsilon)d\epsilon, \quad (6)$$

leading to the result

$$\langle\epsilon^2\rangle = (R/N_0)T^2C_v. \quad (7)$$

This derivation was a simplification of the one Einstein had already given in 1904.[9] I have kept his notation for the constant (R/N_0) for a reason that should become evident shortly.

One could now calculate the energy fluctuations for that part of black-body radiation having frequencies in the interval from ν to $\nu + d\nu$ as soon as one specified the function $\rho(\nu,T)$, the distribution law. For, the average energy E of this radiation in a container of volume V would be given by the equation,

$$E = V\rho(\nu,T)d\nu, \quad (8)$$

and the C_v of Equation (7) can be obtained by differentiating with respect to the temperature.

When Planck's radiation law, Equation (2), is used for ρ, the resulting equation for $\langle\epsilon^2\rangle$ can easily be put in the forms,

$$\langle\epsilon^2\rangle = \{h\nu E + (c^3/8\pi\nu^2 V d\nu)E^2\}, \quad (9a)$$

$$\langle\epsilon^2\rangle = (V d\nu)\{h\nu\rho + (c^3/8\pi\nu^2)\rho^2\}, \quad (9b)$$

if one identifies (R/N_0) with the constant k in Planck's law. Equating (R/N_0) to k amounted to accepting Planck's determination of Avogadro's number, and Einstein had already seen the justification as well as the importance of this step.

The existence of such energy fluctuations was perfectly intelligible from the viewpoint of the wave theory of radiation: they were to be expected as the result of the interference of the various wave trains, localizing more or less of the energy in a given region at any time. The form of the fluctuation equation, however, could not be understood if this were the only mechanism producing the fluctuations. As Einstein argued on dimensional grounds, and as Lorentz confirmed by a detailed and rather lengthy calculation several years later,[10] the wave theory could lead only to the second term in the brackets of Equations (9a) and (9b), that is, to a term proportional to E^2.

If, on the other hand, radiation were composed of classical particles, moving independently of one another, and if these particles were each of energy $h\nu$, so that there would be $E/h\nu$ such particles in V on the average, then the usual statistical argument (the law of large numbers) would lead to just the first term in brackets. [Since the mean square fluctuation in the number of particles would be equal to the average number $E/h\nu$, the corresponding mean square energy fluctuation would be obtained by multiplying by $(h\nu)^2$.]

It appeared, as Einstein remarked, as though there were two independent causes producing the fluctuations, so that the fluctuations from the two sources were simply additive. Either of the two could dominate depending on the frequency range and the temperature one considered. In the high frequency, low temperature region, where Planck's law becomes Wien's law, only the first term is obtained; in the low frequency, high temperature region, when the Planck distribution becomes the Rayleigh-Jeans law, only the second term is present.[11]

Before discussing the implications of this result any further, I shall turn to Einstein's second argument, an analysis of the momentum fluctuations in black-body radiation. He began by showing the necessity for such fluctuations on essentially thermody-

namic grounds. Consider a flat plate, a perfect reflector on both faces, which is free to move in the direction perpendicular to its own plane. Suppose that this plate is in a vessel which contains an ideal gas at low pressure and black-body radiation at the temperature T fixed by the walls of the vessel. The collisions of the gas molecules with the plate will set the latter into an irregular motion, a particular sort of Brownian motion. When the plate moves, however, the forces arising from radiation pressure on its front and back surfaces are no longer equal, as they would be if the plate were at rest. There will be a net force, due to the unbalanced radiation pressures, opposing the motion — a "radiation friction" that increases as the velocity of the plate increases. As a result of this effective frictional force, the kinetic energy of the plate will be converted into energy of the radiation field. Were this radiation friction the only effect of the radiation on the plate, the energy of the gas would eventually all be transformed into energy of the radiation: there could be no equilibrium between gas and radiation.

The feature omitted in this discussion is the irregular fluctuations of the radiation pressure. These fluctuations must not only exist, but they must also make up, on the average, for the frictional loss of energy. They must, in other words, give the plate an average kinetic energy of $kT/2$. In order to carry out the analysis, let v be the velocity of the plate at time t, and let P be the resistive force per unit velocity due to the radiation pressure. (Einstein neglected any terms in this force depending on higher powers of the velocity.) The decrease in the plate's velocity, in a time interval τ, caused by this frictional force is then $Pv\tau/m$, where m is the mass of the plate. If Δ is the increase in momentum of the plate during τ due to the irregular fluctuations of the radiation pressure, then the momentum at time $t + \tau$ will be $(mv + \Delta - Pv\tau)$. Imposing the condition that, on the average, mv does not change during τ we obtain the equation,

$$\overline{(mv + \Delta - Pv\tau)^2} = \overline{(mv)^2}. \qquad (10)$$

If we note that $\overline{v\Delta}$ vanishes because of the irregular nature of the fluctuations, and that the term $(Pv\tau)^2$ can be neglected, we can reduce this equation to the form

$$\overline{\Delta^2} = (2P\tau m)\overline{v^2}. \qquad (11)$$

The average value of the kinetic energy of the plate must be $kT/2$ from the equipartition theorem of statistical mechanics, so that $\overline{v^2}$ is fixed by the equation,

$$m\overline{v^2}/2 = kT/2. \qquad (12)$$

The one remaining task is the evaluation of P in terms of the radiation distribution function $\rho(\nu,T)$. For simplicity, and especially for clarity, Einstein assumed here that the plate serves as a mirror only for radiation whose frequency falls in the interval from ν to $\nu + d\nu$, and that it freely transmits radiation of all other frequencies. A relatively long calculation, which Einstein did not publish until the following year, then leads to the result,

$$P = (3/2c)\{\rho - (\nu/3)\partial\rho/\partial\nu\}Ad\nu, \qquad (13)$$

where A is the area of the plate, c is the velocity of light, and ρ is evaluated at the frequency for which the plate acts as a mirror. If we combine the last three equations, we obtain the general relationship,

$$\overline{\Delta^2}/\tau = (kT)(3/c)\{\rho - (\nu/3)\partial\rho/\partial\nu\}Ad\nu. \qquad (14)$$

The Planck radiation law, Equation (2), can now be substituted for ρ, and we obtain the final result,

$$\overline{\Delta^2}/\tau = (1/c)\{h\nu\rho + (c^3/8\pi\nu^2)\rho^2\}Ad\nu. \qquad (15)$$

The simplicity of the result is impressive, but even more striking is the identity of the bracketed term with the correspondingly bracketed term in Equation (9b) for the energy fluctuations. Just as before, the two terms in the momentum fluctuation equation can be identified as arising individually from the Wien and Rayleigh-Jeans limits of the Planck radiation law. The momentum fluctuation equation also suggests the existence of two independent mechanisms producing the fluctuations, the first (Wien term) being intelligible if the radiation were particulate in structure, the second (Rayleigh-Jeans term) arising from a wave interference mechanism.

There was, of course, no theory that could account for these wave-particle features of radiation. "The principal difficulty," Einstein wrote, "lies in the fact that the fluctuation properties of radiation, as expressed in the equations above, present small foothold for setting up a theory. Just suppose that the phenomena of diffraction and interference were still unknown, but that one knew that the average value of the irregular fluctuations of the radiation pressure were given by the second term of the above equation, where ν is a parameter of unknown significance that determines the color. Who would have enough imagination to construct the wave theory of light on this foundation?"

C. A year later, in 1910, Einstein published the detailed calculation of the momentum fluctuations in a paper written jointly with L. Hopf.[12] The emphasis and direction of this paper are, however, somewhat different from those of the 1909 papers. This time, instead of pointing to the need for a new approach to the theory of radiation, Einstein was disposing of another possible objection to the inevitability of the failure of the classical theory. It had already been shown in a variety of ways that any consistent argument based on the electromagnetic wave theory of light, together with statistical mechanics, necessarily led to the unacceptable Rayleigh-Jeans distribution for black-body radiation. Might not the source of this difficulty lie in the application of the statistical mechanics, and particularly the equipartition theorem, to the radiation itself, or to the high frequency motion of the oscillators interacting with the radiation? Einstein and Hopf showed that this was not the case, that one could show the inevitability of the Rayleigh-Jeans law by an argument in which the equipartition theorem was applied only to the translational motion of gas molecules, an incontestably valid use of this long-disputed theorem.

The argument involved the fluctuations of momentum produced in an oscillator by its interactions with the chaotic radiation field of black-body radiation. After deriving the equivalent of Equations (11) and (12), Einstein and Hopf gave the complete derivation of the key equation (13) which relates the radiation friction to the distribution law $\rho(\nu,T)$. Instead of using an assumed $\rho(\nu,T)$ (Planck's law, for example), to find the momentum fluctuations $\overline{\Delta^2}$, as Einstein had done before, they calculated the latter quantity directly from electromagnetic theory, expressing it too in terms of $\rho(\nu,T)$. As a result, Equation (11) became a differential equation for $\rho(\nu,T)$, whose only solution was the Rayleigh-Jeans law. In other words, the momentum fluctuations calculated from electromagnetic theory demanded the Rayleigh-Jeans law for radiation: electromagnetic theory offered no clue to the additional fluctuations that characterized the Planck law.

The calculation itself is of interest for two reasons. It fuses the ideas and the methods of Einstein's Brownian motion theory and the special theory of relativity in handling a problem intimately related to the third of his 1905 papers—the quantum structure of radiation.[13] The second point is a more technical one. In their calculation Einstein and Hopf used a result from the theory of probability which they proved in an accompanying paper.[14] This theorem, on the statistical independence of the Fourier coefficients of the electromagnetic field, was questioned by von Laue several years later, and a protracted discussion of the details of the averaging process ensued, in which Planck also took part.[15] So far as I can tell, this discussion did not affect the development of the theory in any noticeable way.

D. When Einstein returned to the radiation problem in 1916, after having concentrated almost completely on gravitation for some years with historic results, the quantum theory had taken a new turn. Bohr had opened an extremely fruitful domain of application for the quantum in his theory of the hydrogen atom and its spectrum. In addition, Bohr's work and its generalizations by Sommerfeld and others signified a fresh approach to the foundations of the quantum theory itself. Although Einstein made only passing reference to this work, it had noticeably influenced his own ideas at several key points.

The paper[16] that he published is well known and still widely quoted today for the new derivation of the Planck distribution law that it reported, but, as is usually the case with Einstein's writings, its content is not adequately described by a single idea. Einstein not only rederived Planck's law: he used his new derivation to throw light on "the still so obscure process" of emission and absorption of radiation. He particularly stressed the implications of his analysis for the directed nature of the emission process and for the momentum transfer it produced.

Einstein's starting point is especially interesting for its connection with Wien's work twenty years earlier.[17] He commented on the formal similarity between the frequency distribution of black-body radiation, and Maxwell's distribution law for the speeds of gas molecules. This similarity was too striking to have gone unnoticed, and it had, in fact, been the basis for Wien's proposed equation for the spectral distribution of radiation, still valid as the high frequency limit of Planck's law. Wien's derivation of his distribution law lacked cogency, but Einstein had pursued the idea behind it, and had found a new derivation, resting on the basic ideas of the quantum theory, that demonstrated the non-accidental nature of that formal similarity. In this paper, as in almost all of Einstein's work on the quantum theory, the second law of thermodynamics and its statistical interpretation were at the center of his reasoning.

He considered a gas in equilibrium with black-body radiation, as he had done many times before. The internal states of the gas molecules were assumed to be a discrete set $1, 2, \ldots n, \ldots$ whose energies are $\epsilon_1, \epsilon_2, \ldots \epsilon_n \ldots$. How these states were to be determined was not relevant for Einstein's purposes. All he needed was the assumption of their existence, and the statement that, if the gas is in thermal equilibrium at temperature T, then the relative probability W_n of finding a molecule in state n is given by the equation,

$$W_n = \phi_n \exp(-\epsilon_n/kT). \qquad (16)$$

The coefficients ϕ_n are characteristic of the particular kind of molecule considered, and are the statistical weights of the various quantum states.

Let m and n be a pair of states, $\epsilon_m > \epsilon_n$, such that the molecule can go from state m to state n by emitting radiation, and from state n to state m by absorbing radiation. Einstein assumed that the emission process could occur spontaneously, without any external agency, according to a law exactly analogous to that of radioactive decay,

$$dW_1 = A_{nm}dt, \quad (17)$$

where dW_1 is the probability that the radiation process occurs in time dt, and A_{nm} is a constant characterizing this process. A classical oscillator will absorb or emit energy as it interacts with external radiation tuned to its own frequency, depending on the phase of its motion relative to that of the radiation. Einstein made an analogous assumption for the quantum mechanical behavior: when acted on by external radiation of spectral density ρ and the proper frequency ν, the probability of a molecular transition from state n to state m during dt with absorption of radiation is given by the equation,

$$dW_2 = \rho B_{mn}dt, \quad (18)$$

and the probability for the reverse transition with the emission of radiation is given by the equation,

$$dW_3 = \rho B_{nm}dt. \quad (19)$$

If the Boltzmann distribution of Equation (16) is to remain unaffected by the emission and absorption processes, then the following condition, which expresses the equality of the overall emission and absorption rates, must hold,

$$\rho B_{mn}\phi_n \exp\left(-\epsilon_n/kT\right) = (\rho B_{nm} + A_{nm})\phi_m \exp\left(-\epsilon_m/kT\right). \quad (20)$$

By examining the limiting form of this equation at very high temperatures, where the A_{nm} is relatively unimportant since ρ becomes

large, and where the Boltzmann factors become unity, one obtains the condition

$$\phi_n B_{mn} = \phi_m B_{nm}, \quad (21)$$

relating the rates of induced emission and absorption. If Equation (20) is now solved for ρ, one obtains an equation whose temperature dependence is that of Planck's law, Equation (2):

$$\rho = (A_{nm}/B_{nm})\{\exp\left[(\epsilon_m - \epsilon_n)/kT\right] - 1\}^{-1}. \quad (22)$$

The frequency dependence of ρ can be fixed from Wien's displacement law, itself a consequence of the second law of thermodynamics, which requires that $\rho(\nu,T)$ have the form $\nu^3 f(\nu/T)$. Hence the following two equations must hold:

$$A_{nm}/B_{nm} = \zeta\nu^3, \quad (23a)$$

$$\epsilon_m - \epsilon_n = h\nu, \quad (23b)$$

where ζ and h are universal constants. The latter equation expresses one of the postulates of Bohr's theory of spectra, well established by the time Einstein wrote. The combination of Equations (22) and (23) is, of course, Planck's law, except that the constant ζ is not evaluated by this procedure.

This much of Einstein's argument is widely known, and it is striking enough for its "amazingly simple and general method" of obtaining the radiation law and for its use of probabilities. It is not, however, the result that he emphasized most heavily in his paper. Combining the reasoning just given with the methods of Brownian motion theory that he had used so effectively before, Einstein went on to analyze the motion of the molecules that would have to result from the absorption and emission of radiation. The focus of the argument was again the combination of Equations (11) and (12):

$$\overline{\Delta^2}/\tau = 2PkT, \quad (24)$$

expressing the condition that thermal equilibrium between molecules and radiation is preserved. Einstein calculated the two quantities P and Δ^2, the radiation friction constant and the fluctuations of a molecule's momentum, just as he had done in 1910. This time, though, the calculations used his assumptions on the quantum nature of emission and absorption and not the classical electromagnetic theory. The resulting equation for ρ was now satisfied by the Planck distribution law and not the Rayleigh-Jeans law.

In order that Equation (24) be satisfied under the assumptions of the quantum theory, one had to make a definite assumption about the directed nature of the processes of absorption and emission. Whenever a molecule absorbs or emits a quantum $h\nu$ under the stimulation of external radiation from a definite direction, there must be a change in the momentum of the molecule of magnitude $h\nu/c$ in the direction of the incident radiation, positive or negative according to whether the process is absorption or emission. In addition, and more surprisingly, if a molecule spontaneously radiates a quantum $h\nu$ this process too must be a directed one, leading to a momentum change $h\nu/c$. "There is no radiation of spherical waves. In the spontaneous emission process the molecule suffers a recoil of magnitude $h\nu/c$ in a direction that, in the present state of the theory, is determined only by 'chance'." As a consequence "the establishment of a truly quantum mechanical theory of radiation seems to be almost inevitable." Einstein's next remark is of interest, particularly in the light of his later views on the role of probability in physics: "The weakness of the theory lies, on the one hand, in the fact that it does not bring us any closer to a connection with the wave theory, and, on the other hand, in the fact that it leaves the time and direction of the elementary processes to 'chance'; nevertheless I have full confidence in the reliability of the course taken."

Einstein concluded his paper by emphasizing again the necessity for considering momentum exchanges as well as energy exchanges in any theory of the interaction between matter and radiation. Because of the intimate relationship between momentum and energy, no theory could be judged adequate unless it properly ac-

counted for the motion imparted to the molecules by momentum from the radiation, "as demanded by the theory of heat."

III. The Compton Effect and Some Consequences

A. It is notorious that Einstein's keenest, most physical insights were usually reached without the backing of experimental evidence, though that evidence eventually appeared. Millikan's measurements on the photoelectric effect confirmed the results of the hypothesis of free quanta of radiation a full decade after Einstein had proposed it.[18] Direct experimental support for the idea that radiation is directed and that each quantum carries momentum $h\nu/c$ took just as long to develop, but it began to appear in 1922. In October of that year Arthur Compton[19] announced his results on the anomalous scattering of x-rays and suggested that the wave length of the x-rays was increased in this anomalous scattering process. Several months later Compton[20] and Peter Debye[21] independently published the explanation of this wave length increase, now universally known as the Compton effect. They both assumed that the scattering was an elementary process that could be looked upon as a collision between an incident x-ray quantum and a free electron at rest. If one applies the laws of energy and momentum conservation to this collision one can calculate the wave length of the scattered quantum, the kinetic energy of recoil of the struck electron, and the angle at which it recoils, all in terms of the wave length of the incident quantum and the angle at which it is scattered. The details of the calculation appear in every textbook on atomic physics and need not be repeated here.

Compton made no mention of Einstein's ideas on directed radiation and the momentum of light quanta in any of his papers on the Compton effect. One can presume that Compton, not a theorist himself, was aware of these ideas from discussions at the time, that they were somewhat "in the air" then. In a talk[22] given in 1961, just a year before his death, Compton described the history of his experiments. At the very time he was extending and developing his results he found himself in "the most lively scientific controversy" that he had ever known, with William Duane of Harvard, who doubted the existence of the Compton effect. It took something like a year before Duane agreed that Compton's discovery was a real one. Compton's description justifies his remark that "these experiments were the first to give, at least to physicists in the United States, a conviction of the fundamental validity of the quantum theory."

Debye's paper, published in April 1923, had a very different tone. Debye had been working on the quantum theory for a dozen years or more, and was thoroughly aware of the source of the ideas he brought to bear on the Compton effect. He explicitly stated that the radiation had to be treated as "needle radiation" in the sense of Einstein's directed quanta. Debye emphasized that the momentum properties of quanta had not appeared in the photoelectric effect because the binding energy of the photoelectrons was not negligible compared to the lower energy of the incident ultraviolet quanta, so that appreciable momentum was also taken up by the atom itself. Debye closed his paper by remarking that, since the equations for the Compton effect depended only on the hypothesis of quanta and of "needle radiation" together with the conservation laws, one could hope for a deeper insight into the relationship of the quantum theory and wave optics from further study of this effect.

B. With the evidence provided by the Compton effect in front of them, theoretical physicists had to reckon more seriously with Einstein's ideas on quanta than ever before. A few months after the appearance of Compton's and Debye's papers, Wolfgang Pauli[23] took up a previously unsolved problem, closely related to most of the ideas I have already discussed. It was a problem originally raised by Lorentz in his address to the first Solvay Congress in 1911,[24] the problem of thermal equilibrium between radiation and free electrons. Einstein's papers of 1909 and 1910 had discussed the equilibrium between radiation and harmonic oscillators; could one apply the same methods if the oscillator were replaced by something even simpler—a free charged particle? One ought to be able to get even more reliable information about the structure of radiation by analyzing this elementary case. Lorentz used the equivalent of Equation (11) to express the average kinetic energy of the electron in terms of the radiation friction constant P and the mean square momentum fluctuations $\overline{\Delta^2}$. Since he could calculate the latter quantities from electromagnetic theory, he expected to confirm the equipartition principle for this case, that is, he expected to find $(3/2)kT$ as the value of $\overline{mv^2}/2$ for an electron free to move in all three dimensions. In fact he found a smaller value, and a further analysis, carried out by his student A. D. Fokker,[25] showed that the mechanisms of classical radiation theory were inadequate to preserve thermal equilibrium between the free electron and black-body radiation described by the Planck law. If one artificially assumed that the Planck law did hold, then the average kinetic energy of the electron turned out to be much smaller than its equipartition value. This result was perplexing because there was no apparent need to apply quantum ideas to the electron itself, in contrast to the case of the harmonic oscillator.

When Pauli addressed himself to this problem in 1923, he had Einstein's 1917 paper to guide him in his search for a description, within the quantum theory, of an interaction between electron and radiation that could establish thermal equilibrium. The mechanism would have to allow the radiation to satisfy Planck's law while the electrons' kinetic energies were described by the Maxwell-Boltzmann distribution. The elementary interaction would be just that involved in the Compton effect, and Pauli's task was to find the restrictions on the probabilities of scattering in various directions required by his assumptions. The basic quantity to be fixed was the probability dW that in a time interval dt a quantum of frequency between ν and $\nu + d\nu$, directed within a solid angle $d\Omega$, be scattered so that its frequency is changed into the interval ν' to $\nu' + d\nu'$ and its direction into $d\Omega'$, while the electron goes from an initial momentum range between p and $p + dp$ to a final range p' to $p' + dp'$, where the conservation laws relate these various quantities.

Pauli found that if he took dW to be of the form $(A\rho + B\rho\rho')dt$, where ρ and ρ' are the spectral densities of the radiation at frequencies ν and ν', respectively, then the equilibrium situation in which the electrons satisfy the Maxwell-Boltzmann distribution law requires that ρ be given by the Planck formula. The result seemed paradoxical: the presence of the second term in dW meant that the probability of the Compton scattering process is enhanced by the presence of radiation whose frequency is equal to that of the quantum *after* scattering. There could, however, be no doubt of the necessity for this second term. Pauli showed that, in its absence, the radiation would have to obey the Wien rather than the Planck law at equilibrium. The term proportional to $\rho\rho'$ insured that the radiation would have those properties that arose from interference of waves in the classical theory.

The paradoxical appearance of Pauli's result decreased when, a few months later, Einstein and Paul Ehrenfest[26] showed that it could be obtained by arguments closely parallel to those in Einstein's 1917 paper. The key point lay in recognizing that a Compton process amounted to the disappearance (absorption) of a quantum of frequency ν and the appearance (emission) of a quantum of frequency ν', both appropriately specified as to direction. By the arguments leading to Equations (17)–(19), the joint probability of such a process ought to be of the form $(b\rho)(a' + b'\rho')$, where the second factor contains both spontaneous (a') and induced ($b'\rho'$) terms. When this point was developed, taking proper care to specify inverse processes correctly, Pauli's discussion was seen to be a natural and straightforward generalization of Einstein's earlier work.

C. Despite the successes of the hypothesis of light quanta, particularly in understanding the Compton effect, the basic mystery remained: how could such a picture of the structure of radiation account for the phenomena of interference and diffraction? Einstein had long since announced his opinion that both particle and wave aspects of radiation would have to be fused in a fundamentally new theory, but the way to that new theory was not yet clear. The Compton effect forced a wider acceptance of Einstein's opinion: Sommerfeld, for example, wrote Compton that his discovery "sounds the death knell of the wave theory of radiation."[27]

Drastic steps were called for, and one of the most drastic was proposed early in 1924 by Bohr, Kramers, and Slater.[28] They rejected Einstein's quantum structure of radiation, despite its "great heuristic value," and offered instead a more thoroughly probabilistic approach to the whole problem. The most striking feature of the Bohr, Kramers, Slater paper was their suggestion that the laws of conservation and energy and momentum were not strictly satisfied in processes involving strong interactions with radiation, including the Compton effect.

Heisenberg[29] recently described the Bohr, Kramers, Slater paper as "the first serious attempt to resolve the paradoxes of radiation into rational physics," but it was not a wholly successful attempt. The details of their theory, involving the introduction of a "virtual radiation field," would lead us too far from the main theme of this essay. I shall only remark that the argument is characteristic of what de Broglie[30] has called Bohr's "predilection for 'obscure clarity'," and it suggests why de Broglie referred to him as "the Rembrandt of contemporary physics."

Einstein reacted to the Bohr, Kramers, Slater paper in a letter to Paul Ehrenfest[31] dated May 1, 1924. He reported to Ehrenfest that he had just reviewed the paper for the Colloquium in Berlin, and described it as follows:

"This idea is an old acquaintance of mine, but I don't consider it to be the real thing. Principal reasons:

(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. . . ."

Two months later Einstein referred to this matter again, this time writing to Ehrenfest[32] that the Copenhagen group had "abolished free quanta," but that free quanta "would not allow themselves to be dispensed with."

The Bohr, Kramers, Slater theory failed in a direct experimental test by Bothe and Geiger,[33] who showed that the scattered quanta and the recoil electrons in the Compton effect were essentially always observed in coincidence, a result that would have been extremely improbable if this theory were valid. Further experiments by Compton, with A. W. Simon,[34] verified the angular relationships that followed only if the conservation laws were strictly obeyed. Whatever else might have to be given up in the construction of a theory that incorporated the wave-particle duality for radiation, it was not the conservation laws.

IV. Bose Statistics and de Broglie Waves

A. In July, 1924 the editor of the *Zeitschrift für Physik* received a rather unusual communication. It was a short paper, entitled *Planck's Law and the Hypothesis of Light Quanta,* written by an Indian physicist, S. N. Bose[35] of Dacca University, but the paper was sent in by Albert Einstein. Bose had forwarded his manuscript, in English, to Einstein, who thought sufficiently highly of it to translate it into German himself, and to send it to the journal accompanied by these remarks, published with his translation: "In my opinion Bose's derivation of the Planck formula signifies an important advance. The method used also yields the quantum theory of the ideal gas, as I will work out in detail elsewhere." A week after sending off Bose's paper Einstein read his own paper,[36] applying Bose's method, before the Prussian Academy, referring again to Bose's work as "extremely noteworthy."

There had already been a variety of derivations of the Planck radiation law. Why should Bose's derivation have impressed Einstein as deeply? Bose gave the answer himself at the start of his paper. All previous derivations of the Planck law appealed, at one point or another, to some result of classical electromagnetic theory. This was generally done to obtain the first factor in Equation (2), the factor $(8\pi\nu^2/c^3)$, which could be interpreted as the number of normal modes of the radiation per unit volume and per unit frequency interval, sometimes referred to as the number of degrees of freedom of the aether. This factor was obtained in different ways by Planck,[37] and by Debye[38] (who followed the method used by Rayleigh[39]), but there had never been a derivation that avoided any reference to classical electrodynamics. Bose proposed to do just that, to derive the Planck law directly from Einstein's hypothesis of light quanta using only the methods of statistical mechanics. Such a derivation amounted to a natural development of the ideas that Einstein had been advocating for close to twenty years—no wonder that he considered it "an important advance."

Bose considered the quanta as particles, and specified the location of a quantum in phase space by its coordinates x, y, z and its momentum components p_x, p_y, p_z. The energy of the quantum is related to its momentum by the relativistic equation,

$$(h\nu)^2 = c^2(p_x^2 + p_y^2 + p_z^2). \qquad (25)$$

The volume of phase space available to quanta whose energy lies between $h\nu$ and $h(\nu + d\nu)$ is then given by the equation,

$$\int dx\, dy\, dz\, dp_x\, dp_y\, dp_z = 4\pi(h\nu/c)^2(hd\nu/c)\, V, \qquad (26)$$

where V is the actual volume of the enclosure containing the radiation; the momentum space contribution is just the spherical shell of radius $h\nu/c$ and thickness $hd\nu/c$. Bose treated the phase volume as divided into cells, each cell having a measure h^3. (In this he followed an idea first stated by Planck in his lectures on radiation theory[40] in 1906, and widely used by many physicists since that time.) The number of cells corresponding to the frequency interval ν to $\nu + d\nu$ is then just the ratio of the expression in Equation (26) to h^3, or $(4\pi V/c^3)\nu^2 d\nu$. This number has to be doubled to allow for the fact that each quantum can have two independent polarizations, so that one actually has $(8\pi V/c^3)\nu^2 d\nu$ phase cells available to quanta in the given frequency interval. This is, of course, just the factor discussed a little earlier.

The problem was now to determine the number of ways in which the quanta could be distributed over the phase cells. The situation appeared to be only slightly different from that which had been discussed in statistical mechanics by every writer since Boltzmann. Consider those quanta having frequencies in the particular interval $d\nu_s$. Let there be N_s of them. The number of phase cells available to them is $(8\pi V/c^3)\nu_s^2 d\nu_s$, to be denoted as A_s for brevity. The question Bose had to answer was this: in how many ways can the N_s quanta be distributed over the A_s cells? The question was basically old; the answer was essentially new.

Instead of following Boltzmann and his successors by looking at the number of quanta in each of the A_s cells, Bose specified the distribution of quanta by the set of numbers $p_0^s, p_1^s, p_2^s, \ldots$, where p_0^s is the number of cells containing no quanta, p_1^s is the number of cells with one quantum, . . . , and p_r^s, generally, is the number of cells with r quanta. The number of ways, W, of distributing the quanta is then the number of ways of dividing A_s objects into groups containing $p_0^s, p_1^s, p_2^s, \ldots$ members. Taking all frequency intervals into account, this leads to the expression,

$$W = \prod_s \frac{A_s!}{p_0^s!\, p_1^s!\, p_2^s! \ldots} \qquad (27)$$

The problem is now to find the set of numbers $\{p_r^s\}$ that maximize W subject to the constraint that the total energy E is fixed, expressed in the equation,

$$E = \sum_s N_s h\nu_s, \qquad (28)$$

where N_s is given in terms of the $\{p_r^s\}$ by

$$N_s = \sum_r r p_r^s. \qquad (29)$$

(Note that there is no constraint imposed on the total number of quanta.) Once W is identified with the Boltzmann-Planck "thermodynamic probability" and related to the entropy S of the radiation by the equation

$$S = k \ln W, \qquad (30)$$

the analysis becomes straightforward. A standard sort of calculation leads to the result

$$E = \sum_s (8\pi h\nu_s^3/c^3)\{\exp(h\nu_s/kT) - 1\}^{-1} d\nu_s, \qquad (31)$$

which is equivalent to the Planck formula of Equation (2).

Bose's procedure—determining W by counting the occupancy of the cells rather than the distribution of the quanta—made it possible to obtain the Planck law for the gas of light quanta. It is interesting to compare his paper with another,[41] published two years earlier, whose author began by stating precisely the same goal that Bose was to reach: a derivation of Planck's law from the statistical mechanics of light quanta "without the intervention of electromagnetism." In this paper more thermodynamic methods were used, but the basic statistical procedures were the old ones of Boltzmann, and the outcome was Wien's distribution rather than Planck's. The author, Louis de Broglie, found that he could obtain Planck's law only by treating the radiation as a mixture of gases whose quanta had energies $h\nu, 2h\nu, \ldots, nh\nu, \ldots$. Only when such associated quanta, or "molecules of radiation" were included could Wien's law be avoided.

This was closely related to a point that had been made first by Ehrenfest in 1911 and again in 1914: independent quanta lead to Wien's law.[42] If light quanta are to be used in interpreting the Planck distribution they must lack the statistical independence normally associated with free particles. Bose had implicitly incorporated these correlations into his theory by his unusual counting procedure which effectively denied the individuality of the light quanta.

B. The paper[36] that Einstein promised, when he forwarded Bose's work for publication, contained a quantum theory of the ideal gas—not a gas of light quanta but a gas of monatomic molecules. Using Bose's new statistical method Einstein was able to work out a consistent theory, free of the arbitrary assumptions that marred all of the earlier attempts at a quantum theory of the ideal gas. The only formal differences between Einstein's calculations and those that Bose had made came directly from the differences between the two systems: the energy-momentum relationship for nonrelativistic particles with finite mass had to be used, and the constraint expressing the fixed number of molecules in the gas had to be allowed for. The results were extremely interesting.

Einstein found that the average number of particles in a phase cell of energy ϵ was proportional to the quantity

$$\{\exp(\gamma + \epsilon/kT) - 1\}^{-1}$$

where γ is a constant, independent of ϵ but depending on the volume, temperature, and total number of particles in the gas. Since this expression differed from the Boltzmann factor,

$$\exp(-\epsilon/kT),$$

of ordinary statistical mechanics, all of the thermodynamic properties of the gas were correspondingly more complicated. Einstein was able to show, however, that his equations went over smoothly into those for the classical gas when the temperature was high and the density was low. Under all conditions the pressure P, volume V and average energy U of the gas exactly obeyed the same equation as a classical gas,

$$PV = (2/3)U. \qquad (32)$$

One of the reassuring aspects of the theory was the result it gave for the entropy. Einstein showed that, at high temperatures, the entropy had just the value previously obtained by a number of physicists,[43] the Sackur-Tetrode formula, which included the proper additive constant. At temperatures approaching absolute zero, the entropy approached zero for all values of the volume: Nernst's theorem was automatically satisfied by this gas.

In this first communication Einstein did not investigate the behavior of the gas at low temperatures in any detail, but he did give series expansions for the thermodynamic properties that showed the direction of their deviation from classical behavior. There were

relatively more of the slower, less energetic, molecules than would have been predicted by the Maxwell-Boltzmann distribution. This suggested the nature of the low temperature "degeneracy" that allowed the gas to satisfy Nernst's theorem, a degeneracy which had its ultimate origin in the indistinguishability attributed to the molecules by the Bose counting procedure.

During the fall of 1924 Einstein continued to work on the properties of his gas and particularly on its degenerate low temperature behavior. He reported[44] to Ehrenfest in September that the molecules "condense" into the state of zero energy below a definite temperature, even in the absence of attractive forces between them. With his usual skepticism he added, "The theory is pretty, but is there also any truth to it?" By early December he was convinced that there was, and he wrote[45] to Ehrenfest: "The matter of the quantum gas is getting to be very interesting. It seems to me more and more that there is much that is true and deep at the bottom of it. I look forward to our arguing about it."

Some time that autumn Einstein had read an extremely important piece of work, Louis de Broglie's Paris thesis,[46] and he immediately saw its connections with the problems he was working on.

C. I have already called attention to de Broglie's 1922 paper,[41] which was an attempt at the same sort of thing that Bose carried out successfully two years later. De Broglie did not stop with that first paper, but continued to ponder the problems of the quantum theory and, in particular, the wave-particle duality for radiation. In his own words:[47] "Then a great light suddenly dawned on me. I was convinced that the wave-particle duality discovered by Einstein in his theory of light quanta was absolutely general and extended to all of the physical world, and it seemed certain to me, therefore, that the propagation of a wave is associated with the motion of a particle of any sort—photon, electron, proton, or any other." This insight was first formulated by de Broglie in a series of three short papers[48] published in the *Comptes Rendus* in the fall of 1923. In a considerably extended and developed form this work was the subject of his thesis, submitted to the Sorbonne on November 25, 1924. Paul Langevin, to whom de Broglie turned for advice before his thesis was finally submitted, asked for a second copy which he forwarded to Einstein. The response was all that de Broglie could have hoped for: Einstein wrote Langevin that de Broglie had "lifted a corner of the great veil."[49]

So much of the content of de Broglie's thesis has been worked into the substance of contemporary physics that it takes a conscious effort to realize how very bold his ideas seemed when they were introduced. There was no shred of direct experimental evidence for the waves that he associated with material particles. The arguments that he gave for the existence of matter waves were based on the formal structure of special relativity and on the relationship between the variational principles of mechanics and optics. They did not seem to lead to new experiments that might confirm the existence of matter waves. The main idea was a beautiful one, to be sure, and it exactly complemented Einstein's work on radiation in 1905–1909: where Einstein assigned particle properties to radiation, de Broglie assigned wave properties to matter. The concept of matter waves, whose frequency ν and wave length λ were related to the particle's energy E and momentum p by the equations,

$$E = h\nu, \quad p = h/\lambda, \tag{33}$$

did lead de Broglie to an elegant and suggestive derivation of the Bohr-Sommerfeld quantum conditions. These quantum conditions appeared as the conditions for resonance of the matter waves when the corresponding particles carry out orbital motion. But it is fair to state that de Broglie's arguments were not compelling for the majority of theoretical physicists, who already had more than they could handle in the wave-particle duality for radiation and would not be inclined to complicate things further with a wave-particle duality for matter, if they could help it.

For Einstein, though, whose ideas had served to suggest de Broglie's imaginative step, matter waves could fit into the picture in a natural way. His calculations on the quantum gas, in progress at the time he read de Broglie's thesis, actually offered new arguments in support of de Broglie's idea. He made the basic point in the prefatory remarks to his second paper[50] on the quantum

theory of the ideal gas, published in January 1925. "The interest of this theory lies in the fact that it is based on the hypothesis of a far-reaching formal relationship between radiation and gas. According to this theory, the degenerate gas deviates from the gas of (ordinary) statistical mechanics in a way analogous to that in which the behavior of radiation, according to Planck's law, deviates from its behavior according to Wien's law. If Bose's derivation of the Planck radiation formula is to be taken seriously, then one may not also pass up this theory of the ideal gas; for if one is justified in considering radiation as a gas of quanta, then the analogy between the gas of quanta and the gas of molecules must be complete."

D. This second paper of Einstein's on what we would now call the theory of the Bose-Einstein gas is another of his masterworks, containing as many ideas in its dozen pages as many an annual volume of the journals of physics. Its first section established the existence of the peculiar "condensation" phenomenon that he had already mentioned in the letter to Ehrenfest quoted above.[44] Einstein found that below a certain temperature T_0, proportional to the two-thirds power of the number of molecules per unit volume and inversely proportional to the molecular mass, a finite fraction of the molecules would be found in the state of zero kinetic energy, the ground state. This fraction increases as the temperature of the gas goes to zero, and the phenomenon deserves to be called a "condensation" because the molecules in the ground state are in thermodynamic equilibrium with the remainder. The whole situation has a strong and nontrivial resemblance to the equilibrium of a condensed phase with its saturated vapor. The Einstein condensation phenomenon has had an interesting history of its own since its introduction in 1925, but that history falls outside the bounds of this study.

Einstein went on to discuss an objection that had been raised against both Bose's theory of the Planck radiation formula and his own use of Bose's method for the ideal gas. Ehrenfest and others had found fault with this work because the results indicated that the quanta (or molecules) were not statistically independent entities, and yet this point had not been brought out explicitly by either Bose or Einstein. Einstein granted the complete validity of this criticism, and proceeded to elaborate on what was involved in some detail. He compared the basic combinatorial formula of the Bose-Einstein theory with that which applied to a collection of strictly independent particles. Given that the phase space of a particle was divided into groups of cells with Z_s cells in the sth group, and that one assigned M_s particles to this group of cells, in how many ways W could this be done for a gas of n particles?

Using Bose's method of counting, where the particles were treated as indistinguishable, there would be

$$(n_s + z_s - 1)!/n_s!(z_s - 1)!$$

distinct distributions of the n_s particles among the z_s cells, so that W would be given by the equation,

$$W = \prod_s \frac{(n_s + z_s - 1)!}{n_s!(z_s - 1)!} \tag{34}$$

The entropy S of the gas must then have the form,

$$S = k\sum_s\{(n_s + z_s)\ln(n_s + z_s) - n_s\ln n_s - z_s\ln z_s\}, \tag{35}$$

where it has been assumed that n_s and z_s are always large enough so that Stirling's formula can be used.

If one answers the same combinatorial question for independent particles in the spirit of Boltzmann, then there are $z_s^{n_s}$ ways in which the n_s particles can be distributed over the z_s cells, so that the total number of complexions for the gas is given by the equation,

$$W = \frac{n!}{\prod_s n_s!}\prod_s z_s^{n_s}. \tag{36}$$

The first factor accounts for the number of ways in which the total number, n, of molecules can be divided into the groups n_s. The entropy obtained from Equation (36) has the form,

$$S = k\{n\ln n + \sum_s(n_s\ln z_s - n_s\ln n_s)\}. \tag{37}$$

A comparison of the two expressions for the number of complexions, Equations (34) and (36), made it evident that the particles were not treated as independent in the Bose-Einstein counting procedure. As Einstein put it, "The formula, therefore, indirectly expresses a certain hypothesis about a mutual interaction of the molecules whose nature is at present completely mysterious . . .".

There was another way in which the two theories could be compared. The entropy of Equation (35) is in accord with Nernst's theorem since all the particles will be in the zero energy state at absolute zero and W will be unity. The situation is a good deal less simple for the gas of independent particles. It is true that Equations (36) and (37) lead to W = 1 and zero entropy at absolute zero, but if one examines Equation (37) closely, it will be observed that the entropy is not proportional to the number of particles. There was a way of avoiding this apparent conflict with thermodynamic expectations that had become almost traditional by 1925: one simply divided the W of Equation (36) by $n!$, arguing, with some plausibility, but no rigor, that this procedure eliminated complexions that differed only by a permutation of the molecules which were really equivalent. When the term $nk\ln n$ is removed from the entropy in this way, the resulting expression is extensive, but it no longer satisfies Nernst's theorem, since this S becomes $-nk\ln n$ at absolute zero. The entropy for a gas of independent particles must, therefore, violate either Nernst's theorem or the condition of extensivity. Einstein considered these arguments a reason to prefer the Bose procedure, even if that procedure could not be demonstrated as superior on a priori grounds.

E. The study of fluctuation phenomena had been a characteristic of Einstein's scientific style for over twenty years:[7] his insight into the structure of radiation was based, in large part, on just such studies, as we have already seen. And so it is not surprising that he turned next[50] to an analysis of the density fluctuations required by his new gas theory. He considered the container of the gas, of volume V, connected to an infinite reservoir of gas by a partition that passed only molecules having energies within a specified interval ΔE. He then examined the fluctuations Δ_s about the average values n_s of the number of molecules in this energy interval. A calculation completely analogous to the one he had reported in 1909, (described above in Section II B), led to the result,

$$\langle \Delta_s^2 \rangle = k(-\partial^2 S/\partial n_s^2)^{-1}. \quad (38)$$

The derivative on the right hand side could be evaluated with the help of Equation (35), and the final expression took the form,

$$\langle \Delta_s^2 \rangle = n_s + n_s^2/z_s. \quad (39)$$

This expression was the exact equivalent of the formula for the energy fluctuations in black-body radiation that Einstein had derived in 1909, Equation (9a) above. If one notes that the quantity z_s has the explicit form $(8\pi\nu^2 V d\nu/c^3)$ for radiation, and that the quantities E and ϵ in Equation (9a) can be thought of as $n_s h\nu$ and $\Delta_s h\nu$, respectively, the equations are seen to be identical. The first term is the only one that would be present for a gas of independent particles, where Equation (37) rather than Equation (35) must be used in calculating $\langle \Delta_s^2 \rangle$. The second term then corresponds to the classical wave interference term in the case of radiation. While the first or particle term was the surprising one for radiation, the second term, in the case of the gas, was the one demanding explanation.

"One can interpret it in a corresponding way for the gas too, if one associates a radiation [wave] process with the gas in a suitable way, and calculates its interference fluctuations. I go into this interpretation in more detail because I believe that there is more than a mere analogy involved here." Einstein's reasons for believing that it was no mere analogy that he found, lay in de Broglie's work: "In a very noteworthy work, de Broglie has shown how one can associate a (scalar) wave field with a material particle or a system of material particles. (This dissertation also contains a very remarkable geometrical interpretation of the Bohr-Sommerfeld quantum rule.)" He went on to describe the de Broglie waves, whose frequency ν and phase velocity u were given by the expressions

$$\nu = (m_0 c^2/h)(1-\beta^2)^{-1/2}; \quad u = c^2/v \quad (40)$$

where m_0 is the particle's rest mass, v is the particle's velocity with respect to the observer, and $\beta = v/c$, and to mention that he had convinced himself by calculations that the term n_s^2/z_s could be accounted for by the fluctuating interference of these waves.

Einstein's next step was thoroughly in character. Just as he had gone on from his studies of radiation fluctuations, in 1905, to search out the experimental consequences of the new ideas to which they had led him, he now pursued the implications of the de Broglie waves whose existence seemed required by his new calculations. "This wave field, whose physical nature is still obscure for the time being, must in principle be demonstrable by the diffraction phenomena corresponding to it. A beam of gas molecules which passes through an aperture must, then, undergo a diffraction analogous to that of a light ray. In order that such a phenomenon be observable the wave length λ must be more or less comparable with the dimensions of the aperture." Since de Broglie's relations led to a wavelength λ equal to $h/m\nu$ in the nonrelativistic limit, one could see that direct detection of the diffraction with the help of ordinary apertures seemed out of the question: the wavelengths of the molecules moving with thermal velocities would be only of the order of 10^{-8} or 10^{-9} cm. Such wavelengths, however, are of the same order as molecular dimensions, and this suggested a new possibility to Einstein.

Moving molecules, whose associated wavelength λ is of the order of the molecular diameter, would be strongly diffracted on collisions with other molecules at rest. The mean free path of the molecules in the gas would therefore be reduced by this additional scattering mechanism, and one could expect to see the consequences of this fact in the variation of the viscosity coefficient of the gas with temperature. The effect would be most pronounced for the lightest gases, hydrogen and helium, at low temperatures. There should also be detectable consequences of these diffraction effects in the equation of state of the gas.

Einstein devoted the closing pages of his paper to some additional remarks on the condensation phenomenon and a possible use for it. Although the degeneracy temperature for all ordinary gases seemed too low for the effects to be directly observable, the "gas" of free electrons in a metal might well show these effects.

The new statistics applied to these electrons showed that under normal conditions less than one ten thousandth of the electrons were not in the zero energy condensed phase, so that the old puzzle of the absence of an electronic contribution to the specific heat capacity of metals would at once be cleared up. Even the mystery of superconductivity might be lightened by a further investigation using these methods.* Einstein indicated that these remarks were still very speculative, but he left no doubt of the importance that he attached to the whole theory.

V. From Einstein's Remarks to Schrödinger's Wave Mechanics

A. No one could assess the importance of Einstein's support for de Broglie's ideas more fairly than has de Broglie himself. "The scientific world of the time hung on every one of Einstein's words, for he was then at the peak of his fame. By stressing the importance of wave mechanics, the illustrious scientist had done a great deal to hasten its development. Without his paper my thesis might not have been appreciated until very much later."[53]

Einstein's support was certainly vigorous. Pauli has written[54] that he "remembers that, in a discussion at the physics meeting in Innsbruck in the autumn of 1924, Einstein proposed a search for interference and diffraction phenomena with molecular beams." These phenomena were indeed found in molecular beam experiments by Einstein's old student Otto Stern[55] and his collaborators, but not until several years after they had already been studied for electrons by Davisson and Germer[56] and by G. P. Thomson[57] in 1927.

The first direct use of the idea of matter waves came in July 1925 when Walter Elsasser,[58] a research student at Göttingen, sent a short note to *Naturwissenschaften* on the quantum mechanics of free electrons. Max Born[59] and James Franck, who were professors of theoretical and experimental physics, respectively, at Göttingen had suggested to Elsasser that he investigate some anomalous results in the scattering of electrons by a metal plate, discovered by Davisson and Kunsman[60] in 1923. These experimentalists had found some unexpected maxima in the angular distribution of the scattered electrons, and it apparently occurred to Born and Franck that these might have something to do with de Broglie waves. Elsasser showed that if one interpreted these maxima as diffraction peaks, arising from the diffraction of the de Broglie waves of the electrons by the crystal lattice of platinum in the metal plate, then the wavelengths, so calculated, agreed in order of magnitude with those obtained from de Broglie's formula, Equation (33), for the electron energies used in the experiment. The agreement was only in order of magnitude, but since polycrystalline samples had been used only crude estimates could be made anyway.

Elsasser also used the concept of matter waves to suggest an explanation of the Ramsauer effect.[61] Ramsauer and others had found that electrons accelerated through only a few volts had anomalously long mean free paths in the inert gases. This effect seemed to go very much like the scattering of light from colloidal particles—the wavelengths again matched very roughly with those calculated from de Broglie's formula.

What is especially interesting in the present context is the first paragraph of Elsasser's paper, in which he stated the background for his work: "By way of a detour through statistical mechanics, Einstein has recently arrived at a physically very remarkable result. Namely, he makes plausible the assumption that a wave field is to be associated with every translational motion of a material particle, the properties of the field being determined by the kinematics of the particle. The hypothesis of such waves, already advanced by de Broglie before Einstein, is so strongly supported by Einstein's theory that it seems appropriate to look for experimental tests for it."

One gets the same impression from an examination of Alfred Landé's book, *The Modern Development of the Quantum The-*

* I should point out that Pauli's exclusion principle[51] was first published in January, 1925 and Fermi's statistical method for systems obeying the exclusion principle did not appear until a year afterwards.[62] It was, of course, the Fermi statistics rather than the Bose statistics that held the key to the behavior of metals.

ory,[62] whose second edition was completed in January, 1926. Landé's book was intended as a review of the recent literature of the subject, and in the preface he remarked: "It may perhaps be considered premature that Einstein's gas degeneracy with its 'interference of matter' is taken up; the conceptual content of this investigation is, nevertheless, so abundant and fruitful, also in connection with other questions concerning quanta (the light quantum theory), that a report on it seemed to be demanded, despite the fact that there is as yet no experimental foundation of Einstein's theory."

There is no question that Einstein's authority and his new arguments brought de Broglie's ideas the attention they deserved.

B. The physicist who derived most benefit from his study of de Broglie's thesis was Erwin Schrödinger, and he too was drawn to de Broglie's work through Einstein's quantum theory of the ideal gas. This was natural enough, since Schrödinger had been working on the problems associated with applying the quantum theory to gases before Einstein's papers appeared in 1924 and early 1925. He was well qualified, therefore, to appreciate the power and the novelty of Einstein's theory and to investigate its implications.

There had been attempts to apply the quantum theory to gases from 1911 on, with a good many theoretical physicists trying to develop such a theory by a variety of different methods. It would take us too far afield to follow the history of these attempts. It is sufficient for our purposes to point out that one of the key issues was the significance of the factor $n!$ in the equation for the number of complexions of the gas, Equation (36) above. None of the arguments offered for eliminating this factor was really satisfactory, a point cogently made by Ehrenfest[43] in 1920, and seconded in a paper[63] that Schrödinger published early in 1924.

Schrödinger's principal concern in this paper was with the temperature below which a gas would show degenerate behavior. Nernst[64] had proposed that gases, when cooled at constant volume and kept from condensing, would, at sufficiently low temperatures, enter a degenerate state in which their behavior would be consonant with Nernst's theorem (the third law of thermodynamics).

It could be argued on dimensional grounds that this degeneracy temperature, Θ, ought to have the form,

$$\Theta = h^2/mk\lambda^2, \qquad (41)$$

where m is the mass of a molecule and λ is some characteristic length. Schrödinger tried to show that this characteristic length λ should be the mean free path of the gas molecules, rather than a length of the order of the dimensions of the container or the mean intermolecular distance. All of these had been suggested as possible candidates. There is no need for us to look at the details of his reasoning, but it is important, in view of what was to follow, to notice that his procedure was to specify the state of the gas as a whole, and to evaluate the partition sum for the gas rather than for the individual molecules. It is also worth mentioning that the idea of using the mean free path as the characteristic length was first discussed by Sommerfeld and Lenz[65] in a theory based on consideration of the proper vibrations of the gas in the manner of Debye's theory of the specific heat of crystals. Schrödinger had used the work of Sommerfeld and Lenz as one of his points of departure. As I have already mentioned, he followed Ehrenfest in rejecting the arguments previously advanced, by Planck[66] in particular, for eliminating the $n!$ contribution to the entropy.

The first fruit of Schrödinger's study of Einstein's gas theory was a paper[67] that Planck communicated for him to the Prussian Academy in July, 1925. In this paper Schrödinger returned to the question of whether or not to divide the $n!$ out of the formula counting the number of complexions. This time he accepted, at least for the sake of discussion, Planck's view that the $n!$ should be eliminated because permutations of molecules of the same kind ought not to be considered as giving rise to new complexions. But, argued Schrödinger, even if one grants the validity of this point of view as to the definition of physically different complexions of the gas, it is still not true that division by $n!$ is the proper way to correct the equation. The reason for making the correction is that permutations, in which molecules simply interchange their roles, should not be counted as leading to distinct states. But, "in order for two molecules to be able to interchange their roles, they really must have different roles; otherwise the previous counting has certainly not counted such states as being different; so we need not and may not 'correct away' a multiplicity that never existed in our equation at all! On the contrary, one recognizes that the multiplicity with which a definite 'state in the new sense' appeared in our earlier counting is given precisely by the value $(n!/\prod_s n_s!)$ itself." The proper and consistent way to correct for the effective indistinguishability of the molecules is, therefore, to drop the whole combinatorial factor and to say that each set of numbers $n_1, n_2, \ldots n_s \ldots$ defines a single distribution, regardless of the number of molecular permutations compatible with it. This is exactly the counting procedure of the Bose-Einstein statistics.

Schrödinger's main point, then, was that the new statistical procedure was the necessary consequence of following through the elimination of physically redundant permutations in a logically consistent manner. This new statistical procedure meant "a radical departure from the Boltzmann-Gibbs kind of statistics." Even though there was no interaction energy assumed between the molecules, the new statistics still implied a mysterious correlation, not yet understood.

C. When Schrödinger collected his epoch-making articles on wave mechanics into a book[68] in November, 1926, he unfortunately did not begin quite at the beginning of the story. He did not include the first paper in which he used de Broglie's ideas, the paper that preceded the series on *Quantization as a Proper Value Problem* and that should have served as a preface to them in his book. That paper,[69] *On Einstein's Gas Theory*, was sent to the *Physikalische Zeitschrift* on December 15, 1925, just six weeks before he finished the first of the series on the wave equation and its applications. It is a remarkable work, for its insight and its elegance, as well as for its historical significance. The basic idea cannot be formulated more sharply or more clearly than Schrödinger put it in his opening paragraphs:

"The essential point of the new theory of a gas that Einstein has recently worked out is generally considered to be this: a completely new kind of statistics, the so-called Bose-Einstein statistics,

is to be applied to the motion of gas molecules. It goes against the grain, and justifiably so, to consider these statistics as something primary, incapable of further explanation. They seem, on the contrary, to disguise a certain mutual dependence or interaction between the gas molecules which is, however, hard to analyze in this form.

"One might expect to gain a deeper insight into the essence of the new theory if one could manage to preserve the validity of the old statistical methods—proved by experiment and logically well-based—and to undertake the change in the foundations at a point where it can be done without 'sacrificium intellectus.' The following simple idea is a guide to this goal: Einstein's theory of a gas is obtained by applying to the gas molecules the form of statistics that leads to the Planck radiation law when it is applied to 'atoms of light', (photons). One can, however, also obtain the Planck radiation law by using 'natural' statistics, if one applies them to the so-called 'aether oscillators,' that is, to the degrees of freedom of the radiation. The photons then appear only as the energy levels of the aether oscillators. The transition from natural statistics to Bose statistics can always be retrieved by interchanging the roles of the two concepts: 'the manifold of energy states' and 'the manifold of carriers of these states.' One must, therefore, simply form a picture of the gas like the picture of cavity radiation that does *not* correspond to the extreme light-quantum representation; the natural statistics—using the convenient method of Planck's partition sum, for example—will then lead to the Einstein gas theory. *This means nothing else but taking seriously the de Broglie-Einstein wave theory of moving particles, according to which the particles are nothing more than a kind of 'wave crest' on a background of waves.*" (The italics are mine.)

The situation to be considered was the usual one of an ideal gas of n monatomic molecules in a container of volume V at temperature T. In a common formulation one would say that each molecule could have any of the energies $\epsilon_1, \epsilon_2, \ldots, \epsilon_s, \ldots$ and that any number of molecules could be in each of these states. Schrödinger preferred to say instead that the gas had various modes of oscillation, and that the sth mode had possible energies $0, \epsilon_s, 2\epsilon_s, \ldots, n_s\epsilon_s, \ldots$, according to whether it was "occupied" by $0, 1, 2, \ldots,$

$n_s, \ldots$ molecules. The gas could then be viewed as a collection of harmonic oscillators, the spectrum of this collection being determined by the set $\{n_s\}$.

The partition sum Z of the gas, the sum of the Boltzmann factors $\exp(-E/kT)$ over all possible energies of the gas as a whole, would then be given by the equation,

$$Z = \Sigma \exp\{-(n_1\epsilon_1 + n_2\epsilon_2 + \ldots + n_s\epsilon_s + \ldots)/kT\}, \quad (42)$$

where the sum is over all nonnegative integral values for the $\{n_s\}$, subject to the condition that

$$\sum_s n_s = n, \quad (43)$$

expressing the fixed number of particles in the gas. The sum could be evaluated relatively directly, including the constraint condition, by using the methods introduced a few years earlier by Darwin and Fowler.[70] Once this had been done the properties of the gas were given as explicit sums over the energy spectrum $\{\epsilon_s\}$, which had then to be determined.

It would have been inconsistent to determine the energies $\{\epsilon_s\}$ by dividing the molecular phase space into cells, as Einstein had done, since Schrödinger was treating the set $\{\epsilon_s\}$ as the spectrum of the gas as a whole. His procedure was instead to follow de Broglie in considering the particle as a "signal" in the system of waves, with properties determined by Equations (33) or (40). Finding the spectrum then meant finding the number of normal modes for a wave phenomenon in the volume V, where the dispersion law for the waves was given by de Broglie's equations. This could readily be done, at least for frequencies that were high enough so that the shape of the container was unimportant, and it led to the result

$$\Omega = (4\pi V/^3)(2m/h^2)^{3/2}\epsilon^{3/2}, \quad (44)$$

where Ω is the number of modes whose energy does not exceed ϵ, a result identical with what Einstein had found by counting phase cells of content h^3.

Schrödinger drew attention to the fact that the mass of the particles appeared in the equation for the spectrum of the gas, just as it appeared in the de Broglie dispersion law relating frequency ν and wave length λ,

$$\nu = (h/2m\lambda^2), \quad (45)$$

to quote only the nonrelativistic form.

Once the energy spectrum was known it was straightforward to complete the calculation by showing that the molecular distribution law, and therefore all other thermodynamic properties of the gas, agreed exactly with Einstein's results. The fluctuations could also be calculated very simply and generally by Schrödinger's methods, again agreeing with Einstein's formulas.

The last section of Schrödinger's paper dealt with the possibility of representing particles or light quanta by the interference of waves. Here Schrödinger briefly discussed the use of wave packets, localized in space and time, that constituted "signals" and could serve as models for particles in the wave theory. Even at this stage he recognized that such wave packets, containing waves of a range of frequencies and wave vectors, would not hold together in time, but would soon spread out. Only if this difficulty could be overcome could such wave packets really be used as models for particles.

Although Schrödinger did not include this paper in the canon of his works on wave mechanics, and it has therefore gone unmentioned when the history of that subject is discussed, he made no mystery of its organic connection with his more famous papers. In the first[71] of the series on *Quantization as a Proper Value Problem*, after giving his new solution of the problem of the hydrogen atom, he described the relationship between his ideas and de Broglie's, observing that he had worked with standing waves rather than with the running waves used by de Broglie. "I have recently shown," he went on, referring to the paper in question which was then in press, "that one can base Einstein's theory of a gas on the consideration of such standing proper vibrations, for which one assumes the dispersion law of de Broglie's phase waves. The considerations concerning the atom reported above could have been presented as a generalization of this work on the gas model."

Einstein's gas theory, and the "short but infinitely far-seeing remarks" that it suggested, form the link between de Broglie's matter waves and Schrödinger's wave mechanics.

When Albert Einstein wrote his *Autobiographical Notes*,[72] ("Here I sit at the age of 67 in order to write something like my own obituary"), he made some remarks on his education that reveal more of the nature of his genius than his many biographers have been able to. He explained that one reason for his preference of physics to mathematics was that "my mathematical intuition was not strong enough to make a sure distinction between what was fundamentally important, really basic, and the rest—the erudition that one could more or less dispense with." As a result his feelings, when he was confronted with the various specialities within mathematics, were like those of Buridan's ass, unable to decide upon any particular bundle of hay. "True enough, physics was also divided into separate fields each of which could devour a short working life without having satisfied the hunger for deeper knowledge. . . . But in physics I soon learned to scent out the paths that led to the depths, and to disregard everything else, all the many things that clutter up the mind and divert it from the essential."

He made the same point more pithily in a remark to one of his last assistants, Ernst Straus:[73] "God is inexorable in the way He has allotted His gifts. He gave me the stubbornness of a mule and nothing else; really, He also gave me a keen scent."

Footnotes

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