Einstein and the quantum theory*

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This is an account of Einstein's work and thoughts on the quantum theory. The following topics will be discussed: The light-quantum hypothesis and its gradual evolution into the photon concept. Early history of the photoelectric effect. The theoretical and experimental reasons why the resistance to the photon was stronger and more protracted than for any other particle proposed to date. Einstein's position regarding the Bohr–Kramers–Slater suggestion, the last bastion of resistance to the photon. Einstein's analysis of fluctuations around thermal equilibrium and his proposal of a duality between particles and waves, in 1909 for electromagnetic radiation (the first time this duality was ever stated) and in January 1925 for matter (prior to quantum mechanics and for reasons independent of those given earlier by de Broglie). His demonstration that long-known specific heat anomalies are quantum effects. His role in the evolution of the third law of thermodynamics. His new derivation of Planck's law in 1917 which also marks the beginning of his concern with the failure of classical causality. His role as one of the founders of quantum statistics and his discovery of the first example of a phase transition derived by using purely statistical methods. His position as a critic of quantum mechanics. Initial doubts on the consistency of quantum mechanics (1926–1930). His view maintained from 1930 until the end of his life: quantum mechanics is logically consistent and quite successful but it is incomplete. His attitude toward success. His criterion of objective reality. Differences in the roles relativity and quantum theory played in Einstein's life. His vision regarding quantum theory in the context of a unified field theory. His last autobiographical sketch, written a few months before his death, concluding with a statement about the quantum theory, a subject to which (by his own account) he had given more thought than even to general relativity.

CONTENTS

I. EINSTEIN, THE QUANTUM AND APARTNESS.

> **Apart**, *adv.*, 4. Away from others in action or function; separately, independently, individually ....
>
> Oxford English Dictionary

A. Introduction

In 1948 I undertook to put together the Festschrift in honor of Einstein's seventieth birthday.[1] In a letter to prospective contributors I wrote (1948): "It is planned that the first article of the volume shall be of a more personal nature and, written by a representative col-

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league, shall pay homage to Einstein on behalf of all contributors." I then asked Robert Andrews Millikan (1868–1953) to do the honors, as the senior contributor.[2] He accepted and his article (1949) is written in his customary forthright manner. On that occasion he expressed himself as follows on the equation $E = h\nu - P$ for the photoelectric effect. "I spent ten years of my life testing that 1905 equation of Einstein's and, contrary to all my expectations, I was compelled in 1915 to assert its unambiguous verification in spite of its unreasonableness since it seemed to violate everything we knew about the interference of light."

Physics had progressed, and Millikan had mellowed since the days of his 1915 paper on the photoeffect, as is evidenced by what he had written (1916a) at that earlier time: "Einstein's photoelectric equation .... appears in every case to predict exactly the observed results ... Yet the semicorpuscular theory by which Einstein arrived at his equation seems at present wholly untenable"; and in his next paper (1916b) Millikan mentioned "the bold, not to say the reckless, hypothesis of an electromagnetic light corpuscle." Nor was Millikan at that time the only first-rate physicist to hold such views, as will presently be recalled. Rather, the physics community at large had received the light-quantum hypothesis with disbelief and with skepticism bordering on derision. As one of the architects of the pre-1925 quantum theory, the old quantum theory, Einstein had quickly found both enthusiastic and powerful support for one of his two major contributions to this field: the quantum theory of specific heat. (There is no reason to believe that such support satisfied any particular need in him.) By sharp contrast, from 1905–1923 he was a man apart in being the only one, or almost the only one, to take the light-quantum seriously.

If I had to characterize Einstein by one single word I would choose "apartness." This was forever one of his deepest emotional needs. It was to serve him in his singleminded and singlehanded pursuits, most notably on his road to triumph from the special to the general theory of relativity. It was also to become a practical necessity for him, in order to protect his cherished privacy from a world hungry for legend and charisma. In all of Einstein's scientific career, this apartness was never more pronounced than in regard to the quantum theory. This covers two disparate periods, the first one of which (1905–1923) I have just mentioned. During the second period, from 1926 until the end of his life, he was the only one, or again nearly the only one, to maintain a profoundly skeptical attitude to quantum mechanics. I shall discuss Einstein's position on quantum mechanics in Secs. VIII and X but cannot refrain from stating at once that Einstein's skepticism should not be equated with a purely negative attitude. It is true that he was forever critical of quantum mechanics. But at the same time he had his own alternative program for a synthetic theory in which particles, fields, and quantum phenomena all would find their place. Einstein pursued this program from about 1920 (before the discovery of quantum mechanics!) until the end of his life. Numerous discussions with him in his later years have helped me gain a better understanding of his views. Some personal reminiscences of my encounters with Einstein are found in Sec. X.A.

But let me first return to the days of the old quantum theory. Einstein's contributions to it can be grouped under the following headings.

(a) *The light-quantum*. In 1900 Planck had discovered the blackbody radiation law without using light-quanta. In 1905 Einstein discovered light-quanta without using Planck's law. Section II is devoted to the light-quantum hypothesis. The interplay between the ideas of Planck and Einstein is discussed. A brief history of the photoelectric effect from 1887 to 1915 is given. This section ends with Einstein's formulation, in 1909, of the particle–wave duality for the case of electromagnetic radiation.

(b) *Specific heats*. Toward the end of the nineteenth century there existed evident conflicts between the data on specific heats and their interpretation in terms of the equipartition theorem of classical statistical mechanics. In 1907 Einstein published the first paper on quantum effects in the solid state which showed the way out of these paradoxes. This paper also played an important role in the final formulation of the third law of thermodynamics. These topics are discussed in Sec. III.

(c) *The photon*. The light-quantum as originally defined was a parcel of energy. The concept of the photon as a particle with definite energy and *momentum* emerged only gradually. Einstein himself did not discuss photon momentum until 1917. Relativistic energy momentum conservation relations involving photons were not written down till 1923. Einstein's role in these developments is discussed in Sec. IV. At the beginning of that section I continue the discussion of the reactions to the light-quantum hypothesis, of which I have already given a few samples. This section also contains an account of Einstein's discovery of the A and B coefficients and of his earliest concern with the breakdown of classical causality. The section concludes with remarks on the role of the Compton effect.

The reader may wonder why the man who wrote down the relation $E = h\nu$ for light in 1905 and who propounded the special theory of relativity in that same year would not have stated sooner the relation $p = h\nu/c$. I shall comment on this question in Sec. X.C.

(d) Einstein's work on quantum statistics is treated in Sec. VI, which also includes a discussion of Boltzmann's axiom on identical distinguishable particles and of Bose's contribution.

(e) Einstein's role as a key transitional figure in the discovery of wave mechanics will be discussed in Sec. VII.

I shall continue the outline of this paper in part (c) of this section. First, however, I should like to take leave of our main character for a brief while in order to give an overview of the singular role of the photon in the history of the physics of particles and fields. In so doing I shall interrupt the historical sequence of events in

[2]It was decided later that L. de Broglie, M. von Laue, and Ph. Frank should also write articles of a more personal nature.

order to make some comments from today's vantage point.

B. Particle physics: The first fifty years

Let us leave aside the photon for a while and ask how physicists reacted to the experimental discovery or the theoretical prediction (whichever came first) of other new particles.[3]

The discovery in 1897 of the first particle, the electron, was an unexpected experimental development which brought to an end the ongoing debate: are cathode rays molecular torrents or aetherial disturbances? The answer came as a complete surprise: They are neither but rather a new form of matter. There were some initial reactions of disbelief. Joseph John Thomson (1856–1940) once recalled (1936) the comment of a colleague who was present at the first lecture Thomson gave on the new discovery: "I [J.J.] was told long afterwards by a distinguished physicist who had been present at my lecture that he thought I had been 'pulling their legs'." Nevertheless the existence of the electron was widely accepted within the span of very few years. By 1900 it had become clear that beta rays are electrons as well. The discoveries of the free electron and of the Zeeman effect (in 1896) combined made it evident that a universal atomic constituent had been discovered and that the excitations of electrons in atoms were somehow the sources of atomic spectra.

The discovery of the electron was a discovery at the *outer* experimental frontier. In the first instance this finding led to the abandonment of some earlier qualitative concepts (of the indivisibility of the atom), but it did not require, or at least not at once, a modification of the established corpus of theoretical physics.

During the next fifty years three other particles entered the scene in ways not so dissimilar from the case of the electron, namely via unexpected discoveries of an experimental nature at the outer frontier. They are: the proton, (or, rather, the nucleus), the neutron[4] and—just half a century after the electron—the muon, the first of the electron's heavier brothers. As to the acceptance of these particles, it took little time to realize that their coming was in each instance liberating. Within two years after Rutherford's nuclear model, Niels Bohr (1885–1963) was able to make the first real theoretical predictions in atomic physics. Almost at once after the discovery of the neutron, the first viable models of the nucleus were proposed, and nuclear physics could start in earnest. The muon is still one of the strangest animals in the particle zoo, yet its discovery was liberating too since it made possible an understanding of certain anomalies in the absorption of cosmic rays. (Prior to the discovery of the muon, theorists had already speculated about the need for an extra particle to explain these anomalies.)

To complete the particle list of the first half century there are four more particles[5] which have entered physics—but in a different way: initially they were theoretical proposals.

The first neutrino was proposed in order to save the law of energy conservation in beta radioactivity. The first meson (now called the pion) was proposed as the conveyer of nuclear forces. Both suggestions were ingenious, daring, innovative, and successful—but did not demand a radical change of theory. Within months after the public unveiling of the neutrino hypothesis the first theory of the weak interaction, which is still immensely useful, was proposed. The meson hypothesis immediately led to considerable theoretical activity as well.

The neutrino hypothesis was generally assimilated long before this particle was actually observed. The interval between the proposal and the first observation of the neutrino is even longer than the corresponding interval for the photon. The meson postulate found rapid experimental support from cosmic-ray data—or so it seemed. More than a decade passed before it became clear that the bulk of these observations actually involved muons instead of pions.

Then there was the positron, "a new kind of particle, unknown to experimental physics, having the same mass and opposite charge to an electron" (Dirac, 1931). This particle was proposed in 1931, after a period of about three years of considerable controversy over the meaning of the negative energy solutions of the Dirac equation. During that period one participant (Weyl, 1930) expressed fear for "a new crisis in quantum physics." The crisis was short-lived, however. The experimental discovery of the positron in 1932 was a triumph for theoretical physics. The positron theory belongs to the most important advances of the nineteen thirties.

And then there was the photon, the first particle to be predicted theoretically.

Never, either in the first half-century or in the years thereafter, has the idea of a new particle met for so long with such almost total resistance as the photon. The light-quantum hypothesis was considered somewhat of an aberration even by leading physicists who otherwise held Einstein in the highest esteem. Its assimilation came after a struggle more intense and prolonged than for any other particle ever postulated. Because never, to this day, has the proposal of any particle but the photon led to the creation of a new *inner* frontier. The hypothesis seemed paradoxical: light was known to consist of waves, hence it could not consist of particles. Yet this paradox alone does not fully account for the resistance to Einstein's hypothesis. We shall look more closely at the situation in Sec. IV.A.

[3]No detailed references to the literature will be given, in keeping with the brevity of my comments on this subject.

[4]It is often said, and not without grounds, that the neutron was actually anticipated. In fact, twelve years before its discovery, in one of his Bakerian lectures (1920), Ernest Rutherford (1871–1937) spoke of "the idea of the possible existence of an atom of mass one which has zero nuclear charge." Nor is there any doubt that the neutron being in the air at the Cavendish was of profound importance (Chadwick, 1962) to its discoverer James Chadwick (1891–1974). Even so, not even a Rutherford could have guessed that his 1920 neutron (then conjectured to be a tightly bound proton-electron system) was so essentially different from the particle that would eventually go by that name.

[5]It is too early to include the graviton.

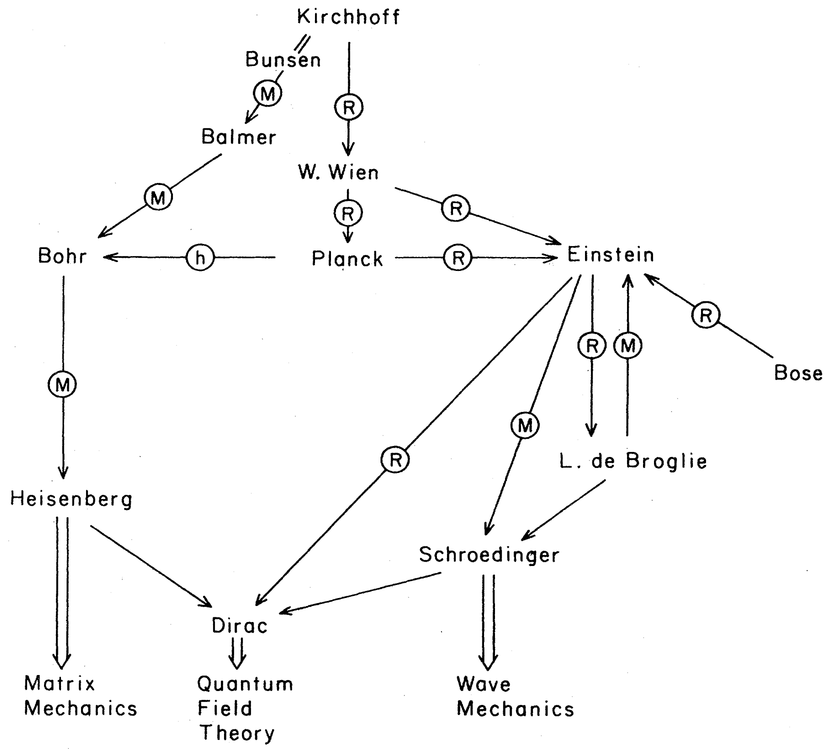

FIG. 1. The quantum theory: Lines of influence.

C. The quantum theory: Lines of influence

The skeleton diagram given in Fig. 1 is an attempt to reduce the history of the quantum theory to its barest outlines. At the same time this figure will serve as a guide to the rest of this paper. X → Y means: the work of X was instrumental to an advance by Y. Arrows marked M and R indicate that the influence went via the theory of matter and of radiation, respectively.

If Planck, Einstein, and Bohr are the fathers of the quantum theory, then Gustav Robert Kirchhoff (1824–1887) is its grandfather. Since he was the founder of optical spectra analysis [in 1860, together with Robert Bunsen (1811–1899)], an arrow leads from him and Bunsen to Johann Jakob Balmer (1825–1898), the inventor of the Balmer formula (1885). From Balmer we move to Bohr, the founder of atomic quantum dynamics. Returning to Kirchhoff, as the discoverer of the universal character of blackbody radiation (Kirchhoff, 1860), we note that his influence goes via Wien to Planck. (See further Sec. II.A.)

The arrow from Wien to Planck refers to the latter's formulation of his blackbody radiation law (Sec. II.A), and the triangle Wien–Planck–Einstein to the mutual influences which led to the light-quantum hypothesis (Sec. II.B–II.E).

The arrow from Bose to Einstein refers to Bose's work on electromagnetic radiation and its impact on Einstein's contributions to the quantum statistics of a material gas. See Sec. VI, where Einstein's influence on Dirac is also briefly mentioned.

The triangle Einstein–de Broglie–Schroedinger has to do with the role of Einstein as the transitional figure in the birth of wave mechanics, discussed in Sec. VII.

The h marking the arrow from Planck to Bohr serves as a reminder that not so much the details of Planck's work on radiation as the very introduction by Planck of his new universal constant h was decisive for Bohr's ideas about atomic stability. An account of Bohr's influence on Heisenberg and of Heisenberg's and Schroedinger's impact on Dirac is beyond the scope of the present paper.

In the case of Einstein and Bohr it cannot be said that the work of one induced major advances in the work of the other. Therefore the simplified diagram does not and should not contain links between them. Nevertheless, for forty years there were influences at work between Einstein and Bohr and these were in fact intense, but they were on a different plane. In a spirit of friendly and heroic antagonism these two men argued about questions of principle. Section V deals with Bohr's resistance to Einstein's idea of the photon. This was but a brief interlude. It ended with the detailed experimental vindication of the photon concept to which Bohr fully subscribed from then on. Their far more important debate on the foundations of quantum mechanics began in 1927. On these issues the intellectual resistance and opposition of one against the most basic views held by the other continued unabated until the end of Einstein's life. At issue were the criteria by which one should judge the completeness of the description of the physical world. Their discussions have not affected the evolution of physical theory. Yet theirs will be remembered as one of the great debates on scientific principle between two dominant contemporary figures. The dialog between Bohr and Einstein had one positive outcome: it forced Bohr to express the tenets of complementarity in increasingly precise language. This debate will be one of the themes of Sec. VIII which deals with Einstein's objections to quantum mechanics.

A point made earlier bears repeating here: Einstein's own visions on physics issues were often in opposition to the mainstream, but they were never negative. So it was in the case of quantum mechanics. After 1930 he considered this theory to be consistent and successful but incomplete. At the same time he had his own aspirations for a future theory of particles and fields. I shall try to make clear in Sec. X what these were.

I do not believe that Einstein presented valid arguments for the incompleteness of quantum theory. But neither do I think that the times are ripe to answer the question whether the quantum-mechanical description is indeed complete, since to this day the physics of particles and fields is a subject beset with many unresolved fundamental problems. Among these there is one which was most dear to Einstein and with which he (and all of us, to date) struggled in vain: the synthesis of quantum physics with general relativity. Since we still have far to go, any assessment of Einstein's views must necessarily be tentative. In order to stress this I have prefaced Sec. X on Einstein's vision with a very brief overview (Sec. IX) of the current status of particle physics.

II. THE LIGHT-QUANTUM

A. From Kirchhoff to Planck

In the last four months of 1859 there occurred a number of events which were to change the course of science.

On the twelfth of September, Urbain Jean Joseph Le Verrier (1811–1877) submitted to the French Academy the text of a letter to Hervé Faye (1814–1902) in which he recorded that the perihelion of Mercury advances by thirty-eight seconds per century due to "some as yet unknown action on which no light has been thrown,"[6] (Le Verrier, 1859). The effect was to remain unexplained[7] until the days of general relativity. On the twenty-fourth of November a book was published in London, entitled *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life*, by Charles Robert Darwin (1809–1882). Meanwhile on the twentieth of October Gustav Kirchhoff (1859) from Heidelberg submitted his observation that the dark D-lines in the solar spectrum are darkened still further by the interposition of a sodium flame. As a result, a few weeks later he proved a theorem and posed a challenge. The response to Kirchhoff's challenge led to the discovery of the quantum theory.

Consider a body in thermal equilibrium with radiation. Let the radiation energy which the body absorbs be converted to thermal energy only, not to any other energy form. Let $E_\nu d\nu$ denote the amount of energy emitted by the body per unit time per cm^2 in the frequency interval $d\nu$. Let A_ν be its absorption coefficient for frequency ν. Kirchhoff's theorem (1860) states that E_ν/A_ν depends only on ν and the temperature T and is independent of any other characteristic of the body:

$$E_\nu/A_\nu = J(\nu, T)\,. \tag{1}$$

Kirchhoff called a body perfectly black if $A_\nu = 1$. Thus $J(\nu, T)$ is the emissive power of a black body. He also gave an operational definition for a system, the "Hohlraumstrahlung", which acts as a perfectly black body: "Given a space enclosed by bodies of equal temperature, through which no radiation can penetrate, then every bundle of radiation within this space is constituted, with respect to quality and intensity, as if it came from a completely black body of the same temperature."

Kirchhoff (1860) challenged theorists and experimentalists alike: "It is a highly important task to find this function [J]. Great difficulties stand in the way of its experimental determination. Nevertheless there appear grounds for the hope that it can be determined by experiment, since undoubtedly it has a simple form as do all functions which do not depend on the properties of individual bodies and which one has become acquainted with till now."

Kirchhoff's emphasis on the experimental complexities turned out to be well justified. Even the simple property of J that it has one pronounced maximum which moves to lower ν with decreasing T was not firmly established experimentally until about twenty years later (Kangro, 1976). The experimentalists had to cope with three main problems: (1) to construct manageable bodies with perfectly black properties; (2) to devise radiation detectors with adequate sensitivity, and (3) to find ways of extending the measurements over large frequency domains. Forty years of experimentation had to go by before the data were sufficient to answer Kirchhoff's question.

Kirchhoff derived Eq. (1) by showing that its violation would imply the possibility of a "perpetuum mobile" of the second kind. The novelty of his theorem was not so much its content as the precision and generality of its proof, based exclusively on the still-young science of thermodynamics. A quarter of a century passed before the next theoretical advance in blackbody radiation came about.

In 1879 Josef Stefan (1835–1893) conjectured on experimental grounds that the total energy radiated by a hot body varies with the fourth power of the absolute temperature, (Stefan, 1879). This statement is not true in its generality. The precise formulation was given in 1884 when Ludwig Boltzmann (1844–1906), [then a professor of experimental physics in Graz (Austria)], proved theoretically that the strict T^4 law holds—and only holds—for bodies which are black, (Boltzmann, 1884). His proof involved again thermodynamics, but combined this time with a still younger branch of theoretical physics: The electromagnetic theory of James Clerk Maxwell (1831–1879).

For the case of Hohlraumstrahlung the radiation is homogeneous, isotropic and unpolarized so that

$$J(\nu, T) = (c/8\pi)\rho(\nu, T)\,. \tag{2}$$

$\rho(\nu, T)$, the spectral density, is the energy density per

[6]"...dû à quelque action encore inconnue, 'cui theoriae lumen nundum accesserit'."

[7]The present value is 43 sec per century.

unit volume for frequency ν. In this case the Stefan-Boltzmann law reads (V is the volume of the cavity)

$$E(T) \equiv V \int \rho(\nu, T) d\nu = a V T^4 . \tag{3}$$

This law was the very first thermodynamical consequence derived from Maxwell's theorem according to which the numerical value of the radiation pressure equals one-third of the energy per unit volume. When in 1893 Wilhelm Wien (1864–1928) proved his displacement law (Wien, 1893)

$$\rho(\nu, T) = \nu^3 f(\nu/T) , \tag{4}$$

one had come as far as is possible on the basis of thermodynamics and general electromagnetic theory. [Proofs of Eqs. (3), (4) are found in standard texts.]

Meanwhile, ever since the 1860s proposals for the correct form of ρ had begun to appear. All these guesses may be forgotten except for one, Wien's exponential law, proposed in 1896, (Wien, 1896):

$$\rho = \alpha \nu^3 e^{-\beta\nu/T} . \tag{5}$$

Experimental techniques had sufficiently advanced by then to put this formula to the test. This was done by Friedrich Paschen (1865–1947) from Hannover whose measurements (very good ones) were made in the near-infrared, $\lambda = 1 - 8\mu$ (and $T = 400 - 1600$ °K). He published his data in January 1897. His conclusion: "It would seem very difficult to find another function [of ν and T, Eq. (5)] which represents the data with as few constants," (Paschen, 1897). For a brief period it appeared that Wien's law was the final answer. But then, in the year 1900, this conclusion turned out to be premature and the correct response to Kirchhoff's challenge was found. Two factors were decisive. One, a breakthrough in experimental techniques in the far infrared. The other, the persistence and vision of Max Karl Ernst Ludwig Planck (1858–1947).

It happened in Berlin. At the Physikalisch Technische Reichsanstalt, at that time probably the world's best equipped physics laboratory, two teams were independently at work on blackbody radiation experiments. The first of these, Otto Lummer (1860–1925) and Ernst Pringsheim (1859–1917), had tackled the problem in an as yet unexplored wave length region, $\lambda = 12 - 18\mu$, (and $T = 300 - 1650$ °K). In February 1900 they stated their conclusion: Wien's law fails in that region,[8] (Lummer and Pringsheim, 1900). The second team, consisting of Heinrich Rubens (1865–1922) and Ferdinand Kurlbaum (1857–1927), moved even farther into the infrared: $\lambda = 30 - 60\mu$, (and $T = -200 - 1500$ °C). They arrived at the same conclusion (Rubens and Kurlbaum, 1900).

I need to say more about the latter results but I should like to comment first on the role of experiment in the discovery of the quantum theory. The Rubens–Kurlbaum paper is a classic. The work of these authors as well as that of Paschen and of Lummer and Pringsheim was of a pioneering nature. By the middle of the nineteenth century wavelengths had been measured up to $\lambda \sim 1.5\mu$. Progress was slow in the next forty years, as demonstrated by a question raised by Samuel Pierpont Langley (1834–1906) in a lecture given in 1885 before the AAAS meeting in Ann Arbor: "Does [the] ultimate wavelength of 2.7μ which our atmosphere transmits correspond to the lowest [frequency] which can be obtained from any terrestrial source?" (Langley, 1886). The great advance came in the 1890s. The first sentence of the first paper in the first issue of the Physical Review reads as follows: "Within a few years the study of obscure radiation has been greatly advanced by systematic inquiry into the laws of dispersion of the infrared rays." This was written in 1893, by Ernest Fox Nichols (1869–1924). At about that time new techniques were developed which culminated in the "Reststrahlen" ("residual rays") method of Rubens and Nichols (1897): one eliminates short wavelengths from a beam of radiation by subjecting it to numerous reflections on quartz or other surfaces. This procedure leads to the isolation of the long wavelengths in the beam. These experimental developments are of fundamental importance for our main subject, the quantum theory, since they were crucial to the discovery of the blackbody radiation law.

The paper by Rubens and Kurlbaum was presented to the Prussian Academy on October 25, 1900. Figure 2 shows some of the measured points they recorded[9] and some theoretical curves with which they compared their findings. One of these was the Wien curve, which did not work. Neither did a second curve proposed by Lord Rayleigh. (I return to Rayleigh's work in Sec. II.B.) I shall leave aside the other two comparison curves which they drew and turn to the all important "fifth formula, given by Herr M. Planck after our experiments had already been concluded .... [and which] reproduces our observations [from −188° to 1500 °C] within the limits of error" (Rubens and Kurlbaum, 1900).

Kirchhoff had moved from Heidelberg to Berlin to take the chair in theoretical physics. After his death this position was offered to Boltzmann who declined. Then Heinrich Hertz (1857–1894) was approached; he also declined. The next candidate was Planck to whom the offer of extraordinarius (associate professor) was made. Planck accepted and was soon promoted to full professor. His new position brought him in close proximity to the experimental developments outlined above. This nearness was to be one of the decisive factors in the destiny of this most unusual man.

Planck most probably[10] discovered his law in the early evening of Sunday, October 7. Rubens and his wife had called on the Plancks on the afternoon of that day. In the course of the conversation Rubens mentioned to

[8]There had been earlier indications of deviations from Wien's law, but these were not well documented.

[9]These refer to observations at $\lambda = 51.2\mu$. This wavelength was isolated by multiple reflections off rock salt. The blackbody radiation intensity is plotted as a function of T. (Recall that after multiple reflection those specific frequencies predominantly survive which correspond to the ionic vibrations in the crystal lattice chosen as reflector.)

[10]Here I rely on the obituary of Rubens by Georg Hettner (1922) (himself an experimental expert on blackbody radiation). Hettner's account differs slightly from the recollections which Planck himself wrote (1958) in his late eighties.

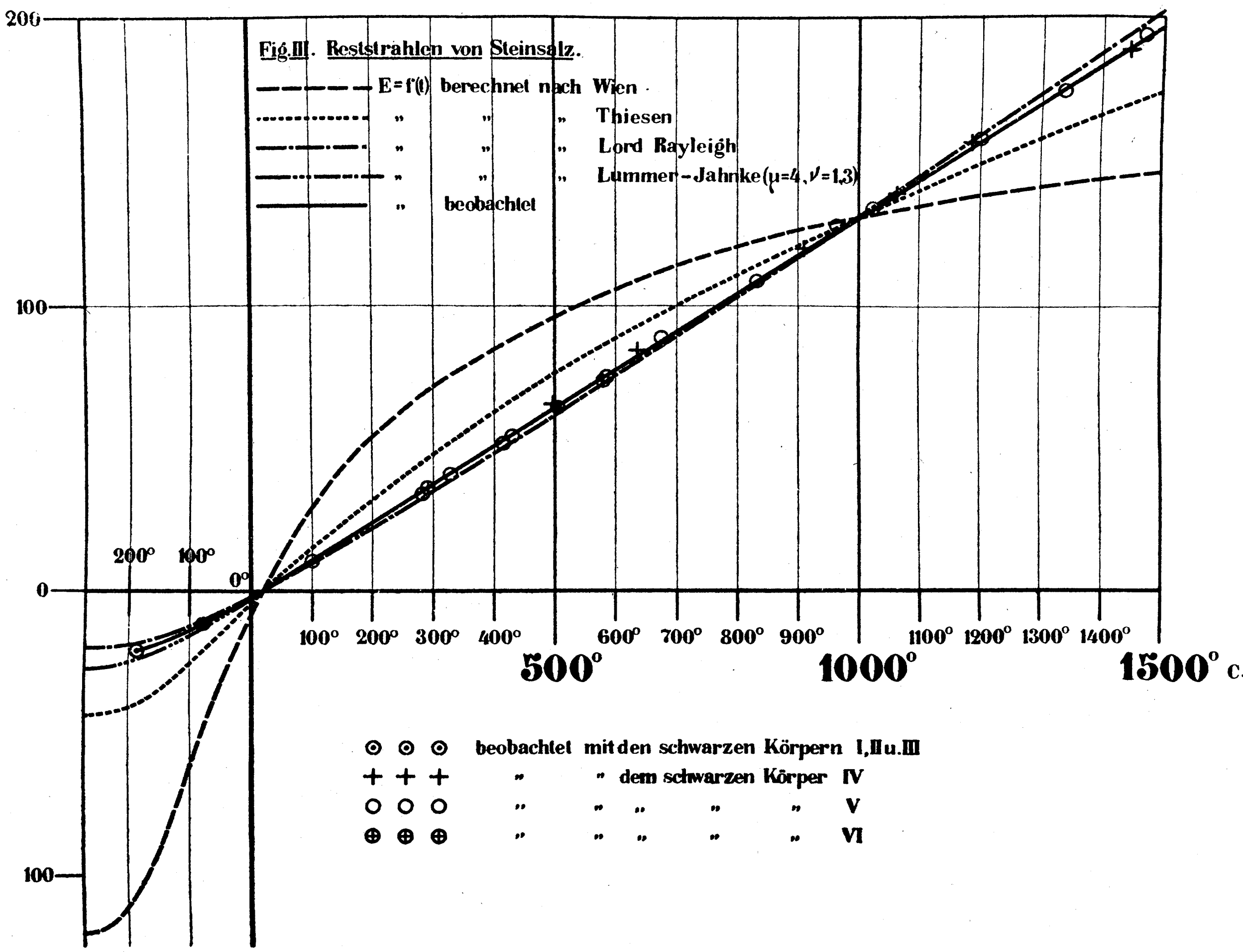

FIG. 2. Sample of the Rubens–Kurlbaum data which led Planck to guess his radiation formula (Rubens and Kurlbaum, 1900). ρ is plotted versus T for $\lambda = 51,2\mu$. ("berechnet nach" means "computed after", "beobachtet" means "observed".) The curves marked "Wien" and "Lord Rayleigh" refer to best fits to the Eqs. (5), (17), respectively. The curves marked "Thiesen" and "Lummer-Jahnke" refer to theoretical proposals which are not discussed in the present paper. Planck's formula is not yet plotted.

Planck that he had found $\rho(\nu, T)$ to be proportional to T for small ν. Planck went to work after the visitors had left and found an interpolation between this result and Wien's law, Eq. (5). He communicated his formula by postcard to Rubens, that same evening, and stated it publicly (Planck, 1900a) in a discussion remark on October 19, following the presentation of a paper by Kurlbaum. Expressed in notations introduced by Planck two months later, he proposed that

$$\rho(\nu, T) = \frac{8\pi h \nu^3}{c^3} \frac{1}{e^{h\nu/kT} - 1} \quad . \tag{6}$$

Equation (6) contains Wien's law of 1896:

$$\rho(\nu, T) = \frac{8\pi h \nu^3}{c^3} e^{-h\nu/kT} \text{ for } \frac{h\nu}{kT} \gg 1 \, , \tag{7}$$

which is indeed correct in the quantum regime $h\nu/kT \gg 1$, a condition which is well satisfied in Paschen's experiment (1897) mentioned earlier, ($h\nu/kT \simeq 15$ for $T = 1000$ °K and $\lambda = 1\mu$). Strange as it may sound, the quantum theory was discovered only after classical deviations from the quantum regime had been observed in the far infrared.

It would do grave injustice to Planck if I left the reader with the impression that Planck's discovery was exclusively the result of interpolating experimental data. For years it had been his ambition to derive the correct radiation law from first principles. Thus the rapidity of his response to Rubens' remark is less surprising than the correctness of his answer. I shall discuss elsewhere Planck's earlier research (cf. also Klein, 1977) and shall not even describe here how he made his guess. However, it is very important for what follows to give a brief account of Planck's activities from October to December 1900, the heroic period in his life. It is necessary to do so for an understanding not only of Einstein's starting point in 1905 (Sec. II.B) but also of the subsequent reactions to the light-quantum hypothesis (Sec. IV.A).

Even if Planck had stopped after October 19, he would forever be remembered as the discoverer of the radiation law. It is a true measure of his greatness that he went further. He wanted to interpret Eq. (6). That made him the discoverer of the quantum theory. I shall briefly outline the three steps he took (Planck, 1900).

(1) The electromagnetic step. This concerns a result which Planck (1900c) had obtained some time earlier. Consider a linear oscillator with mass m and charge e in interaction with a monochromatic periodic electric field (with frequency ω) in the direction of its motion. The equation of motion is (as Planck was the first to show)

$$m\ddot{x} + fx - \frac{2e^2}{3c^3}\dddot{x} = eF\cos 2\pi\,\omega t\,. \tag{8}$$

Let ν denote the frequency of the free oscillator, $f/m = (2\pi\nu)^2$. Consider in particular the case in which the radiation damping due to the $\dddot{x}$ term is very small, that is, $\gamma \ll \nu$ where $\gamma = 8\pi^2 e^2\nu^2/3mc^3$. Then one may approximate $\dddot{x}$ by $-(2\pi\nu)^2\dot{x}$. The solution of Eq. (8) can be written[11] as $x = C\cos(2\pi\omega t - \alpha)$. One can readily solve for C and α. The energy E of the oscillator equals $m(2\pi\nu)^2C^2/2$ and one finds

$$E = \frac{e^2F^2}{2m}\,\frac{1}{4\pi(\nu-\omega)^2+\gamma^2}\,. \tag{9}$$

Next let the electric field consist of an incoherent isotropic superposition of frequencies in thermal equilibrium at temperature T. In that case the equilibrium energy U of the oscillator is obtained by replacing the electric field energy density $F^2/2$ in Eq. (9) by $4\pi\rho(\omega,T)d\omega/3$ and integrating over ω:

$$U = \frac{4\pi e^2}{3m}\int\frac{\rho(\omega,T)d\omega}{4\pi(\nu-\omega)^2+\gamma^2}\,.$$

Since γ is very small, the response of the oscillator is maximal if $\omega = \nu$. Thus we may replace $\rho(\omega,T)$ by $\rho(\nu,T)$ and extend the integration from $-\infty$ to $+\infty$. This yields

$$\rho(\nu,T) = \frac{8\pi\nu^2}{c^3}\,U(\nu,T)\,, \tag{10}$$

an equation to which we shall often refer in what follows.

(2) The thermodynamic step. Planck concluded from Eq. (10) that it suffices to determine U in order to find ρ. (There is a lot more to be said about this seemingly innocent statement; see Sec. II.B). Working backward from Eqs. (6) and (10) he found U. Next he determined the entropy S of the linear oscillator by integrating $T\,dS = dU$, where T is to be taken as a function of U (for fixed ν). This yields

$$S = k\left[\left(1+\frac{U}{h\nu}\right)\ln\left(1+\frac{U}{h\nu}\right) - \frac{U}{h\nu}\ln\frac{U}{h\nu}\right]. \tag{11}$$

Equation (6) follows if one can derive Eq. (11).

(3) The statistical step. I should rather say: what Planck held to be a statistical step. Consider a large number N of linear oscillators, all with frequency ν. Let $U_N = NU$ and $S_N = NS$ be the total energy and entropy of the system respectively. Put $S_N = k\ln W_N$, where W_N is the thermodynamic probability. Now comes the quantum postulate.

U_N is supposed to be made up out of *finite* energy elements ε: $U_N = P\varepsilon$, where P is a large number. Define W_N to be the number of ways in which the P *indistinguishable* energy elements can be distributed over N *distinguishable* oscillators. Example: for $N=2, P=3$ the partitions are $(3\varepsilon,0)$; $(2\varepsilon,\varepsilon)$; $(\varepsilon,2\varepsilon)$; $(0,3\varepsilon)$. In general

$$W_N = \frac{(N-1+P)!}{P!\,(N-1)!}\,. \tag{12}$$

Insert this in $S_N = k\ln W_N$, use $P/N = U/\epsilon$, $S_N = NS$ and apply the Stirling approximation. This gives

$$S = k\left[\left(1+\frac{U}{\varepsilon}\right)\ln\left(1+\frac{U}{\varepsilon}\right) - \frac{U}{\varepsilon}\ln\frac{U}{\varepsilon}\right]. \tag{13}$$

It follows from Eqs. (4), (10), and $T\,dS = dU$ that S is a function of U/ν only. Therefore

$$\varepsilon = h\nu\,. \tag{14}$$

Thus one recovers Eq. (11). And that is how the quantum theory was born. This derivation was first presented (1900b) on December 14, 1900.

From the point of view of physics in 1900 the logic of Planck's electromagnetic and thermodynamic steps was impeccable, but his statistical step was wild. The latter was clearly designed to argue backwards from Eqs. (12)–(14) to Eq. (11). In 1931 Planck referred to it as "an act of desperation ... I had to obtain a positive result, under any circumstances and at whatever cost" (Hermann, 1969, p. 32). Actually there were two desperate acts rather than one. First there was his unheard-of step of attaching physical significance to finite "energy elements" [Eq. (14)]. Secondly there was his equally unheard-of counting procedure given by Eq. (12). In Planck's opinion (1901a) "the electromagnetic theory of radiation does not provide us with any starting point whatever to speak of such a probability [W_N] in a definite sense". This statement is of course incorrect. As will be discussed in Sec. II.B, the classical equipartition theorem could have given Planck a quite definite method to determine all thermodynamical equilibrium quantities he was interested in—but would not have given him the answer he desired to derive. Planck correctly referred to Eq. (12) as a hypothesis. "Experience will prove whether this hypothesis is realized in nature" (1901a). In his papers Planck alluded to the inspiration he had derived from Boltzmann's probabilistic methods. But in Boltzmann's case the question was to determine the most probable way in which a fixed number of *distinguishable* gas molecules with fixed total energy are distributed over cells in phase space. (Boltzmann's method is reviewed in Sec. VI.A below.) The corresponding combinatorial problem had nothing to do with Planck's counting of partitions of indistinguishable objects, the energy elements.[12] (Planck's procedure is actually a precursor of Bose–Einstein counting.) The only justification for Planck's two desperate acts was that they led to the answer he wanted. His reasoning was mad, but his madness has that divine quality which only the greatest transitional figures can bring to science. It cast Planck, conservative by in-

[11]For more details see a review by Pauli (1964) pp. 602–607.

[12]Planck may have found the formulae he used in Boltzmann's work, however (Klein, 1977).

clination, into the role of a reluctant revolutionary. Deeply rooted in nineteenth-century thinking and prejudice, he made the first conceptual break that has made twentieth-century physics look so discontinuously different from the preceding era. Although there have been other major innovations in physics since December 1900, the world has not seen since a figure like Planck. Einstein venerated him.[13]

In all the history of physics there has been no period of transition as abrupt, as unanticipated, and over as wide a front as the decade 1895–1905. The discovery of radioactivity, of the electron, of the quantum theory, and of the special relativity theory are its four main themes. When special relativity appeared, it was at once "all there." There never was an "old" theory of special relativity. It took more than a quarter of a century for the quantum theory to be "all there" (at least in its nonrelativistic aspects). It is remarkable that the old quantum theory would originate from a question as complex as blackbody radiation. From 1859–1926 this problem remained at the frontier of theoretical physics, first in thermodynamics, then in electromagnetism, then in the old quantum theory, and finally in quantum statistics.

From the experimental point of view, blackbody radiation had in essence been understood by 1900. The subsequent years only saw refinements of the early results. The quality of the work by the pioneers can best be illustrated by the following numbers. In 1901 Planck (1901b) obtained from the available data the value $h = 6.55\times 10^{-27}$ ergsec for his constant. The modern value is 6.63×10^{-27}. For the Boltzmann constant he found $k = 1.34\times 10^{-16}$ erg $^{\circ}K^{-1}$; the present best value is 1.38×10^{-16}. Using his value for k he could determine Avogadro's number N from the relation $R = Nk$, where R is the gas constant. Then from Faraday's law for univalent electrolytes: $F = Ne$ he obtained (Planck, 1901a) the value $e = 4.69\times 10^{-10}$ esu. The present best value is 4.80×10^{-10}. At the time of Planck's determination of e, J. J. Thomson (1899) had measured the charge of the electron with the result $e = 6.5\times 10^{-10}$! Not until 1908, when the charge of the α particle was found to be 9.3×10^{-10} (Rutherford and Geiger, 1908), was it realized how good Planck's value for e was.

Nothing further happened in quantum physics after 1901 until Einstein proposed the light-quantum hypothesis.

B. Einstein on Planck: 1905. The Rayleigh-Einstein-Jeans law

The first sentence on the quantum theory published by Einstein was written in the month of March, in the year 1905. It is the title of his first paper on light-quanta (1905a): "On a heuristic point of view concerning the generation and conversion of light." Webster's Dictionary contains the following definition of the term heuristic: "Providing aid and direction in the solution of a problem but otherwise unjustified or incapable of justification." Toward the end of this paper (Sec. X.D) I shall mention the last sentence published by Einstein on scientific matters, also written in March, exactly one half-century later. It also deals with the quantum theory. It has one thing in common with the opening sentence mentioned above. They both express Einstein's view that the quantum theory is provisional in nature. The persistence of this opinion of Einstein is one of the main themes of the present paper. Whatever one may think of the status of the quantum theory in 1955, in 1905 this opinion was, of course, entirely justified.

Einstein completed his first scientific paper (on capillary phenomena) on December 13, 1900, one day before Planck (1900b) presented the first paper on the quantum theory. He had finished his academic studies at the ETH in Zürich in the preceding August. The next five years brought many changes in his life. To begin with he had trouble finding a job. Then, in June 1902, he started work at the Patent Office in Bern and in that same month submitted his first paper (Einstein, 1902) on the statistical foundations of thermodynamics. Einstein had marriage plans to which his parents were strongly opposed. Only on his deathbed did his father [Hermann Einstein (1847–October 1902)] give his consent.[14] The marriage[15] took place in January 1903. His first son was born in May 1904. By that time two more papers (1903, 1904) on the foundations of statistical mechanics had been submitted. Then came 1905, the year in which Einstein received his doctor's degree in Zürich[16] and in a series of papers revolutionized physics. For the purpose of the present account I shall of course focus on the light-quantum paper (1905a) submitted March 17.

In his autobiographical notes, published in 1949, Einstein recalled that his concern with Planck's pioneering work began shortly after 1900. "All my attempts ... to adapt the theoretical foundations of physics to this [new type of] knowledge failed completely. It was as if the ground had been pulled out from under one, with no firm foundation to be seen anywhere ..." (Einstein, 1949a). What was the meaning of Planck's derivation of Eq. (6)? "The imperfections of [that derivation] remained at first hidden, which was most fortunate for the development of physics" (1949a). Einstein's 1905 paper opens with a section entitled "On a difficulty concerning the theory of blackbody radiation" in which he put these imperfections in sharp focus.

His very simple argument was based on two solid con-

[13]In September 1918 Einstein proposed Planck for the Nobel Prize for 1919, too late as it happened, since Planck received it later in 1918.

[14]I learned these and other details of Einstein's life from a biographical sketch written in 1924 by his sister Maja Winteler-Einstein (1881–1951). This biography is in the Einstein Archives in Princeton.

[15]To Mileva Maric (1876–1948). They had two sons, Hans Albert (1904–1973) and Eduard (1910–1965). (There were two sons from Hans Albert's first marriage and also one adopted daughter. The younger son died at age five.) Einstein and his wife separated in 1914 and divorced in February 1919. He remarried in June 1919. His second wife, Elsa Einstein Lowenthal (1874–1936) was a widowed cousin of his. She had two daughters, Ilse and Margot, by her first marriage.

[16]His thesis was on "A new determination of the dimensions of molecules." It was also published in Ann. Phys. (Leipz.) (Einstein, 1906a).

sequences of classical theory. The first of these was Planck's Eq. (10). Recall that the quantity U in that equation is the equilibrium energy of a one-dimensional harmonic oscillator. Einstein's second ingredient was the equipartition law of classical statistical mechanics according to which

$$U(\nu, T) = (R/N)T , \tag{15}$$

where R is the gas constant, and N Avogadro's number. $R/N = k$, the Boltzmann constant. (For a number of years Einstein did not use the symbol k in his papers.) From Eqs. (10) and (15) Einstein obtained

$$\rho(\nu, T) = \frac{8\pi\nu^2}{c^3}\frac{R}{N}T \tag{16}$$

and went on to note that this classical relation is in disagreement with experiment and has the disastrous consequence that $a = \infty$, where a is the Stefan–Boltzmann constant given in Eq. (3).

"If Planck had drawn this conclusion, he would probably not have made his great discovery" Einstein said later (1949a). Planck had obtained Eq. (10) in 1897. At that time the equipartition law had been known for almost thirty years. (See also Sec. III.) During the 1890s Planck had made several errors in reasoning before he arrived at his radiation law, but none as astounding and of as great a historical significance as his fortunate failure to be the first to derive Eq. (16). This omission is, no doubt, related to Planck's decidedly negative attitude (before 1900) to Boltzmann's ideas on statistical mechanics.

Equation (16), commonly known as the Rayleigh–Jeans law, has an interesting and rather hilarious history, as may be seen from the following chronology of events.

(i) In June 1900 there appeared a brief paper (1900a) by Lord Rayleigh (1842–1919). It contains for the first time the suggestion to apply to radiation "the Maxwell-Boltzmann doctrine of the partition of energy" (i.e., the equipartition theorem). From this doctrine Rayleigh went on to derive the relation $\rho = c_1\nu^2T$ but did not evaluate the constant c_1. It should be stressed that Rayleigh's derivation of this result had the distinct advantage of dispensing with the material oscillators altogether![17] Rayleigh also realized that this relation should be interpreted as a limiting law: "The suggestion is then that [$\rho = c_1\nu^2T$], rather than [Wien's law Eq. (5)], may be the proper form *when* [T/ν] is *great*" (my italics).[18] In order to suppress the catastrophic high-frequency behavior, he introduced next an *ad hoc* exponential cut-off factor and proposed the overall radiation law

$$\rho(\nu, T) = c_1\nu^2 T e^{-c_2\nu/T} . \tag{17}$$

This expression became known as the Rayleigh law. Already in 1900, Rubens and Kurlbaum (and also Lummer and Pringsheim) found this law wanting, as is seen in Fig. 2.

Thus the experimentalists close to Planck were well aware of Rayleigh's work. One wonders whether or not Planck himself knew of this important paper which appeared half a year before he proposed his own law. Whichever may be the case, in 1900 Planck did not refer to Rayleigh's contribution.[19]

(ii) Einstein gives the derivation of Eq. (16) discussed previously. His paper is submitted March 17, 1905 and appears June 9 of that year.

(iii) In a letter to *Nature* (submitted May 6, published May 18, all in 1905) Rayleigh (1905a) returns to his ν^2T law and now computes c_1. His answer for c_1 is off by a factor 8.

(iv) On June 7, 1905, James Hopwood Jeans (1877–1946) adds a postscript to a completed paper in which he corrects Rayleigh's oversight. The paper appears a month later (Jeans, 1905a). In July 1905 Rayleigh (1905b) acknowledges Jeans' contribution.

It follows from this chronology (not that it matters much) that the Rayleigh–Jeans law ought properly to be called the Rayleigh–Einstein–Jeans law.

The purpose of this digression about Eq. (16) is not merely to note who said what first. Of far greater interest is the role which this equation played in the early reactions to the quantum theory. From 1900–1905 Planck's radiation formula was generally considered to be neither more nor less than a successful representation of the data (cf. Benz, 1975). Only in 1905 did it begin to dawn, and then only on a few, that a crisis in physics was at hand (Einstein, 1913). The failure of the Rayleigh–Einstein–Jeans law was the cause for this turn of events.

Rayleigh's position on the failure of Eq. (16) as a universal law was that "we must admit the failure of the law of equipartition in these extreme cases" [i.e., at high frequencies] (1905a). Jeans (1905b) took a different view: The equipartition law is correct but "the supposition that the energy of the ether is in equilibrium with that of matter is utterly erroneous in the case of ether vibrations of short wavelength under experimental conditions". Thus Jeans considered Planck's constant h as a phenomenological parameter well suited as a help to fit data but devoid of fundamental significance. The debate—nonequilibrium versus failure of equipartition—continued for a number of years (Hermann, 1969). The issue was still raised at the first Solvay Congress in 1911, but by then the nonequilibrium view no longer aroused much interest.

[17]Planck derived his radiation law in a circuitous way via the equilibrium properties of his material oscillators. He did so because of his simultaneous concern with two questions: How is radiative equilibrium established? What is the equilibrium distribution? The introduction of the material oscillators would, Planck hoped, show the way to answer both questions. Rayleigh wisely concentrated on the second question only. He considered a cavity filled with "aetherial oscillators" *assumed* to be in equilibrium. This enabled him to apply equipartition directly to these radiation oscillators.

[18]This same observation was also made independently by Einstein in 1905.

[19]Neither did Lorentz, who in 1903 gave still another derivation of the ν^2T law (Lorentz, 1903). The details need not concern us. It should be noted that Lorentz also gave the correct answer for the constant c_1. However, he did not derive the expression for c_1 directly. Rather he found c_1 by appealing to the long-wavelength limit of Planck's law.

C. The light-quantum hypothesis and the heuristic principle.

Let us return to Einstein. In 1905 his position was as follows. Equation (6) agrees with experiment but not with existing theory. Equation (16) agrees with existing theory but not with experiment. He therefore set out to study blackbody radiation in a way "which is not based on a picture of the generation and propagation of radiation"—that is, by not making use of Planck's Eq. (10). But then something had to be found to replace that equation. For this purpose Einstein chose to reason "im Anschluss an die Erfahrung," "in connection with experiment." The experimental information he made use of was the validity of Wien's law [Eq. (5)] in the region of large ν/T. He extracted the light-quantum postulate from this law by drawing an analogy between radiation in the Wien regime and a gas of (classical) noninteracting point particles, often called an ideal Boltzmann gas. Specifically he made use of the volume dependence of the entropy for such a gas. This well known relation between entropy and volume can be derived in the following way from the second law of thermodynamics and the ideal gas law.

Consider a system in equilibrium described by its pressure p, volume v, and temperature T. According to the second law an infinitesimal reversible change of such a system obeys the relation

$$T\ dS = c_v\ dT + \left[\left(\frac{\partial U}{\partial v}\right) + p\right]\ dv\ . \tag{18}$$

c_v the specific heat, S the entropy, and U the internal energy all are in general functions of v and T. From

$$\partial\,\frac{(\partial S/\partial v)}{\partial T} = \partial\,\frac{(\partial S/\partial T)}{\partial v}$$

and from Eq. (18) it follows that

$$\partial p/\partial T - \frac{(p + \partial U/\partial v)}{T} = 0\ .$$

For an ideal Boltzmann gas this last relation reduces to $\partial U/\partial v = 0$ since in this case $Npv = nRT$, where n is the number of molecules in the gas. In turn $\partial U/\partial v = 0$ implies that c_v is a function of T only,[20] hence $T\ dS(v, T) = c_v(T)\ dT + nRT\ dv/Nv$. Consider a finite reversible change at constant T in which the gas molecules in the volume v_0 are confined to a subvolume v. Then the entropy change for an ideal Boltzmann gas is given by

$$S(v, T) - S(v_0, T) = \frac{R}{N}\ln\left(\frac{v}{v_0}\right)^n\ . \tag{19}$$

Einstein derived Eq. (19) by a different method on which I shall comment in the next subsection D.

Now back to the radiation problem. Let $\phi(\nu, T)d\nu$ be the entropy density per unit volume in the frequency interval between ν and $\nu + d\nu$. Then (ρ is again the spectral density)

$$\frac{\partial\phi}{\partial\rho} = \frac{1}{T}\ . \tag{20}$$

Assume that Wien's law [Eq. (5)] is applicable. Then

$$\phi = \frac{-\rho}{\beta\nu}\left(\ln\frac{\rho}{\alpha\nu^3} - 1\right)\ . \tag{21}$$

Let the radiation be contained in a volume v. $S(\nu, v, T) = \phi v\, d\nu$ and $E(\nu, v, T) = \rho v d\nu$ are the total entropy and energy in that volume in the interval ν to $\nu + d\nu$, respectively. In the Wien regime S follows trivially from Eq. (21). One finds in particular that

$$S(\nu, v, T) - S(\nu, v_0, T) = \frac{E}{\beta\nu}\ln\left(\frac{v}{v_0}\right) = \frac{R}{N}\ln\left(\frac{v}{v_0}\right)^{NE/R\beta\nu}\ . \tag{22}$$

Compare Eqs. (22) and (19) and we have Einstein's

Light-quantum hypothesis. "Monochromatic radiation of low density [(i.e.,) within the domain of validity of the Wien radiation formula] behaves in thermodynamic respect as if it consists of mutually independent energy quanta of magnitude $R\beta\nu/N$" ($\beta = h/k, R/N = k, R\beta\nu/N = h\nu$).

It is astonishingly simple. Einstein phrased this hypothesis in the form of a theorem. It was a hypothesis nevertheless, since it was based on Wien's law which itself still needed proof from first principles.

Einstein (1905a) made next a daring step which was to gain him the Nobel prize in 1921.

The heuristic principle. "If, in regard to the volume dependence of the entropy, monochromatic radiation (of sufficiently low density) behaves as a discrete medium consisting of energy quanta of magnitude $R\beta\nu/N$, then it is suggestive to inquire whether also the laws of the generation and conversion of light are constituted as if light were to consist of energy quanta of this kind."

In other words: The light-quantum hypothesis is an assertion about a quantum property of free electromagnetic radiation; the heuristic principle is a tentative extension of these properties of light to the interaction between light and matter.[21]

I shall continue next with a discussion of certain matters of principle contained in Einstein's papers of 1904, 1905, and 1906. In part F of this section I shall turn to the application of the heuristic principle to the photo electric effect.

D. Three remarks on Einstein's statistical physics

(1) Einstein's contributions to physics fall under three main headings: statistical physics, quantum theory, and relativity theory. His researches in statistical physics and on the quantum theory are strongly interrelated. This will become progressively clear in the further discussion of Einstein's work in the years

[20]Actually c_v does not depend on T either for an ideal Boltzmann gas, but we do not need this fact for the present reasoning.

[21]The need for a distinction between the light-quantum hypothesis and the heuristic principle was emphasized to me by R. Jost. I am grateful to him for an illuminating discussion on this subject. See also Jost's own contribution (1979) to the Einstein Centennial meeting held in Zürich, February 24, 1979.

1905–1925. Moreover, when we come to Einstein's role as a transitional figure (Sec. VII) it will become evident that wave mechanics is an offspring of statistical physics in a sense to be described.

As Klein has noted, the first intimations of this interplay between statistical methods and quantum arguments in Einstein's work are found already before 1905. For details on the early influences of thermodynamics on Einstein's thought the reader is referred to Klein's papers (1967, 1979). Here I shall confine myself to a few brief remarks on this subject.

The first of these concerns the following question. What led Einstein to combine Wien's law with—of all things—the volume dependence of thermodynamic quantities? I have no firm answer, but it seems relevant to note that volume dependence also plays a role in the first published remarks by Einstein on radiation in thermal equilibrium. These date from 1904 and appeared in his early work on fluctuation phenomena (1904).

Consider a system which may have various energies $E_1, E_2, \ldots$ in thermal equilibrium with a large second system at temperature T. The equilibrium energy of the first system is given by

$$\langle E\rangle = \frac{\sum E_i e^{-\beta E i}}{\sum e^{-\beta E i}}, \beta = 1/kT . \tag{23}$$

In 1904 Einstein deduced a formula for the mean square energy fluctuation $\langle \epsilon^2\rangle \equiv \langle (E - \langle E\rangle)^2\rangle = \langle E^2\rangle - \langle E\rangle^2$ of the first system, namely

$$\langle \varepsilon^2\rangle = -\frac{\partial\langle E\rangle}{\partial\beta} = kT^2\frac{\partial\langle E\rangle}{\partial T} . \tag{24}$$

He then introduced a criterion for fluctuations to be large:

$$\xi \equiv \frac{\langle\varepsilon^2\rangle}{\langle E\rangle^2} \sim 1 . \tag{25}$$

This relation is not satisfied by an ideal Boltzmann gas under normal conditions since $\langle E\rangle = nkT/2$ so that $\xi = 0(n^{-1})$, independent of the volume. Einstein went on to note that ξ can be of order unity for one kind of system: blackbody radiation. In that case $\langle E\rangle = avT^4$, [Eq. (3)], hence $\xi = 4k/avT^3$. T is proportional to the inverse of $\lambda_{\max}$, the wavelength at which the spectral distribution reaches its maximum. Thus volume dependence is important: for given T, ξ can get large if $\lambda^3_{\max}/v$ is large, hence if v is small.

In 1904 Einstein was mainly interested in Eq. (24) because of the new vistas it opened for experimental determinations of k (and therefore of Avogadro's number N) in model-independent ways. These interests were to branch in the following year, 1905, when the macroscopic fluctuations typical for Brownian motion (Einstein, 1905b) gave him N while volume dependences led him to light-quanta.

Equation (24) plays an important role in Einstein's papers of 1909, 1917, and 1925 on the quantum theory. When Einstein first derived this equation, he did not know that Josiah Willard Gibbs (1839–1903) had done so before him (Gibbs, 1902). Some years later Einstein wrote (1911a) that he would have reduced his early published papers on the molecular theory of heat to a few remarks if he had been aware of Gibbs's book. However, in 1909 Einstein (1909b) gave a new derivation of Eq. (24), this one all his own. It is characteristic that his statistical argument would appear in a paper principally devoted to the quantum theory.

Einstein argued as follows. Consider a large system with volume V in equilibrium at temperature T. Divide V into a small subvolume V_0 and a rest V_1, $V = V_0 + V_1$, $V_0 \ll V_1$. The fixed total energy is likewise divided, $E = E_0 + E_1$. *Assume* that also the entropy is additive:

$$S = S_0 + S_1 . \tag{25}$$

Suppose that E_0, E_1 deviate by amounts $\Delta E_0, \Delta E_1$ from their respective equilibrium values. Then

$$\Delta S = \left[\frac{\partial S_0}{\partial E_0}\right]\Delta E_0 + \left[\frac{\partial S_1}{\partial E_1}\right]\Delta E_1 + \frac{1}{2}\left[\frac{\partial^2 S}{\partial E_0^2}\right](\Delta E_0)^2 + \frac{1}{2}\left[\frac{\partial^2 S}{\partial E_1^2}\right](\Delta E_1)^2 + \ldots \tag{26}$$

where the expressions in [ ] refer to equilibrium values. The first-order terms cancel since $\Delta E_0 = -\Delta E_1$ (energy conservation) and $[\partial S_0/\partial E_0] = [\partial S_1/\partial E_1]$ (equilibrium). Furthermore $[\partial^2 S_0/\partial E_0^2] = -1/c_0T^2$ and $[\partial^2 S_1/\partial E_1^2] = -1/c_1T^2$, where c_0, c_1 are the respective heat capacities at constant volume. $c_1 \gg c_0$ since $V_1 \gg V_0$. Thus Eq. (26) becomes

$$\Delta S = \Delta S_0 = -\frac{(\Delta E_0)^2}{2c_0T^2} . \tag{27}$$

Next Einstein applied the relation $S_0 = k\ln W_0$ to the subsystem and reinterpreted this equation to mean that W_0 is the probability for the subsystem to have the entropy S_0 (at a given time). Hence

$$W_0 = \overline{W}_0 e^{\Delta S/k} , \tag{28}$$

where $\overline{W}_0$ is the equilibrium value of W_0. Equations (27) and (28) show that W_0 is Gaussian in ΔE_0. Denote (as before) the mean square deviation of this distribution by $\langle\varepsilon^2\rangle$. Then $\langle\varepsilon^2\rangle = kc_0T^2$ which is again Eq. (24).

This derivation is typical for Einstein's approach. Instead of reasoning from the microscopic to the macroscopic ($W \to S$) he often argued in the inverse direction (and did so to great advantage). In Sec. II.G I shall note that this last derivation was briefly challenged at a later time.

(2) The fact that Einstein devoted two and a half pages of his light-quantum paper to a derivation of Eq. (22) from a molecular statistical point of view illustrates again how strongly statistical and quantum theory are interwoven in Einstein's work. It also demonstrates that in those days statistical mechanics was still a quite young discipline.[22] Earlier I gave an essentially thermodynamic proof of Eq. (22) in order not to divert attention from the arguments which led Einstein to the light-quantum. Let me now briefly sketch Einstein's own derivation.

[22]In 1910, Lorentz gave an instructive comparison of the statistical methods of Boltzmann, Gibbs, and Einstein (see Lorentz, 1927).

Einstein started from Boltzmann's relation

$$S=\frac{R}{N}\ln W+\text{const}. \tag{29}$$

according to which a reversible change from a state a to a state b satisfies

$$S^a-S^b=\frac{R}{N}\ln\frac{W^a}{W^b}. \tag{30}$$

Let the system consist of subsystems $1, 2, \ldots$ which do not interact and therefore are statistically independent. Then

$$W=W_1W_2\ldots$$
$$S^a-S^b=\frac{R}{N}\ln\frac{W_1^a}{W_1^b}\frac{W_2^a}{W_2^b}\ldots. \tag{31}$$

For the case of an ideal Boltzmann gas, the subsystems may be taken to be the individual molecules. Let the gas in the states a and b have volume and temperature (v, T) and (v_0, T) respectively. Then $W_i(v)/W_i(v_0) = v/v_0$ for all i. The n molecules are statistically independent, so that

$$\left(\frac{W^a}{W_b}\right)=\left(\frac{v}{v_0}\right)^n. \tag{32}$$

Equations (30) and (31) again give Eq. (19).[23]

(3) Einstein's introduction of light-quanta in the Wien regime is the first step towards the concept of radiation as a Bose gas of photons. From the analogy made between Eqs. (19) and (22) it follows that Einstein's energy quanta are statistically independent in the Wien region, see Eq. (32). The photon gas is described (for all frequencies) by Bose statistics. In that description Eq. (32) does not hold in general. I shall comment in Sec. VI.C on the equivalence of Boltzmann and Bose statistics in the Wien regime.

E. Einstein on Planck: 1906.

In 1906 Einstein returned once more to Planck's theory of 1900. Now he had much more positive things to say about Planck's radiation law. This change in attitude was due to his realization that "Planck's theory makes implicit use of the ... light-quantum hypothesis" (Einstein, 1906b). Einstein's reconsideration of Planck's reasoning and of its relation to his own work can be summarized in the following way:

(1) Planck had used the $\rho-U$ relation, Eq. (10), which follows from classical mechanics and electrodynamics.

(2) Planck had introduced a quantization related to U, namely the prescription $U=Ph\nu/N$ [see Eqs. (11)–(14)].

(3) If one accepts step 2, which is alien to classical theory, then one has no reason to trust Eq. (10) which is an orthodox consequence of classical theory.

(4) Einstein had introduced a quantization related to ρ: the light-quantum hypothesis. In doing so he had not used the $\rho-U$ relation (10).

(5) The question arises if one can establish a connection between Planck's quantization related to U and Einstein's quantization related to ρ.

Einstein's answer: This is indeed possible namely by introducing a *new assumption:* Equation (10) is also valid in the quantum theory! Thus Einstein proposed to trust Eq. (10) even though its theoretical foundation had become a mystery when quantum effects are important. Einstein then re-examined the derivation of Planck's law with the help of this new assumption. I omit the details and only state his conclusion. "We must consider the following theorem to be the basis of Planck's radiation theory: the energy of a [Planck oscillator] can only take on values which are integral multiples of $h\nu$; in emission and absorption the energy of a [Planck oscillator] changes by jumps which are multiples of $h\nu$." Thus already in 1906 Einstein had guessed correctly the main properties of a quantum-mechanical oscillator and its behavior in radiative transitions. We shall see later that Planck was not at all prepared to accept at once Einstein's reasoning, in spite of the fact that it lent support to his own endeavors. As to Einstein himself, his acceptance of Planck's Eq. (10), albeit as a hypothesis, led to a major advance in his own work: The quantum theory of specific heats, to be discussed in Sec. III.

F. The photoelectric effect: The second coming of *h*.

The most widely remembered part of Einstein's 1905 paper on the quantum theory deals with his interpretation of the photoelectric effect. The present discussion of this subject is organized as follows. After a few general remarks I first sketch its history from 1887 to 1905. Then I turn to Einstein's contribution. Finally I outline the developments up to 1916 by which time Einstein's predictions were confirmed.

These days photoelectron spectroscopy is a giant field of research with its own journals. Gases, liquids, and solids are being investigated. Applications range from solid state physics to biology. The field has split into subdisciplines such as the spectroscopy in the ultraviolet (UPS) and in the X-ray region (XPS). In 1905, however, the subject was still in its infancy. We have a detailed picture of the status of photoelectricity a few months before Einstein finished his paper on light-quanta: In December 1904 the first review article on this topic was completed (von Schweidler, 1904). We infer from it that at that time photoelectricity was as much a frontier subject as were radioactivity, cathode ray physics, and (to a slightly lesser extent) the study of Hertzian waves.

In 1905 the status of experimental techniques was still rudimentary in all these areas, yet in each of them, initial discoveries of great importance had already been made. Not surprisingly, an experimentalist, mainly active in one of these areas, would also apply himself to some of the others. Thus Heinrich Hertz (1857–1894), the first one to observe a photoelectric phenomenon,[24] made this discovery at about the same time he demonstrated the electromagnetic nature of light. The high

[23]Einstein noted that Eqs. (18) and (32) together with $S=k\ln W$ yield the ideal gas law.

[24]I consider only the so-called external photoelectric effects.

school teachers Julius Elster (1854–1920) and Hans Geitel (1855–1923) pioneered the study of photoelectric effects in vacuum and constructed the first phototubes (Elster and Geitel, 1890); they also performed fundamental experiments in radioactivity. Pierre Curie (1859–1906) and one of his co-workers were the first to discover that photoelectric effects can be induced by x rays (Curie and Sagnac, 1900). J. J. Thomson is best remembered for his discovery of the electron in his study of cathode rays (Thomson, 1897). Yet perhaps his finest experimental contribution deals with the photoeffect.

Let us now turn to the work by the pioneers.

(1) *1887: Hertz.* Five experimental observations made within the span of one decade have largely shaped the physics of the twentieth century. In order of appearance they are the discoveries of the photoelectric effect, x rays, radioactivity, the Zeeman effect, and the electron. The first three of these were made accidentally. Hertz found the photoeffect when he became intrigued by a side effect which he found in the course of his investigations on the electromagnetic wave nature of light (Hertz, 1887). At one point he was studying spark discharges generated by potential differences between two metal surfaces. A primary spark coming from one surface generates a secondary spark on the other. Since the latter was harder to see Hertz built an enclosure around it to eliminate stray light. He was struck by the fact that this caused a shortening of the secondary spark. He found next that this effect was due to that part of the enclosure which was interposed between the two sparks. It was not an electrostatic effect since it made no qualitative difference whether the interposed surface was a conductor or an insulator. Hertz began to suspect that it might be due to the light given off by the primary spark. In a delightful series of experiments he confirmed his guess: light can produce sparks. For example he increased the distance between the metal surfaces until sparks ceased to be produced. Then he illuminated the surfaces with a nearby electric arc lamp: the sparks reappeared. He also came to the (not quite correct) conclusion that "... If the observed phenomenon is indeed an action of light, then it is only one of ultraviolet light."

(2) *1888: Hallwachs.* Stimulated by Hertz's work, Wilhelm Hallwachs (1859–1922) showed next that irradiation with ultraviolet light causes uncharged metallic bodies to acquire a positive charge (Hallwachs, 1888).

The earliest speculations on the nature of the effect predate the discovery of the electron in 1897. It was suggested in 1889 that ultraviolet light might cause specks of metallic dust to leave the metal surface (Lenard and Wolf, 1889).

(3) *1899: J. J. Thomson.* Thomson (1899) was the first to state that the photoeffect induced by ultraviolet light consists of the emission of electrons. He began his photoelectric studies by measuring the e/m of the particles produced by light, using the same method he had applied to cathode rays two years earlier (the particle beams move through crossed electric and magnetic fields). His conclusion: "The value of m/e in the case of ultraviolet light .... is the same as for cathode rays." In 1897 he had been unable to determine m or e separately for cathode rays. Now he saw his way clear to do this for photoelectrons. His second conclusion: "e is the same in magnitude as the charge carried by the hydrogen atom in the electrolysis of solutions."

Thomson's method for finding e is of major interest since it is one of the earliest applications of cloud chamber techniques. His student Charles Thomson Rees Wilson (1869–1959) had discovered that charged particles can form nuclei for condensation of supersaturated water vapor. Thomson applied this method to determine the number of charged particles by droplet counting. Their total charge was determined electrometrically. In view of these technical innovations his value for e (6.8×10^{-10} esu) must be considered as very respectable.

(4) *1902: Lenard.* In 1902 Philip Lenard (1862–1947) studied the photoeffect using a carbon arc light as a source. He could vary the intensity of his light source by a factor ~1000. He made the crucial discovery that the electron energy showed "not the slightest dependence on the light intensity" (Lenard, 1902). What about the variation of the photoelectron energy with the light frequency? One increases with the other, that was all that was known in 1905 (von Schweidler, 1904).

(5) *1905: Einstein.* On the basis of his heuristic principle Einstein proposed the following "simplest picture" for the photoeffect. A light-quantum gives all its energy to a single electron. And the energy transfer by one light-quantum is independent of the presence of other light-quanta. He also noted that an electron ejected from the interior of the body will in general suffer an energy loss before it reaches the surface. Let $E_{\max}$ be the electron energy for the case that this energy loss is zero. Then, Einstein proposed, we have the relation (in modern notation)

$$E_{\max} = h\nu - P, \tag{33}$$

where ν is the frequency of the incident (monochromatic) radiation, and P is the work function, the energy needed to escape the surface. He pointed out that Eq. (33) explains Lenard's observation of the light intensity independence of the electron energy.

Equation (33) represents the second coming of h. This equation made very strong predictions. First, E should vary linearly with ν. Secondly, the slope of the (E, ν) plot is a universal constant, independent of the nature of the irradiated material. Thirdly, the value of the slope was predicted to be Planck's constant determined from the radiation law.

Einstein gave several other applications of his heuristic principle. (1) The frequency of light in photoluminescence cannot exceed the frequency of the incident light (Stokes' rule). (2) In photoionization the energy of the emitted electron cannot exceed $h\nu$, where ν is the incident light frequency.[25] These two statements were made in 1905 (Einstein, 1905a). (3) In 1906 he discussed the application to the inverse photoeffect (Volta effect) (Einstein, 1906b). (4) In 1909 he treated

[25] In 1912 Einstein (1912a, b, c) noted that the heuristic principle could be applied not only to photoionization but also in a quite similar way to photochemical processes.

the generation of secondary cathode rays by X-rays (Einstein, 1909a). (5) In 1911 he used the principle to predict the high-frequency limit in Bremsstrahlung (Einstein, 1912d, p. 443).

(6) *1915: Millikan; the Duane–Hunt limit.* In 1909 a second review paper on the photoeffect appeared (Ladenburg, 1909). We learn from it that experiments were in progress to find the frequency dependence of $E_{\max}$ but that no definite conclusions could be drawn as yet. Among the results obtained during the next few years those of Arthur Llewellyn Hughes (1883–1978), J. J. Thomson's last student, are of particular interest. Hughes found a linear $E-\nu$ relation and a value for the slope parameter which varied from $4.9-5.7\times10^{-27}$, depending on the nature of the irradiated material (Hughes, 1912). These and other results were critically reviewed in 1913 and technical reservations about Hughes' results were expressed (Pohl and Pringsheim, 1913). However, soon thereafter Jeans stated in his important survey of the theory of radiation (Jeans, 1914) that "there is almost general agreement" that Eq. (33) holds true. Opinions were divided, yet evidently experimentalists were beginning to close in on the Einstein relation.

In the meantime, in his laboratory at the University of Chicago, Millikan had already been at work on this problem for several years. He used visible light (a set of lines in the mercury spectrum); various alkali metals served as targets. (These are photosensitive up to ~0.6μ). On April 24, 1914 and again on April 24, 1915 he reported on the progress of his results at meetings of the American Physical Society (Millikan, 1914, 1915). A long paper (Millikan, 1916b) published in 1916 gives the details of the experiments and a summary of his beautiful results: Eq. (33) holds very well and "Planck's h has been photoelectrically determined with a precision of about 0.5% and is found to have the value $h=6.57\times10^{-27}$."

Also the Volta effect confirmed the heuristic principle. This evidence came from x-ray experiments performed in 1915 at Harvard by William Duane (1872–1935) and his assistant Franklin Hunt. (Duane was one of the first biophysicists in America, his interest in x rays was due largely to the role they play in cancer therapy.) Working with an x-ray tube which is operated at a constant potential V they found that the x-ray frequencies produced have a sharp upper limit ν given by $eV=h\nu$, as was predicted by Einstein in 1906. This limiting frequency is now called the Duane–Hunt limit. They also obtained the respectable value $h=6.39\times10^{-27}$.

In Sec. I, I have already mentioned some of Millikan's reactions to these developments. Duane and Hunt (1915) did not quote Einstein at all in their paper. In Sec. IV.A I shall discuss further the impact of these discoveries on the acceptance of Einstein's ideas.

G. The fusion of particles and waves and Einstein's destiny

1909 was another eventful year in Einstein's life. He had started his academic career as a Privatdozent in Bern and gave his first lectures there in the winter term 1908–1909. His subject: the theory of radiation. On July 6 he left the Patent Office. He received his first honorary degree in that same month (from the University of Geneva). In September he attended his first physics conference (which took place in Salzburg). In October he moved to Zürich, to start work as Extraordinarius (associate professor) at the University. In those days he was already much involved in the problem of how to generalize the special theory of relativity. Yet his intense preoccupation with the quantum problems continued. In 1908 he had written to a friend: "I am incessantly busy with the question of the constitution of radiation .... This quantum question is so uncommonly important and difficult that it should concern everyone" (Einstein, 1908). I now turn to the discussion of two profound papers on the quantum theory which Einstein published in 1909. The first one (1909b) was completed in January. The second one (1909a) was presented to the Salzburg conference in October.

In 1905 Einstein had used the Wien law although it had not yet acquired a firm theoretical foundation. In 1909 he did the same with Planck's law. In earlier days he had never mentioned doubts about the experimental validity of Planck's expression for $\rho(\nu,T)$. But he had never expressed himself more firmly in favor of accepting this law than in his talk at Salzburg: "One cannot think of refusing [to accept] Planck's theory." In the next sentence he gave a new reason for this conviction: Geiger and Rutherford's value for the electric charge had been published and Planck's value for e had been "brilliantly confirmed." (See the discussion at the end of Sec. II.A).

In his January paper Einstein gave the derivation mentioned in Sec. II.D of the fluctuation formula (24) and applied this result to energy fluctuations of blackbody radiation in a frequency interval between ν and $\nu+d\nu$. In order to understand how this refinement is made, consider a small subvolume v of a cavity filled with thermal radiation. Enclose v with a wall which prevents all frequencies but those in $d\nu$ from leaving v while those in $d\nu$ can freely leave and enter v. We may then apply Eq. (24) with $\langle E\rangle$ replaced by $\rho v d\nu$. $\langle\epsilon^2\rangle$ is now a function of ν and T, and we have

$$\langle\varepsilon^2(\nu,T)\rangle=kT^2 v d\nu(\partial\rho/\partial T)\,. \tag{34}$$

This equation expresses the energy fluctuations in terms of the spectral function ρ in a way which is independent of the detailed form of ρ. Consider now the following three cases.

(1) ρ is given by the Rayleigh-Einstein-Jeans law Eq. (16). Then

$$\langle\varepsilon^2(\nu,T)\rangle=\frac{c^3}{8\pi\nu^2}\rho^2 v\,d\nu\,. \tag{35}$$

(2) ρ is given by the Wien law Eq. (5). Then

$$\langle\varepsilon^2(\nu,T)\rangle=h\nu\rho v\,d\nu\,. \tag{36}$$

(3) ρ is given by the Planck law Eq. (6). Then

$$\langle\varepsilon^2(\nu,T)\rangle=\left(h\nu\rho+\frac{c^3}{8\pi\nu^2}\rho^2\right)v\,d\nu\,. \tag{37}$$

(I need not apologize for having used the same symbol ρ in the last three equations even though ρ is a different function of ν and T in each of them.[26])

In his discussion of Eq. (37) Einstein stressed that "the current theory of radiation is incompatible with this result." By "current theory" he meant, of course, the classical wave theory of light. Indeed, the classical theory would only give the second term in Eq. (37), the "wave term" [compare Eqs. (37) and (35)]. About the first term of Eq. (37) Einstein had this to say: "If it alone were present, it would result in fluctuations [to be expected] if radiation were to consist of independently moving pointlike quanta with energy $h\nu$." In other words, compare Eqs. (36) and (37). The former corresponds to Wien's law which in turn holds in the regime in which Einstein had introduced the light-quantum postulate.

Observe the appearance of a new element in this last statement by Einstein. The word "pointlike" occurs. Although he did not use the term, he now was clearly thinking of quanta as particles. His own way of referring to the particle aspect of light was to call it "the point of view of the Newtonian emission theory." Eq. (37) suggests (loosely speaking) that the particle and wave aspects of radiation occur side by side. This is one of the arguments which led Einstein in 1909 to summarize his view on the status of the radiation theory in the following way.[27]

"*I already attempted earlier to show that our current foundations of the radiation theory have to be abandoned ... it is my opinion that the next phase in the development of theoretical physics will bring us a theory of light which can be interpreted as a kind of fusion of the wave and the emission theory ... [the] wave structure and [the] quantum structure ... are not to be considered as mutually incompatible ... it seems to follow from the Jeans law [Eq. 16] that we will have to modify our current theories, not to abandon them completely.*"

This fusion now goes by the name of complementarity. The reference to the Jeans law we would now call an application of the correspondence principle.

The extraordinary significance for twentieth century physics of Einstein's summation hardly needs to be stressed. I also see it as highly meaningful in relation to the destiny of Einstein the scientist if not of Einstein the man. In 1909, at age 30, he was prepared for a fusion theory. He was alone in this. Planck certainly did not support this vision. Bohr had yet to arrive on the scene. Yet when the fusion theory arrived, Einstein could not accept the duality of particles and waves inherent in that theory, quantum mechanics, as being fundamental and irrevocable. It may have distressed him that one statement he made in 1909 needed revision: moving light-quanta with energy $h\nu$ are not pointlike. In the later parts of this paper I shall have to make a number of comments on the scientific reasons which changed Einstein's apartness from a figure far ahead of his time to a figure on the sidelines. Can this change be fully explained on the grounds of his scientific philosophy? It is too early to tell, but I doubt it.

Exactly half a century had passed since Kirchhoff stated that there had to be a blackbody radiation law. The law had been found. A small number of physicists had realized that its implications were momentous. A proof of the law did not yet exist.

At this point I need to interrupt the account of the history of radiation theory in order to describe Einstein's contributions to the theory of specific heat. I shall return to radiation in Sec. IV, which contains another important result found in Einstein's 1909 papers. As a postscript to the present section I add a brief remark on Einstein's energy fluctuation formula.

Equations (35)–(37) were derived by a statistical reasoning. One should also be able to derive them in a directly dynamical way. Einstein himself had given qualitative arguments for the case of Eq. (35). He noted that the fluctuations come about by interference between waves with frequencies within and outside the $d\nu$ interval. A few years later Lorentz (1916) gave the detailed calculation, obtaining Eq. (35) from classical electromagnetic theory. However, difficulties arose when it was attempted to derive the Planck case Eq. (37) dynamically. These were noted in 1919 by Leonard Salomon Ornstein (1880–1941) and Frits Zernike (1888–1966), two Dutch experts on statistical physics. The problem was further elaborated (1925) by Paul Ehrenfest (1880–1933). It was known at that time that one can obtain Planck's expression for ρ by introducing the quantum prescription[28] that the electromagnetic field oscillators could only have energies $nh\nu$. However, the mentioned authors found that the same prescription applied to the fluctuation formula gave the wrong answer. The source of the trouble seemed to lie in Einstein's entropy additivity assumption, Eq. (25). According to Uhlenbeck (private communication) these discrepancies were for some years considered to be a serious problem. In their joint 1925 paper, Born, Heisenberg, and Jordan refer to it as a fundamental difficulty (Born, Heisenberg, and Jordan 1925). In that same paper it was shown, however, that the new quantum mechanics applied to a set of noninteracting oscillators does give the Einstein answer. The noncommutatvity of coordinates and momenta plays a role in this derviation. Again according to Uhlenbeck (private communication) the elimination of this difficulty was considered as one of the early successes of quantum mechanics. (It is not necessary for our purposes to discuss subsequent improvements of the Heisenberg–Born–Jordan treatment.[29])

III. EINSTEIN AND SPECIFIC HEATS

A. Specific heats in the nineteenth century.

By the end of the first decade of the twentieth century three major quantum theoretical discoveries had been

[26]Equations (35) and (36) do not explicitly occur in Einstein's own paper.

[27]In the following quotation I combine statements made in the January and in the October paper. The italics are mine.

[28]The elementary derivation due to Debye in 1910 is found in Sec. VII.C.

[29]The reader interested in these further developments is referred to a paper by Gonzalez and Wergeland (1973) which also contains additional references to this subject.

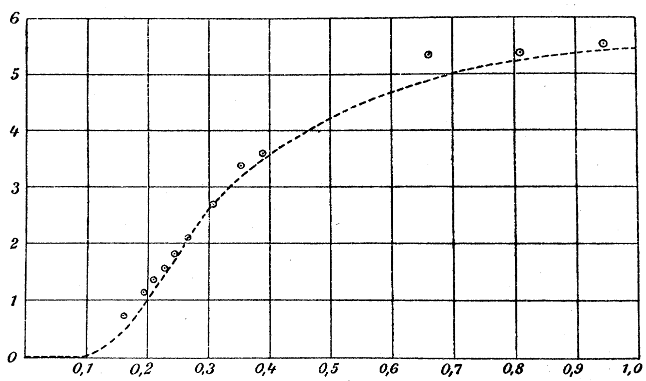

FIG. 3. The first published graph dealing with the quantum theory of the solid state: Einstein's expression for the specific heat of solids [given in Eq. (42)] plotted versus $h\nu/kT$. The little circles are Weber's experimental data for diamond. Einstein's best fit to Weber's measurements corresponds to $h\nu/k \cong 1300$ °K.

made. They concern the blackbody radiation law, the light-quantum postulate, and the quantum theory of the specific heat of solids. All three arose from statistical considerations. There are, however, striking differences in the time intervals between these theoretical advances and their respective experimental justification. Planck formulated his radiation law in a most uncommonly short time after learning about the experiments in the far infrared which complemented earlier results at higher frequencies. It was quite a different story with the light-quantum. Einstein's hypothesis was many years ahead of its decisive experimental tests. As we shall see next, the story is quite different again in the case of specific heats. Einstein's first paper on the subject (1907a), submitted in November 1906, contains the qualitatively correct explanation of an anomaly that had been observed as early as 1840: the low value of the specific heat of diamond at room temperature. Einstein showed that this can be understood as a quantum effect. His paper contains one graph: the specific heat of diamond as a function of temperature, reproduced in Fig. 3. The dashed line is Einstein's theoretical curve. The little circles refer to data obtained in 1875. Figure 3 is the first published graph in the history of the quantum theory of the solid state. It also represents one of only two instances I know in which Einstein published a graph for the purpose of comparing theory with experiment. (The other example will be mentioned later in this section.)

In order to recognize an anomaly one needs a theory or a rule or at least a prejudice. As I just mentioned, peculiarities in specific heats were diagnosed more than half a century before Einstein explained them. It was also known well before 1906 that specific heats of gases exhibited even more curious properties. In what sense did one so early consider diamond so exceptional? And what about other substances? For a perspective on Einstein's contributions it is necessary to sketch the answer to these questions. I therefore begin with a short account of specific heats in the nineteenth century.

This story begins in 1819 when two young Frenchmen, Pierre Louis Dulong (1785–1838) and Alexis Thérèse Petit (1791–1820) made an unexpected discovery during the researches in thermometry on which they had been jointly engaged for a number of years. For a dozen metals and for sulphur (all at room temperature) they found that c, the specific heat *per gram atom*[30] (briefly referred to as the specific heat hereafter) had practically the same value, approximately 6 cal/mole deg (Petit and Dulong, 1819). They did of course not regard this as a mere coincidence: "One is allowed to infer [from these data] the following law: The atoms of all simple bodies [elements] have exactly the same heat capacity." They did not restrict this statement to elements in solid form. Rather they believed initially that improved experiments might show their law to hold also for gases. By 1830 it was clear, however, that the rule could, at best, apply to solids only (Fox, 1968).

Much remained to be learned about atomic weights in those early days of modern chemistry. In fact in several instances Dulong and Petit (correctly) halved the value of atomic weights, obtained earlier by other means, in order to bring their data into line with their law (Fox, 1968). For many years their rule continued to be an important tool for atomic weight determinations.

It became clear rather soon, however, that even for solid elements the Dulong–Petit rule is not as general as its propounders had thought. Amedeo Avogadro (1776–1856) was one of the first to remark on deviations in the case of carbon, but his measurements were not very precise (Avogadro, 1833, especially pp. 96–98).[31] Matters got more serious in 1840 when two Swiss physicists, Auguste de la Rive (1801–1873) and François Marcet (1803–1883) reported on studies of carbon. In particular they had obtained "not without difficulty and expense" an amount of diamond powder sufficient to experiment with and for which they found $c \simeq 1.4$. At almost the same time diamond was also studied (Regnault, 1841, especially pp. 202–205) by Henri Victor Regnault (1810–1878) who more than any other physicist contributed to the experimental investigations of specific heats in the nineteenth century. His value: $c \simeq 1.8$. Regnault's conclusion about carbon was unequivocal: It is "a complete exception among the simple bodies: it does not satisfy the general law which [relates] specific heats and atomic weights." During the next twenty years he continued his studies of specific heats and found many more deviations from the general law, though none as large as for diamond.

We now move to the 1870's when Heinrich Friedrich Weber (1843–1912), then in Berlin, made the next advance (Weber, 1872). He began by reanalyzing the data of de la Rive and Marcet and of Regnault and came to

[30] To be precise, these and other measurements on solids to be mentioned hereafter refer to c_p at atmospheric pressure. Later on, a comparison will be made with theoretical values for c_v. This requires a tiny correction to go from c_p to c_v. This correction will be ignored (Lewis, 1907).

[31] In 1833 Avogadro obtained $c \simeq 3$ for carbon at room temperature. This value is too high. Since it was accidentally just half the Dulong–Petit value, Avogadro incorrectly conjectured "that one must reduce the atom [i.e., the atomic weight] of sulphur and metals in general by [a factor] one half."

the correct conclusion that the different values for the specific heat of diamond found by these authors was not due to systematic errors. However, the Swiss value referred to a temperature average from 3 °–14 °C while Regnault's value was an average from 8°–98 °C. Weber noted that both experiments could be right if the specific heat of carbon were to vary with temperature! Tiny temperature variations of specific heats had long been known for substances such as water (see Neumann, 1831). In contrast, Weber raised the issue of a very strong T dependence—a new and bold idea. His measurements for twelve different temperatures between 0°–200 °C confirmed his conjecture: for diamond c varied by a factor three over this range. He wanted to continue his observations, but it was March and there was no more snow for his ice calorimeter. He announced that he would go on with his measurements "as soon as meteorological circumstances permit." The next time we hear from Weber is in 1875 when he presented his beautiful specific heat measurements for boron, silicon, graphite and diamond, from −100° to 1000 °C (Weber, 1875). For the case of diamond c varied by a factor 15 between these limits.

Already in 1872 Weber had made a conjecture which he confirmed in 1875: at high T one gets close to the Dulong-Petit value. In Weber's words (1875): "The three curious exceptions [C, B, Si] to the Dulong-Petit law which were till now a cause for despair have been eliminated: the Dulong-Petit law for the specific heats of solid elements has become an unexceptional rigorous law." This is of course not quite true but it was distinct progress. The experimental points in Fig. 3 are Weber's points of 1875.[32]

[Weber later moved from Germany to Zürich and became one of Einstein's physics professors during the latter's student days at the ETH (1896–1900). Einstein's notebooks of Weber's lectures are preserved. They do not indicate that as a student Einstein heard of Weber's results.]

In 1872, not only Weber but also a second physicist made the conjecture that the Dulong-Petit value $c \simeq 6$ would be reached by carbon at high temperatures: James Dewar (1842–1923). His road to the carbon problem was altogether different: for reasons having to do with solar temperatures Dewar became interested in the boiling point of carbon. This led him to do high-temperature experiments from which he concluded (Dewar, 1872) that the mean specific heat of carbon between 0° and 2000 °C equals about 5, and that "the true specific heat [per gram] at 2000° must be at least 0, 5 so that at this temperature carbon would agree with the law of Dulong and Petit."[33]

Dewar's most important contribution to our subject deals with very low temperatures. He had liquefied hydrogen in 1898. In 1905 he reported on the first specific heat measurements in the newly opened temperature region. It will come as no surprise that diamond was among the first substances he chose to study. For this case he found the very low average value $c \simeq 0.05$ in the interval $T = 20°–85°K$. "An almost endless field of research in the determination of specific heats is now opened," Dewar remarked in this paper (Dewar, 1905). His work is included in a detailed compilation by Wigand (1907) of the literature on the specific heats of solid elements which appeared in the same issue of the Annalen der Physik as Einstein's first paper on the quantum theory of specific heats. We are therefore up to date in regard to the experimental developments preceding Einstein's work.

The early theoretical considerations began in 1871 with Boltzmann. At that time only the simplest application of the equipartition theorem was known: the average kinetic energy equals $kT/2$ for each degree of freedom. In 1871 Boltzmann (1871) showed that the average kinetic energy equals the average potential energy for a system of particles, each one of which oscillates under the influence of external harmonic forces. In 1876 he applied these results to a three-dimensional lattice (Boltzmann 1876). This gave him an average energy 3RT ≃ 6 cal/mol. Hence c_v, the specific heat at constant volume, equals 6 cal/mol deg. Thus after half a century the Dulong-Petit value had found a theoretical justification! As Boltzmann himself put it, his result was in good agreement with experiment "for all simple solids with the exception of carbon, boron[34] and silicon." Boltzmann went on to speculate that these anomalies might be a consequence of a loss of degrees of freedom due to a "sticking together" at low temperatures of atoms at neighboring lattice points. This suggestion was elaborated by others (Richarz, 1893) and is mentioned by Wigand (1907) in his 1906 review as the best explanation of this effect. I mention this incorrect speculation only in order to bring out one important point: Before Einstein's paper of 1906 it was not realized that the diamond anomaly was to be understood in terms of the failure (or, rather, the inapplicability) of the classical equipartition theorem. Einstein was the first one to state this fact clearly.

By sharp contrast, it was well appreciated that the equipartition theorem was in trouble when applied to the specific heat of gases. This was a matter of grave concern to the nineteenth century masters. Even though this is a topic which does not directly bear on Einstein's work in 1906, I believe it to be useful to complete the nineteenth century picture with a brief explanation why gases caused so much more aggravation.

The reasons were clearly stated by Maxwell (1965) in a lecture given in 1875. "The spectroscope tells us that some molecules can execute a great many different kinds of vibrations. They must therefore be systems of a very considerable degree of complexity, having far more than six variables [the number characteristic for a rigid body]... every additional variable increases the specific heat... every additional degree of complexity which we

[32]By the end of the nineteenth century it was clear that the decrease of c with temperature occurs far more generally than just for C, B, and Si (Behn, 1893).

[33]There followed a controversy about priorities between Weber and Dewar, but only a very mild one by nineteenth century standards. In any event, there is no question but that the issues were settled only by Weber's detailed measurements in 1875.

[34]The good professor wrote "bromine" but meant "boron."

attribute to the molecule can only increase the difficulty of reconciling the observed with the calculated value of the specific heat. I have now put before you what I consider the greatest difficulty yet encountered by the molecular theory."

Maxwell's conundrum was the mystery of the missing vibrations. The following oversimplified picture suffices to make clear what troubled him. Consider a molecule made up of n structureless atoms. There are $3n$ degrees of freedom, three for translations, at most three for rotations, the rest are vibrations. The kinetic energy associated with each degree of freedom contributes $kT/2$ to c_v. In addition there is a positive contribution from the potential energy. Maxwell was saying that this would almost always lead to specific heats which are too large. One consequence of Maxwell's lecture was that it focused attention on monatomic gases and in 1876 the equipartition theorem scored an important success: it was found that $c_p/c_v \cong 5/3$ for mercury vapor, in accordance with $c_v = 3R/2$ and the ideal gas rule $c_p - c_v = R$ (Kundt and Warburg, 1876). Also, it had been known since the days of Regnault[35] that several diatomic molecules (including hydrogen) have a c_v close to $5R/2$. It was not yet recognized by Maxwell that this is the value prescribed by the equipartition theorem for a rigid dumbbell molecule; this observation was first made by Boltzmann (1876). This theorem was therefore very helpful, yet, on the whole, the specific heat of gases remained a murky subject.

Things were getting worse. Already before 1900 instances were found in which c_v depends (weakly) on temperature (Wüllner, 1896), in flagrant contradiction to classical concepts. No wonder these results troubled Boltzmann: molecules in dilute gases hardly stick together! In 1898 he suggested a rather desperate way out: lack of thermal equilibrium (Boltzmann, 1912).

In 1900 Rayleigh (1900b) remarked that "What would appear to be wanted is some escape from the destructive simplicity of the general conclusion [derived from equipartition]." Later that year Kelvin (1901), quoting Rayleigh in a lecture before the Royal Institution, added his own comment: "Such an escape [from equipartition] would mean that in the beginning of the twentieth century [we would] lose sight of a cloud which has obscured the brilliance of the molecular theory of heat and light during the last quarter of the nineteenth century."

Such was the state of affairs when Einstein took on the specific heat problem.

B. Einstein

Until 1906 Planck's quantum had played a role only in the rather isolated problem of blackbody radiation. Einstein's work on specific heats (1907a) is above all important because it made clear for the first time that quantum concepts have a far more general applicability. His 1906 paper is also unusual because here we meet an Einstein who is quite prepared to use a model he knows to be approximate in order to bring home a point of principle. Otherwise this paper is much like his other innovative articles: succinctly directed to the heart of the matter.

Earlier in 1906 Einstein had come to accept Planck's relation (10) between ρ and U as a new physical assumption (see Sec. II.E). We have seen in Sec. II.A that Planck had obtained the expression

$$U(\nu, T) = \frac{\xi kT}{\exp \xi - 1}, \quad \xi = \frac{h\nu}{kT} \tag{38}$$

by introducing a prescription which modified Boltzmann's way of counting states. Einstein's specific heat paper begins with a new prescription for arriving at the same result. He wrote U in the form[36]

$$U(\nu, T) = \frac{\int E e^{-E/kT} \omega(E, \nu)\, dE}{\int e^{-E/kT} \omega(E, \nu)\, dE}. \tag{39}$$

The exponential factor denotes the statistical probability for the energy E. The weight factor ω contains the dynamical information about the density of states between E and $E + dE$. For the case in hand (linear oscillators) ω is trivial in the classical theory: $\omega(E, \nu) = 1$. This yields the equipartition result $U = kT$. Einstein proposed a new form for ω. Let $\varepsilon = h\nu$. Then ω shall be different from zero only when $n\varepsilon \leq E \leq n\varepsilon + \alpha$, $n = 0, 1, 2, \ldots$ "where α is infinitely small compared to ε," and such that

$$\int_{n\varepsilon}^{n\varepsilon+\alpha} \omega\, dE = A, \text{ for all } n, \tag{40}$$

where the value of the constant A is irrelevant. Mathematically this is the forerunner of the δ function! Today we would write

$$\omega(E, \nu) = \sum_n \delta(E - nh\nu).$$

From Eqs. (39) and (40) we recover Eq. (38). This new formulation is important because for the first time the statistical and the dynamical aspects of the problem are clearly separated. "Degrees of freedom must be weighed and not counted," as Sommerfeld (1968) put it later.

In commenting on his new derivation of Eq. (38) Einstein remarked: "I believe we should not content ourselves with this result." If we must modify the theory of periodically vibrating structures in order to account for the properties of radiation, are we then not obliged to do the same for other problems in the molecular theory of heat, he asked. "In my opinion the answer cannot be in doubt. If Planck's theory of radiation goes to the heart of the matter, then we must also expect to find contradictions between the present [i.e., classical] kinetic theory and experiment in other areas of the theory of heat—contradictions which can be resolved by following this new path. In my opinion this expectation is actually realized."

Then Einstein turned to the specific heat of solids. He introduced the following model of a three-dimensional crystal lattice. The atoms on the lattice points oscillate independently, isotropically, harmonically, and

[35]A detailed review of the specific heats of gases from the days of Lavoisier until 1896 is found in Wüllner's textbook (1896).

[36]I do not always use the notations of the original paper.

with a single frequency ν around their equilibrium positions.[37] He emphasized that one should of course not expect rigorous answers because of all these approximations.

The first generalization. Einstein applied Eq. (39) to his three-dimensional oscillators: In thermal equilibrium the total energy of a gram atom of oscillators equals $3NU(\nu, T)$, where U is given by Eq. (38) and N is Avogadro's number. Hence

$$c_v = 3R \frac{\xi^2 \exp \xi}{(\exp \xi - 1)^2}, \tag{42}$$

Einstein's specific heat formula.

The second generalization. For reasons of no particular interest to us now, Einstein initially believed that his oscillating lattice points were electrically charged ions. A few months later he published a correction to his paper (1907b) in which he observed that this was an unnecessary assumption. (In Planck's case the linear oscillators had of course to be charged!) Einstein's correction freed the quantum rules (in passing, one might say) from any specific dependence on electromagnetism.

Let us next consider a few properties of Einstein's specific heat formula.

(1) It yields the Dulong-Petit rule in the high-temperature limit.

(2) It is the first recorded example of a specific heat formula with the property

$$c_v(T) \to 0 \text{ as } T \to 0. \tag{43}$$

As we shall see in the next subsection, Eq. (43) played an important role in the ultimate formulation of Nernst's heat theorem.

(3) It is a one-parameter formula. The only freedom is the choice of the frequency[38] ν or, equivalently, the "Einstein temperature" T_E, the value of T for which $\xi = 1$. As was mentioned before, Einstein compared his formula with Weber's points for diamond. Einstein's fit can be expressed in temperature units by $T_E \simeq 1300\,°\text{K}$ for which "the points lie indeed almost on the curve." This high value of T_E makes clear why a light and hard substance like diamond exhibits quantum effects at room temperature. (By contrast $T_E \simeq 70\,°\text{K}$ for lead.)

(4) By his own account, Einstein took Weber's data from the Landolt-Bornstein tables. He must have used the 1905 edition (Landolt and Bornstein, 1905) which would be readily available in the Patent Office. These tables do not yet contain the earlier-mentioned results by Dewar in 1905. Apparently Einstein was not aware of these data in 1906 (although they were noted in that year by German physicists (Wigand, 1907)). Perhaps this was fortunate. In any case, Dewar's value $c_v \simeq 0.05$ for diamond refers to an average over the range $\xi \simeq 0.02$–0.07. This value is much too large to be accommodated (simultaneously with Weber's points) by Einstein's equation (42): The exponential drop of c_v as $T \to 0$, predicted by that equation, is far too steep.

Einstein did become aware of this discrepancy in 1911 when the much improved measurements by Nernst (1911a) showed that Eq. (42) fails at low T. Nernst correctly ascribed the disagreement to the incorrectness of the assumption that the lattice vibrations are monochromatic. Einstein himself (1911c) explored some modifications of this assumption. The correct T dependence at low temperatures was first obtained by Peter Debye (1884–1966): for nonmetallic substances $c_v \to 0$ as T^3 (Debye 1912). Einstein had ended his active research on the specific heats of solids by the time the work of Debye and the more exact treatment of lattice vibrations by Max Born (1882–1970) and Theodore von Kármán (1881–1963) appeared (Born and von Kármán, 1912, 1913).
These further developments need therefore not be discussed here.

However, in 1913 Einstein returned once again to specific heats, this time to consider the case of gases. This came about as the result of important experimental advances on this subject which had begun in 1912 with a key discovery by Arnold Eucken (1884–1950). It had long been known by then that $c_v \simeq 5$ for molecular hydrogen at room temperature. Eucken (1912) showed that this value decreased with decreasing T and that $c_v \simeq 3$ at $T \simeq 60\,°\text{K}$. As is well known today, this effect is due to the freezing of the two rotational degrees of freedom of this molecule at these low temperatures. In 1913 Einstein correctly surmised that the effect was related to the behavior of these rotations and attempted to give a quantitative theory. In a paper on this subject we find the second instance of curve fitting by Einstein (Einstein and Stern, 1913). However, this time he was wrong. His answer depended in an essential way on the incorrect assumption that rotational degrees of freedom have a zero point energy.

In 1925 Einstein was to turn his attention one last time to gases at very low temperatures, as we shall see in Sec. VI.C.

C. Nernst; Solvay I[39]

"As the temperature tends to absolute zero the entropy of a system tends to a universal constant which is independent of chemical or physical composition or of other parameters on which the entropy may depend. The constant can be taken to be zero." This modern general formulation of the third law of thermodynamics (barring a few exceptional situations) implies that specific heats tend to zero as $T \to 0$ (see Huang, 1963). The earliest and most primitive version of the "heat theorem" was presented in 1905, before Einstein had written his first paper on specific heats. The final form of the third law was arrived at and accepted only after decades of controversy and confusion.[40] For the present

[37] Volume changes due to heating and contributions to the specific heat due to the motions of electrons within the atoms are neglected, Einstein notes.

[38] In a later paper, Einstein (1911b) attempted to relate this frequency to the compressibility of the material.

[39] The preparation of this subsection was much facilitated by my access to an article by Klein (1965) and a book by Hermann (1969).

[40] Simon (1956) has given an excellent historical survey of this development.

account it is important to note the influence of Einstein's work on this evolution.

On December 23, 1905 Hermann Walther Nernst (1864–1941) read a paper at the Göttingen Academy entitled "On the computation of chemical equilibria from thermal measurements" in which he proposed a new hypothesis for the thermal behavior of liquids and solids at absolute zero (Nernst, 1906). For our purposes the 1905 hypothesis is of particular interest insofar as it applies to a chemically homogeneous substance. For this case the hypothesis states in essence that the entropy difference between two modifications of such a substance (for example graphite and diamond in the case of carbon) tends to zero as $T \to 0$. This does not guarantee that the entropy of a given substance tends to zero, (it does not exclude the possibility that the entropy becomes singular for $T \to 0$), and therefore it does not exclude a nonzero specific heat at zero temperatures. In fact, in 1906 Nernst (1906a, b) assumed that all specific heats tend to 1.5 cal/deg at $T = 0$. However, he noted that he had no proof of this statement because of the absence of sufficient low temperature data. He stressed (1906a) that it was a "most urgent task" to acquire these. Nernst's formidable energies matched his strong determination. He and his collaborators embarked on a major program for measuring specific heats at low temperatures. It covered the same temperature domain already studied by Dewar but the precision was much increased and more substances were examined. One of these was diamond, obviously.

By 1910 Nernst (1910) was ready to announce his first results. From his curves "one gains the clear impression that the specific heats become zero or at least take on very small values at very low temperatures. This is in qualitative agreement with the theory developed by Herr Einstein..."

Thus the order of events was as follows. Late in 1905 Nernst stated a primitive version of the third law. In 1906 Einstein gave the first example of a theory which implies that $c_v \to 0$ as $T \to 0$ for solids. In 1910 Nernst noted the compatibility of Einstein's result with "the heat theorem developed by me." But it was actually Planck who, later in 1910, took a step which "not only in form but also in content goes a bit beyond [the formulation given by] Nernst himself." In Planck's formulation the specific heat of solids and liquids does go to zero as $T \to 0$ (Planck, 1911). It should be stressed that neither Nernst nor Planck gave a proof of the third law. The status of this law was apparently somewhat confused, as is clear from Einstein's remark in 1914 that "All attempts to derive Nernst's theorem theoretically in a thermodynamical way with the help of the experimental fact that the specific heat vanishes at $T = 0$ must be considered to have failed." Einstein (1914) went on to remark that the quantum theory is indispensable for an understanding of this theorem.

Nernst's reference to Einstein in his paper of 1910 was the first occasion on which he acknowledged the quantum theory in his publications. His newly aroused interest in the quantum theory was, however, thoroughly pragmatic. In an address (on the occasion of the birthday of the emperor) he said: "At this time the quantum theory is essentially a computational rule, one may well say a rule with most curious, indeed grotesque properties. However... it has borne such rich fruits in the hands of Planck and Einstein that there is now a scientific obligation to take a stand in its regard and to subject it to experimental test." He went on to compare Planck with Dalton and Newton (Nernst, 1911b). Also in 1911, Nernst tried his hand at a needed modification of Einstein's Eq. (42) (Nernst and Lindemann, 1911).

Nernst was a man of parts, a gifted scientist, a man with a sense for practical applications, a stimulating influence on his students and an able organizer. Many people disliked him. But he commanded respect "so long as his egocentric weakness did not enter the picture" (Einstein, 1942a). He now saw the need for a conference on the highest level which should deal with the quantum problems. His combined talents as well as his business relations enabled him to realize this plan. He found the industrialist Ernest Solvay willing to underwrite the conference. He planned the scientific program in consultation with Planck and Lorentz. On October 29, 1911 the first Solvay Conference convened. Einstein was given the honor of being the final speaker. The title of his talk: "The current status of the specific heat problem." He gave a beautiful review of this subject—and used the occasion to express his opinion on the quantum theory of electromagnetic radiation as well. His contributions to the latter topic are without doubt more profound than his work on specific heats. Yet his work on the quantum theory of solids had a far greater immediate impact and considerably enlarged the audience of those willing to take quantum physics seriously.

Throughout the period discussed in the foregoing, the third law was considered to apply only to solids and liquids. Only in 1914 did Nernst dare to extend his theorem to hold for gases as well. Eucken's results on the specific heat of molecular hydrogen were a main motivation for taking this bold step (Nernst, 1914). Unlike the case for solids, Nernst could not point to a convincing theoretical model of a gas with the property $c_v \to 0$ as $T \to 0$. So it was to remain until 1925 when the first model of this kind was found. Its discoverer: Einstein. (See Sec. VI.C.)

IV. FROM THE LIGHT-QUANTUM TO THE PHOTON

A. Reactions to the light-quantum hypothesis

In Sec. II we followed the development of Einstein's work on electromagnetic radiation from 1905 to 1909. The present section deals with the continuation and conclusion of his work on this problem. It contains one more result which he obtained in 1909. Then it moves on to his contributions in 1916 and 1917. It ends with the discovery of the Compton effect in 1923.

Einstein moved around a good deal during this period. In March 1911 he left Zürich for Prague, starting his first appointment as full professor. In August 1912 he returned to Zürich to occupy a similar position, this time at the ETH. In December 1913 he accepted appointments in Berlin to a special chair at the Prussian Academy; as director of an Institute for Physics, to be founded by the Kaiser-Wilhelm Gesellschaft (this institute was established in 1917); and as a professor at the University of Berlin with the right but not the obligation to teach. He moved there in April 1914 and gave his inaugural address less than one month before the outbreak of the First World War.

In Sec. I, I have already touched on the reactions of the physics community to Einstein's light-quantum postulate. (See the end of Sec. I.B.) As a prelude to the discussion of the further evolution of this concept I begin with a more detailed analysis of the reasons why the light-quantum was so strongly resisted. To set the tone I mention what Planck, Nernst, Rubens, and Warburg wrote in 1913 when they proposed Einstein for membership in the Prussian Academy. Their recommendation expressed the highest praise for his achievements. It concludes as follows. "In sum one can say that there is hardly one among the great problems, in which modern physics is so rich, to which Einstein has not made a remarkable contribution. That he may sometimes have missed the target in his speculations, as for example in his hypothesis of light-quanta, cannot really be held too much against him for it is not possible to introduce really new ideas even in the most exact sciences without sometimes taking a risk" (Kirsten and Korber, 1975).

(1) *Einstein's caution.* Einstein and Michele Angelo Besso (1873–1955) had met in Zürich in about 1897 and pursued their friendship by correspondence from 1903 until the year of their death. Einstein's letters provide a rich source of his insights into physics and people. His struggles with the quantum theory in general and with the light-quantum hypothesis in particular is a recurring theme. In 1951 he wrote (1951a) to Besso: "Die ganzen 50 Jahre bewusster Grübelei haben mich der Antwort der Frage 'Was sind Lichtquanten' nicht näher gebracht."[41]

Throughout his scientific career quantum physics remained a crisis phenomenon to Einstein. His views on the nature of the crisis would change, but the crisis would not go away. This led him to approach quantum problems with great caution in his writings—a caution already evident in the way the title of his first paper on the quantum theory (1905a) was phrased. In the earliest years following his light-quantum proposal Einstein had good reasons to regard it as provisional. He could only formulate it clearly in the domain $h\nu/kT \gg 1$, where Wien's blackbody radiation law holds. Also, he had used this law as an experimental fact without explaining it. Above all, it was obvious to him from the start that grave tensions existed between his principle and the wave picture of electromagnetic radiation—tensions which, in his own mind, were resolved neither then nor later. A man as perfectly honest as Einstein had no choice but to emphasize the provisional nature of his hypothesis. He did this very clearly in 1911, at the first Solvay congress, where he said: "I insist on the provisional character of this concept [light-quanta] which does not seem reconcilable with the experimentally verified consequences of the wave theory" (Einstein, 1912d, p. 443).

This statement seems to have created the belief in several quarters that Einstein was ready to retract. In 1912 Arnold Sommerfeld (1912) wrote: "Einstein drew the most far-reaching consequences from Planck's discovery [of the quantum of action] and transferred the quantum properties of emission and absorption phenomena to the structure of light energy in space without, as I believe, maintaining today his original point of view [of 1905] in all its audacity." Referring to the light-quanta, Millikan (1913) stated in 1913 that Einstein "gave... up, I believe, some two years ago;" and in 1916 he wrote: "Despite... the apparently complete success of the Einstein equation [for the photoeffect] the physical theory of which it was designed to be the symbolic expression is found so untenable that Einstein himself, I believe, no longer holds to it" (Millikan, 1916b).

It is my impression that the resistance to the light-quantum idea was so strong that one almost hopefully mistook Einstein's caution for vacillation. However, judging from his papers and letters there is no evidence that at any time he retracted any of his statements made in 1905.

(2) *Electromagnetism: free fields and interactions.* Einstein's 1905 paper on light-quanta is the second of the revolutionary papers on the old quantum theory. The first one was of course Planck's paper (1900b) of December 1900. Both papers contained proposals which flouted classical concepts. Yet the resistance to Planck's ideas—while certainly not absent—was much less pronounced and vehement than in the case of Einstein. Why?

First a general remark on the old quantum theory. Its main discoveries concerned quantum rules for stationary states of matter and of pure radiation. By and large no comparable breakthroughs occurred in regard to the most difficult of all questions concerning electromagnetic phenomena: the interaction between matter and radiation. There, advances became possible only after the advent of quantum field theory when the concepts of particle creation and annihilation were formulated. Since then, progress on the interaction problems has been enormous. Yet even today this is not by any means a problem area on which the books are closed.

As we have seen in Sec. II, when Planck introduced the quantum in order to describe the spectral properties of pure radiation he did so by a procedure of quantization applied to matter, namely to his material oscillators. He was unaware of the fact that his proposal implied the need for a revision of the classical radiation field itself. His reasoning alleged to involve only a modification of the interaction between matter and radiation. This did not seem too outlandish, since the interaction problem was full of obscurities in any event. By contrast, when Einstein proposed the light-quantum he had dared to tamper with the Maxwell equations for free fields which were believed (with good reason) to be much better understood. Therefore it seemed less repugnant to accept Planck's extravaganzas than Einstein's.

[41]"All these 50 years of pondering have not brought me closer to answering the question 'What are light-quanta'." The collected Einstein-Besso correspondence will be referred to as (Speziali, 1972) in what follows.

This difference in assessment of the two theoretical issues, one raised by Planck, one by Einstein, is quite evident in the writings of the leading theorists of the day. Planck himself had grave reservations about light-quanta. In 1907 he wrote to Einstein" "I am not seeking for the meaning of the quantum of action [light-quanta] in the vacuum but rather in places where absorption and emission occur and [I] assume that what happens in the vacuum is rigorously described by Maxwell's equations" (Planck, 1907). A remark by Planck (1909) at a physics meeting in 1909 vividly illustrates his and others' predilections for "leaving alone" the radiation field and for seeking the resolution of the quantum paradoxes in the interactions: "I believe one should first try to move the whole difficulty of the quantum theory to the domain of the interaction between matter and radiation." In that same year Hendrik Antoon Lorentz (1865–1928) expressed his belief in "Planck's hypothesis of the energy elements" but also his strong reservations regarding "light-quanta which retain their individuality in propagation" (Lorentz, 1909).

Thus by the end of the first decade of the twentieth century many leading theorists were prepared to accept the fact that the quantum theory was here to stay. However, the Maxwell theory of the free radiation field, pure and simple, neither provided room for modification (it seemed) nor a place to hide one's ignorance, in contrast with the less transparent situation concerning the interaction between matter and radiation. This position did not change much until the nineteen twenties and remained one of the deepest roots of resistance to Einstein's ideas.

(3) *The impact of experiment.* The first three revolutionary papers on the old quantum theory were those by Planck (1900b), Einstein (1905a) and Bohr (1913a). All three contained proposals which flouted classical concepts. Yet the resistance to the ideas of Planck and Bohr—while certainly not absent—was much less pronounced and vehement than in the case of Einstein. Why? The answer: because of the impact of experiment.

Physicists, good physicists, enjoy scientific speculation in private, but tend to frown upon it when done in public. They are conservative revolutionaries, resisting innovation as long as possible at all intellectual cost, but embracing it when the evidence is incontrovertible—or, if they do not, physics tends to pass them by.

I often argued with Einstein about reliance on experimental evidence for confirmation of fundamental new ideas. In Sec. X, I shall have more to say about Einstein's position on this issue. Meanwhile, I shall discuss next the influence of experimental developments on the acceptance of the ideas of the three men just mentioned.

First, Planck. His proximity to the first-rate experiments on blackbody radiation being performed at the Physikalisch Technische Reichsanstalt in Berlin was beyond doubt a crucial factor in his discovery of 1900 (though it would be very wrong to say that this was the only decisive factor). In the first instance, experiment also set the pace for the acceptance of the Planck formula. One could (and did and should) doubt his derivation—as, among others, Einstein did in 1905. But at the same time neither Einstein nor any one else denied the fact that Planck's highly nontrivial universal curve admirably fitted the data. Somehow he had to be doing something right.

Bohr's paper (1913a) of April 1913 about the hydrogen atom was revolutionary and certainly not at once generally accepted. But there was no denying that his expression $2\pi^2 e^4 m/h^3 c$ for the Rydberg constant of hydrogen was remarkably accurate (to within 6%, in 1913). When, in October 1913, Bohr (1913b) was able to give an elementary derivation of the ratio of the Rydberg constants for singly ionized helium and hydrogen, in agreement with experiment to five significant figures, it became even more clear that Bohr's ideas had a great deal to do with the real world. When told of the helium/hydrogen ratio, Einstein is reported to have said of Bohr's work, "Then it is one of the greatest discoveries" (de Hevesy, 1913).

Einstein himself had little to show by comparison.

To be sure, he had mentioned a number of experimental consequences of his hypothesis in his 1905 paper. But he had no curves to fit, no precise numbers to show. He had noted that in the photoelectric effect the electron energy E is constant for fixed light frequency ν. This explained Lenard's results. But Lenard's measurements were not so precise as to prevent men like J. J. Thomson and Sommerfeld from giving alternative theories of the photoeffect of a kind in which Lenard's law does not rigorously apply (see Stuewer, 1975, Chap. 2). Einstein's photoelectric equation $E = h\nu - P$ predicts a linear relation between E and ν. At the time Einstein proposed his heuristic principle no one knew how E depended on ν beyond the fact that one increases with the other. Unlike Bohr and Planck, Einstein had to wait a decade before he saw one of his predictions vindicated, the linear E-ν relation, as was discussed in Sec. II.F. One immediate and salutary effect of these experimental discoveries was that alternative theories of the photoeffect vanished from the scene.

Yet Einstein's apartness did not end even then.

I have already mentioned that Millikan relished his result on the photo effect but declared that, even so, the light quantum theory "seems untenable" (Millikan, 1916a) In 1918, Ernest Rutherford (1871–1937) commented as follows on the Duane-Hunt results: "There is at present no physical explanation possible of this remarkable connection between energy and frequency" (Rutherford, 1918). One can go on. The fact of the matter is that, even after Einstein's photoelectric law was accepted, almost no one but Einstein himself would have anything to do with light-quanta.

This went on until the early 1920s, as is best illustrated by quoting the citation (Arrhenius, 1965) for Einstein's Nobel prize in 1921:[42] "To Albert Einstein for his services to theoretical physics and especially for his discovery of the photoelectric effect." This is not only an historic understatement but also an accurate

[42]Einstein could not attend the festivities since he was in Japan at that time. He showed his indebtedness one year later by going to Göteborg and giving an address on relativity theory.

reflection on the consensus in the physics community.

To summarize: the enormous resistance to light-quanta found its roots in the particle–wave paradoxes. The resistance was enhanced because the light-quantum idea seemed to overthrow that part of electromagnetic theory which was believed to be best understood: the theory of the free field. Moreover, experimental support was long in coming and, even after the photoelectric effect predictions were verified, light-quanta were still largely considered unacceptable. Einstein's own emphasis on the provisional nature of the light-quantum hypothesis tended to strengthen the reservations held by other physicists.

B. Spontaneous and induced radiative transitions

In 1916 Einstein (1916a) wrote to Besso: "A splendid light has dawned on me about the absorption and emission of radiation." He had obtained a deep insight into the meaning of his heuristic principle and this had led him to a new derivation of Planck's radiation law. His reasoning is contained in each one of three papers, two of which appeared in 1916 (1916b, 1916c), the third one early in 1917 (1917a). His method is based on general hypotheses about the interaction between radiation and matter. No special assumptions are made about intrinsic properties of the objects which interact with the radiation. These objects "will be called molecules in what follows" (Einstein, 1916b). (It is completely inessential to Einstein's arguments that these molecules could be Planck oscillators!)

Einstein considered a system consisting of a gas of his molecules in interaction with electromagnetic radiation. The entire system is in thermal equilibrium. Denote by E_m the energy levels of a molecule and by N_m the equilibrium number of molecules in the level E_m. Then

$$N_m = p_m e^{-E_m/kT}, \tag{44}$$

where p_m is a weight factor. Consider a pair of levels $E_m, E_n, E_m > E_n$. Einstein's hypothesis is that the total number dW of transition in the gas per time interval dt is given by

$$dW_{mn} = N_m(\rho B_{mn} + A_{mn})dt, \text{ for } m \to n, \tag{45}$$

$$dW_{nm} = N_n \rho B_{nm}\, dt, \text{ for } n \to m. \tag{46}$$

The A coefficient corresponds to spontaneous transitions $m \to n$ which occur with a probability which is independent of the spectral density ρ of the radiation present. The B terms refer to induced emission and absorption. In Eqs. (45) and (46), ρ is a function of ν and T where "we will assume that a molecule can go from the state E_n to the state E_m by absorption of radiation with a definite frequency ν, and [similarly] for emission" (Einstein, 1916b). Microscopic reversibility implies that $dW_{mn} = dW_{nm}$. Using Eq. (44) we therefore have

$$A_{mn}p_m = \rho(B_{nm}p_n e^{(E_m - E_n/kT)} - B_{mn}p_m). \tag{47}$$

(Note that the second term on the right hand side corresponds to induced emission. Thus if there were no induced emission we would obtain Wien's law.) Einstein (1916b) remarked that "the constants A and B could be computed directly, if we were to possess an electrodynamics and mechanics modified in the sense of the quantum hypothesis." That, of course, was not yet the case. He therefore continued his argument in the following way. For fixed $E_m - E_n$ and $T \to \infty$ we should get the Rayleigh–Einstein–Jeans law [Eq. (16)]. This implies that

$$B_{nm}p_n = B_{mn}p_m, \tag{48}$$

whence

$$\rho = \alpha_{mn}\,(e^{(E_m - E_n)/kT} - 1)^{-1}, \tag{49}$$

where $\alpha_{mn} \equiv A_{mn}/B_{mn}$. Then Einstein (1916b) concluded his derivation by appealing to the universality of ρ and to Wien's displacement law Eq. (4): "That α_{mn} and $E_m - E_n$ cannot depend on particular properties of the molecule but only on the active frequency ν follows from the fact that ρ must be a universal function of ν and T. Further it follows from Wien's displacement law that α_{mn} and $E_m - E_n$ are proportional to the third and first power of ν respectively. Thus one has

$$E_m - E_n = h\nu \tag{50}$$

where h denotes a constant."

The content of Eq. (50) is far more profound than a definition of the symbol ν (and h!). It is a compatibility condition. Its physical content is this: In order for Eqs. (45) and (46) to lead to Planck's law it is necessary that the transitions $m \rightleftarrows n$ are accompanied by a single monochromatic radiation quantum. By this remarkable reasoning Einstein therefore established a bridge between blackbody radiation and Bohr's theory of spectra.

About the assumptions he made in the above derivation Einstein wrote (1916b): "The simplicity of the hypotheses make it seem probable to me that these will become the basis of the future theoretical description." Once again he was right.

Two of the three papers under discussion (1916c, 1917a) contained another result which Einstein himself considered far more important than his derivation of the radiation law: light-quanta carry a momentum $h\nu/c$. This will be our next topic.[43]

C. The completion of the particle picture

A photon is a state of the electromagnetic field with the following properties.

(1) It has a definite frequency ν and a definite wave vector $\mathbf{k}$.

(2) Its energy E

$$E = h\nu, \tag{51}$$

and its momentum $\mathbf{p}$

$$\mathbf{p} = h\mathbf{k}, \tag{52}$$

[43] I can only mention in passing another paper on the quantum theory published by Einstein (1917b) in 1917. It deals with the restrictions imposed by the old quantum theory on allowed phase space orbits. Einstein examined the topological characterization of these orbits. (He never returned to this subject.)

satisfy the dispersion law

$$E=c\,|\mathbf{p}| \tag{53}$$

characteristic for a particle of zero rest mass.[44]

(3) It has spin one and (like all massless particles with nonzero spin) two states of polarization. The single particle states are uniquely specified (Wigner, 1939) by the properties (1)–(3).

The number of photons is in general not conserved in particle reactions and decays. I shall return to the non-conservation of photon number in the section on quantum statistics (Sec. VI) but would like to note here an ironical twist of history. The term "photon" first appeared in the title of a paper written in 1926.[45] The title: "The conservation of photons." The author: the distinguished physical chemist Gilbert Newton Lewis (1875–1946) from Berkeley. The subject: a speculation that light consists of "a new kind of atom.... uncreatable and indestructible [for which] I.... propose the name photon" (Lewis, 1926). This idea was soon forgotten but the new name almost immediately became part of the language. In October 1927 the fifth Solvay conference was held. Its subject was "Electrons et Photons."

When Einstein introduced light-quanta in 1905, these were *energy* quanta satisfying Eq. (51). There was no mention in that paper of Eqs. (52) and (53). In other words, the full-fledged particle concept embodied in the term photon was not there all at once. For this reason I make the distinction between light-quantum ("$E=h\nu$ only") and photon in this section. The dissymmetry between energy and momentum in the 1905 paper is of course intimately connected with the origins of the light-quantum postulate in equilibrium statistical mechanics. In the statistical mechanics of equilibrium systems one derives important relations between the overall energy and other macroscopic variables. The overall momentum plays a trivial and subsidiary role. These distinctions between energy and momentum are much less pronounced when one considers fluctuations around the equilibrium state. It was via the analysis of statistical fluctuations of blackbody radiation that Einstein eventually came to associate a definite momentum with a light-quantum. This happened in 1917. Before I describe what he did I should again draw the attention of the reader to the remarkable fact that it took the father of special relativity theory twelve years to write down the relation $p=h\nu/c$ side by side with $E=h\nu$. I shall have more to say about this in Sec. X.C.

Now, once again, to the fluctuations. To begin with I copy a formula (discussed earlier in Sec. II.G) which was derived by Einstein (1909a, 1909b) in 1909:

$$\langle \varepsilon^2(\nu, T)\rangle = [\rho h\nu + c^3\rho^2/8\pi\nu^2]v\,d\nu\,. \tag{37}$$

Recall that $\langle\varepsilon^2\rangle$ is the energy fluctuation in the interval $d\nu$ referring to a subvolume v of a cavity filled with radiation in thermal equilibrium; and that Eq. (37) holds if ρ is given by Planck's formula (6). For our purposes it is important to note a second fluctuation formula which is found in the same 1909 papers. This one deals with momentum fluctuations and is pertinent to the question of photon momentum. Einstein considered the case of a plane mirror with mass m and area f placed inside the cavity. The mirror moves perpendicular to its own plane and has a velocity v at time t. During a small time interval from t to $t+\tau$ its momentum changes from mv to $mv-Pv\tau+\Delta$. The second term describes the drag force due to the radiation pressure (P is the corresponding friction constant). This force would eventually bring the mirror to rest where it not for the momentum fluctuation term Δ induced by the fluctuations of the radiation pressure. In thermal equilibrium the mean square momentum $m^2\langle v^2\rangle$ should remain unchanged over the interval τ. Hence[46] $\langle\Delta^2\rangle = 2mP\tau\langle v^2\rangle$. The equipartition law applied to the kinetic energy of the mirror implies that $m\langle v^2\rangle = kT$. Hence

$$\langle\Delta^2\rangle = 2P\tau kT\,. \tag{51}$$

Einstein computed P in terms of ρ for the case in which the mirror is fully transparent for all frequencies except those between ν and $\nu+d\nu$, which it reflects perfectly. Using Planck's expression for ρ he found that

$$\langle\Delta^2\rangle = \frac{1}{c}\left[\rho h\nu + \frac{c^3\rho^2}{8\pi\nu^2}\right]f\tau\,d\nu\,. \tag{52}$$

The parallels between Eqs. (37) and (52) are striking. The respective first terms dominate if $h\nu/kT \gg 1$, the regime in which ρ is approximated by Wien's exponential law. The first term in Eq. (37) extends Einstein's "energy quantum postulate" of 1905 to energy fluctuations. One might expect that the first term in Eq. (52) would lead Einstein to state, in 1909, the "momentum quantum postulate": Monochromatic radiation of low density behaves in regard to pressure fluctuations as if it consists of mutually independent momentum quanta of magnitude $h\nu/c$. It is unthinkable to me that Einstein did not think so. But he did not quite say so.

This is what he did say (1909a): "If the radiation were to consist of very few extended complexes with energy $h\nu$ which move independently through space and which are independently reflected—a picture which represents the roughest visualization of the light-quantum hypothesis—then as a consequence of fluctuations in the radiation pressure such momenta would act on our plate as are represented by the first term only of our formula [(Eq. 52)]." He did not refer explicitly to momentum quanta nor to the relativistic connection between $E=h\nu$ and $p=h\nu/c$. Yet a particle concept (the photon) was clearly on his mind since he went on to conjecture (1909a) that "the electromagnetic fields of

[44]There have been occasional speculations that the photon might have a tiny nonzero mass. Direct experimental information on the photon mass is therefore a matter of interest. The best determinations of this mass come from astronomical observations. The present upper bound (Davis, Goldhaber, and Nieto, 1975) is $8\cdot 10^{-49}$ g. In what follows the photon mass is taken to be strictly zero.

[45]For ease I shall often use the term "photon" also in referring to times prior to 1926.

[46]Terms $0(\tau^2)$ are dropped. $\langle v\Delta\rangle = 0$ since v and Δ are uncorrelated.

light are linked to singular points similar to the occurrence of electrostatic fields in the theory of electrons." It seems fair to paraphrase this statement as follows: light-quanta may well be particles in the same sense that electrons are particles. The association between the particle concept and a high degree of spatial localization is typical for that period. It is of course not correct in general.

The photon momentum made its explicit appearance in that same year, 1909. Johannes Stark (1874–1957) had attended the Salzburg meeting at which Einstein (1909a) had discussed the radiative fluctuations. A few months later Stark (1909) stated that according to the light-quantum hypothesis, "the total electromagnetic momentum emitted by an accelerated electron is different from zero and.... in absolute magnitude is given by $h\nu/c$." As an example he mentioned bremsstrahlung for which he wrote down the equation

$$m_1\mathbf{v}_1+m_2\mathbf{v}_2=m_1\mathbf{v}_1'+m_2\mathbf{v}_2'+\frac{h\nu}{c^2}\mathbf{c}\,, \tag{53}$$

the first ocasion on record in which the photon enters explicitly into the law of momentum conservation for an elementary process.

Einstein himself did not explicitly introduce photon momentum until 1916, in the course of his studies on thermal equilibrium between electromagnetic radiation and a molecular gas (Einstein, 1916c, 1917a). In addition to his new discussion of Planck's law Einstein raised the following problem. In equilibrium the molecules have a Maxwell distribution for the translational velocities. How is this distribution maintained in time considering the fact that the molecules are subject to the influence of radiation pressure? In other words, what is the Brownian motion of molecules in the presence of radiation?

Technically the following issue arises. If a molecule emits or absorbs an amount ε of radiative energy all of which moves in the same direction, then it experiences a recoil of magnitude ε/c. There is no recoil if the radiation is not directed at all, as for a spherical wave. Question: what can one say about the degree of directedness of the emitted or absorbed radiation for the system under consideration? Einstein began the discussion of this question in the same way he had treated the mirror problem in 1909. Instead of the mirror he now considered molecules which all move in the same direction. Then there is again a drag force $-Pv\tau$ and a fluctuation term Δ. Equipartition gives again $m\langle v^2\rangle=kT$ and one arrives once more at Eq. (51). Next comes the issue of compatibility. With the help of Eqs. (45) and (46) Einstein could compute separately expressions for $\langle\Delta^2\rangle$ as well as for P in terms of the A's, B's, and ρ, where ρ is now given by Planck's law.[47] I shall not reproduce the details of these calculations but do note the crux of the matter. In order to obtain the same answer for the quantities on the lhs and rhs of Eq. (51) Einstein (1917a) had to invoke a condition of directedness: "If a bundle of radiation causes a molecule to emit or absorb an energy amount $h\nu$ then a momentum $h\nu/c$ is transferred to the molecule, directed along the bundle for absorption and oppositely to the bundle for [induced] emission." (The question of spontaneous emission is discussed below.) Thus Einstein found that consistency with the Planck distribution [and Eqs. (45), (46)] requires that the radiation be fully directed. (One often called this *Nadelstrahlung*.) And so with the help of his trusted and beloved fluctuation methods Einstein once again produced a major insight, the association of momentum quanta with energy quanta. Indeed if we leave aside the question of spin[48] we may say that *Einstein abstracted not only the light-quantum but also the more general photon concept entirely from statistical mechanical considerations.*

D. Earliest Unbehagen about chance

Einstein prefaced his statement about photon momentum which I just quoted with the remark that this conclusion can be considered "als ziemlich sicher erwiesen," ("as fairly certainly proven"). If he had some lingering reservations, this was mainly because he had derived some of his equations (1917a) on the basis of "the quantum theory [which is] incompatible with the Maxwell theory of the electromagnetic field." Moreover, his momentum condition was a sufficient, not a necessary condition, as was emphasized by Wolfgang Pauli (1900-1958) in a review article (Pauli, 1964, p. 630) completed in 1924: "From Einstein's considerations it could... not be seen with complete certainty that his assumptions were the only ones which guarantee thermodynamic—statistical equilibrium." Nevertheless his 1917 results led Einstein to drop his caution and reticence about light-quanta. They had become real to him. In a letter to Besso about the needle rays he wrote (Einstein, 1916d): "Damit sind die Lichtquanten so gut wie gesichert."[49] And, in a phrase contained in another letter about two years later (Einstein, 1918), "... I do not doubt anymore the *reality* of radiation quanta, although I still stand quite alone in this conviction", he underlined the word "*Realität*."

On the other hand, at about the same time that Einstein lost any remaining doubts about the existence of light-quanta we also encounter the first expressions of his Unbehagen, his discomfort with the theoretical implications of the new quantum concepts in regard to "Zufall," "chance".

This earliest unease stemmed from the conclusion concerning spontaneous emission which Einstein had been forced to draw from his consistency condition (51): The needle ray picture applies not only to induced processes (as was mentioned above) but also to spontaneous emission. That is, in a spontaneous radiative transition the molecule suffers a recoil $h\nu/c$. However,

[47] In 1910, Einstein had made a related calculation, together with Ludwig Hopf (1884–1939) (Einstein and Hopf, 1910). At that time he used the classical electromagnetic theory to compute $\langle\Delta^2\rangle$ and P. This cast Eq. (51) into a differential equation for ρ. Its solution is Eq. (16).

[48] Some comments on the photon spin will be found in Sec. VI.

[49] "With that [the existence of] the light-quanta is practically certain."

the recoil direction cannot be predicted! Einstein (1917a) stressed (quite correctly of course) that it is "a weakness of the theory... that it leaves time and direction of elementary processes to chance." What decides when the photon is spontaneously emitted? What decides in which direction it shall go?

These questions were not new. They also apply to another class of emission processes, the spontaneity of which had puzzled physicists since the turn of the century: radioactive transformations. A spontaneous emission coefficient was in fact first introduced by Rutherford (1900) in 1900 when he wrote down[50] the equation $dN = -\lambda N dt$ for the decrease of the number N of radioactive thorium emanation atoms in the time interval dt. Einstein himself (1916b) drew attention to this similarity: "It speaks in favor of the theory that the statistical law assumed for [spontaneous] emission is nothing but the Rutherford law of radioactive decay." I have written elsewhere (Pais, 1977) about the ways physicists responded to this baffling lifetime problem. I should now add that Einstein was the first to realize that the probability for spontaneous emission is a nonclassical quantity. No one before Einstein in 1917 saw as clearly the depth of the conceptual crisis generated by the occurrence of spontaneous processes with a well-defined (partial) lifetime. He expressed this in prophetic terms: "The properties of elementary processes required by [Eq. (51)] make it seem almost inevitable to formulate a truly quantized theory of radiation" (Einstein, 1917a).

Immediately following his comment on "chance", to which I have just referred, Einstein (1917a) continued: "Nevertheless I have full confidence in the route which has been taken." If Einstein was confident at that time about the route, he also felt strongly that it would be a long one. The chance character of spontaneous processes meant that something was amiss with classical causality. That would forever deeply trouble him. As early as March 1917 Einstein (1917c) had written on this subject to Besso: "I feel that the real joke which the eternal inventor of enigmas has presented us with has absolutely not been understood as yet." It is believed by nearly all of us that the joke was understood soon after 1925 when it became possible to calculate Einstein's $A_{mn}\gamma$ and B_{mn} from first principles. As I shall discuss below, Einstein eventually accepted these principles but never considered them to be *first* principles. Einstein's attitude throughout the rest of his life was: the joke has not been understood as yet. One further example may show how from 1917 on Einstein could not make his peace with the quantum theory. In 1920 he wrote as follows to Born. "That business about causality causes me a lot of trouble too. Can the quantum absorption and emission of light ever be understood in the sense of the complete causality requirement, or would a statistical residue remain? I must admit that there I lack the courage of a conviction. But I would be very unhappy to renounce complete causality" (Einstein, 1920a).

[50]Here a development began which, two years later, culminated in the transformation theory for radioactive substances (Rutherford and Soddy, 1902).

E. The Compton effect

We now come to the dénouement of the photon story.

Since, after 1917, Einstein firmly believed that light-quanta were here to stay, it is not surprising that he would look for new ways in which the existence of photons might lead to observable deviations from the classical picture. In this he did not succeed. At one point, in 1921, he thought he had found a new quantum criterion (Einstein, 1921), but it soon turned out to be a false lead (Einstein, 1922; Klein, 1970). In fact, after 1917 nothing particularly memorable happened in regard to light-quanta until capital progress was achieved[51] when Arthur Compton (1892-1962) (Compton, 1923) and Debye (1923) independently wrote down the relativistic kinematics for the scattering of a photon on an electron at rest:

$$h\vec{k} = \vec{p} + h\vec{k}' , \tag{54}$$

$$hc|\vec{k}| + mc^2 = hc|\vec{k}'| + (c^2p^2 + m^2c^4)^{1/2} . \tag{55}$$

Why were these elementary equations not published five or even ten years earlier, as well they could have been? Even those opposed to quantized radiation might have found these relations to their liking since (independent of any quantum dynamics) they yield at once significant differences from the classical theories of the scattering of light by matter[52] and, therefore, provide simple tests of the photon idea.

I have no entirely satisfactory answer to this question. In particular it is not clear to my why Einstein himself did not consider these relations. However, there are two obvious contributing factors. First, because photons were rejected out of hand by the vast majority of physicists, few may have felt compelled to ask for tests of an idea they did not believe to begin with. Secondly, it was only in about 1922 that strong evidence became available for deviations from the classical picture. This last circumstance impelled both Compton and Debye to pursue the quantum alternative.[53] Debye (1923), incidentally, mentioned his indebtedness to Einstein's work on needle radiation. Compton in his paper does not mention Einstein at all.[54]

The same paper in which Compton discussed Eqs. (54) and (55) also contains the result of a crucial experiment. These equations imply that the wavelength difference $\Delta\lambda$ between the final and the initial photon is

[51]Einstein (1924a) attached great importance to an advance in another direction which took place in the intervening years: the effect discovered by Otto Stern (1888–1969) and Walther Gerlach (1889–1979). Together with Ehrenfest he made a premature attempt at its interpretation (Einstein and Ehrenfest, 1922).

[52]For details on these classical theories see Stuewer's fine monograph (1975) on the Compton effect.

[53]Nor is it an accident that these two men came forth with the photon kinematics at about the same time. In his paper Debye (1923) acknowledges a 1922 report by Compton in which the evidence against the classical theory was reviewed. A complete chronology of these developments in 1922 and 1923 is found in Stuewer (1975), p. 235.

[54]For a detailed account of the evolution of Compton's thinking see Stuewer (1975), Chapter 6.

given by

$$\Delta\lambda = (h/mc)(1-\cos\theta), \tag{56}$$

where θ is the photon scattering angle. Compton found this relation to be satisfied within the error.[55] The quality of the experiment is well demonstrated by the value he obtained for the Compton wavelength: $h/mc \simeq 0.0242$ Å which is within less than one percent from the modern value. (For the current state of the subject see Williams, 1977). Compton concluded: "The experimental support of the theory indicates very convincingly that a radiation quantum carries with it directed momentum as well as energy."[56]

This discovery "created a sensation among the physicists of that time" (Allison, 1965). There were the inevitable controversies surrounding a discovery of such major proportions. Nevertheless the photon idea was rapidly accepted. Sommerfeld incorporated the Compton effect in his new edition (1924) of *Atombau und Spektrallinien* with the comment: "It is probably the most important discovery which could have been made in the current state of physics."

What about Einstein's own response? A year after Compton's experiments he wrote a popular article for the Berliner Tageblatt (Einstein, 1924b) which ends as follows: "The positive result of the Compton experiment proves that radiation behaves as if it consisted of discrete energy projectiles, not only in regard to energy transfer but also in regard to *Stosswirkung* (*momentum transfer*)." Here then, in projectile (that is, particle) language, is the "momentum quantum postulate," phrased in close analogy to the energy quantum postulate of 1905. In both cases we encounter the phraseology: "Radiation... behaves... as if it consists of...."

Still, Einstein was not (and would never be) satisfied. There was as yet no real theory. In the same article he also wrote: "There are therefore now two theories of light, both indispensable, and—as one must admit today in spite of twenty years of tremendous effort on the part of theoretical physicists—without any logical connection."

The years 1923–1924 mark the end of the first phase of Einstein's apartness in relation to the quantum theory. Yet there remained one important bastion of resistance to the photon, centering around Niels Bohr.

V. INTERLUDE: THE BKS PROPOSAL

Sie haben sich heiss und innig geliebt.
Helen Dukas.

In January 1924 Niels Bohr, Hendrik Anton Kramers, (1894–1952) and John Clarke Slater (1900–1976) submitted to the Philosophical Magazine an article (Bohr, Kramers, and Slater, 1924) which contained drastic theoretical proposals concerning the interaction of light with matter. It was written after Compton's discovery, yet it rejected the photon. It was written also after Einstein and Bohr had met. This section on the BKS proposal serves a twofold purpose. It is a postscript to the story of the photon and a prelude to the Bohr–Einstein dialog which will occupy us more fully in later sections.

I have already mentioned that Einstein was immediately and strongly impressed by Bohr's work of 1913. They did not yet know each other at that time. A number of years was to pass before their first encounter; meanwhile they followed each others' published work. Also, Ehrenfest kept Einstein informed of the progress in Bohr's thinking. "Ehrenfest tells me many details from Niels Bohr's Gedankenküche, [thought kitchen]; he must be a very first-rate mind, extremely critical and far seeing, which never loses track of the grand design" (Einstein, 1919). Einstein remained forever deeply respectful of Bohr's pioneering work. When he was nearly seventy he wrote (Einstein, 1949a): "That this insecure and contradictory foundation [of physics in the teens] was sufficient to enable a man of Bohr's unique instinct and tact to discover the major laws of the spectral lines and of the electron shells of the atoms together with their significance for chemistry appeared to me like a miracle—and appears to me as a miracle even today. This is the highest form of musicality in the sphere of thought."

Einstein and Bohr finally met in the spring of 1920, in Berlin. At that time they both had already been widely recognized as men of destiny who would leave their indelible marks on the physics of the twentieth century. The impact of their encounter was intense and went well beyond only a meeting of minds. Shortly after his visit, Einstein (1920b) wrote to Bohr: "Not often in life has a human being caused me such joy by his mere presence as you did." Bohr (1920) replied: "To meet you and to talk with you was one of the greatest experiences I have ever had." Some years later Einstein (1923a) began a letter to Bohr as follows: "Lieber oder vielmehr geliebter Bohr." ("Dear or rather beloved Bohr.") Once when I talked with Helen Dukas, Einstein's devoted secretary, about the strong tie between these two men[57] she made the comment which is at the head of this section. Its literal translation would distort its meaning. Rendered more freely it means: "They were deeply involved with one another in affectionate ways."

All who have known Bohr will be struck by this characterization which Einstein (1954a) gave of him much later: "He utters his opinions like one perpetually groping and never like one who believes to be in the possession of definite truth." Bohr's style of writing makes clear for all to see how he groped and struggled. "Never express yourself more clearly than you think," he used to admonish himself and others. Bohr's articles are sometimes dense. Having myself assisted him

[55] K line x rays from a molybdenum anticathode were scattered off graphite. Compton stressed that one should only use light elements as scatterers so that the electrons will indeed be quasifree. Scattered x rays at 45°, 90°, and 135° were analyzed.

[56] The work of Compton and Debye led Pauli (1923) to extend Einstein's work of 1917 to the case of radiation in equilibrium with free electrons. Einstein and Ehrenfest (1923) subsequently discussed the connection between Pauli's and Einstein's Stosszahlansatz.

[57] This discussion took place in June 1978. Miss Dukas started to work for Einstein in April 1928.

on a number of occasions when he was attempting to put his thoughts on paper, I know to what enormous lengths he went to find the most appropriate turn of phrase. I have no such first-hand information about the way Einstein wrote. But, again for all to see, there are his papers, translucent. The early Einstein papers are brief, their content is simple, their language is sparse. They exude finality even when they deal with a subject in flux. For example, no statement made in the 1905 paper on light-quanta needs to be revised in the light of later developments. Whether he published in German or English, he initially wrote in German. He had a delicate musical sense of language and a keen insight into people, as his description of Bohr illustrates.

Their meeting in 1920 took place some years before they found themselves at scientific odds on profound questions of principle in physics. They did not meet very often in later times. They did correspond but not voluminously. I was together a few times with both of them some thirty years after their first encounter, when their respective views on the foundations of quantum mechanics had long since become irreconcilable. More about that later. Let me note here only that neither the years nor later events ever diminished the mutual esteem and affection in which they held one another.

Let us now turn to the BKS proposal.

As I have already stressed in Sec. IV.A, it was the position of most theoretical physicists during the first decades of the quantum era that the conventional continuous description of the free radiation field should be protected at all cost and that the quantum puzzles concerning radiation should eventually be resolved by a revision of the properties of interaction between radiation and matter. The BKS proposal represents the extreme example of this position. Its authors suggested that radiative processes have highly unconventional properties "the cause of [which] we shall not seek in any departure from the electrodynamic theory of light as regards the laws of propagation in free space, but in the peculiarities of the interaction between the virtual field of radiation and the illuminated atoms" (Bohr, Kramers, and Slater, 1924). Before describing these properties I should point out that the BKS paper represents a program rather than a detailed research report. In contains no formalism whatsoever.[58] This program was not to be the right way out of the difficulties of the old quantum theory, yet the paper had a lasting impact in that (as we shall see) it stimulated important experimental developments. Let us discuss next the two main paradoxes which BKS addressed.

The first paradox. Consider an atom which emits radiation in a transition from a higher to a lower state. BKS assume that in this process "energy [is] of two kinds, the continuously changing energy of the field and the discontinuously changing atomic energy" (Slater, 1925). But how can there be conservation of an energy which consists of two parts, one changing discontinuously, the other continuously? The BKS answer (Bohr, Kramers, and Slater, 1924): "As regards the occurrence of transitions, which is the essential feature of the quantum theory, we abandon... a direct application of the principles of conservation of energy and momentum." Energy and momentum conservation, they suggested, does not hold true for individual elementary processes but should only hold statistically, as an average over many such processes.

The idea of energy nonconservation had already been on Bohr's mind a few years prior to the time of the BKS proposal (Bohr, 1923, especially Sec. 4).[59] However, it was not Bohr but Einstein who had first raised—and rejected—this possibility. In 1910 Einstein wrote to a friend (see Seelig, 1954, p. 137). "At present I have high hopes for solving the radiation problem and that without light-quanta. I am enormously curious how it will work out. One must renounce the energy principle in its present form." A few days later he was disenchanted: "Once again the solution of the radiation problem is getting nowhere. The devil has played a rotten trick on me" (Seelig, 1954, p. 137). He raised the issue one more time at the 1911 Solvay meeting, noting that his formula for the energy fluctuations of blackbody radiation could be interpreted in two ways: "One can choose between the [quantum] structure of radiation and the negation of an absolute validity of the energy conservation law." He rejected the second alternative. "Who would have the courage to make a decision of this kind?... We will agree that the energy principle should be retained" (Einstein, 1912d, pp. 429, 436). But others were apparently not as convinced. In 1916 the suggestion of statistical energy conservation was taken up by Nernst (1916).[60] Not later than January 1922 Sommerfeld (1922) remarked that the "mildest cure" for reconciling the wave theory of light with quantum phenomena would be to relinquish energy conservation. (See Klein, 1970, for similar speculations by other physicists.) Thus the BKS proposal must be regarded as an attempt to face the consequences of an idea that had been debated for quite some time.

In order to understand Bohr's position in 1924 it is important above all to realize that the correspondence principle was to him the principal reliable bridge between classical and quantum physics. But the correspondence principle is of course no help in understanding light-quanta: the controversial issue of photons versus waves lies beyond this principle. To repeat, the photon-wave duality was the earliest known instance of what was later to be called a complementary situation. The BKS theory, with its rejection of photons and its insistence on the continuous picture of light at the price of nonconservation, historically represents the last stand of the old quantum theory. For very good reasons this

[58]The same is true for a sequel to this paper which Bohr (1925a) wrote in 1925. Schroedinger (1924) and especially Slater (1925) did make attempts to put the BKS ideas on a more formal footing. See also Slater's own recollections (1967) of that period.

[59]A letter from Ehrenfest (1922) to Einstein shows that Bohr's thoughts had gone in that direction at least as early as 1922.

[60]The title of Nernst's paper is (in translation): "On an attempt to revert from quantum-mechanical considerations to the assumption of continuous energy changes."

proposal was characterized some years later (Heisenberg, 1929) by one of the principal architects of the quantum mechanics as representing the height of the crisis in the old quantum theory. Nor was nonconservation of energy and momentum in individual processes the only radical proposal made by BKS.

The second paradox. This one concerns a question which had troubled Einstein since 1917 (as we have seen): How does an electron know *when* to emit radiation in making a spontaneous transition?

In its general form the BKS answer to this question was: there is no *truly* spontaneous emission. They associated with an atom in a given state a "virtual radiation field" which contains all the possible transition frequencies to other stationary states and assumed that "the transitions which in [the Einstein theory of 1917] are designated as spontaneous are, on our view, *induced* [my italics] by the virtual field." According to BKS, the spontaneous transition to a specific final state is connected with the virtual field mechanism "by probability laws which are analogous to those which in Einstein's theory hold for induced transitions." In this way "the atom is under no necessity of knowing what transition it is going to make ahead of time" (Slater, 1925). Thus spontaneous emission is ascribed to the action of the virtual field, but this action is noncausal. I shall not discuss details of the BKS picture of induced emission and absorption and other radiative processes. Suffice it to say that all of these are supposed to be due to virtual fields and that in all of these causality is abandoned. In a paper completed later in 1924 Slater (1925) noted that the theory "has unattractive features.... [but] it is difficult at the present stage to see how [these are] to be avoided."

But what about the Compton effect? The successfully verified Eq. (56) rests on the conservation laws (54) and (55). However (BKS argued), these equations do hold in the average and the experiment on $\Delta\lambda$ refers only to the average change of the wavelength. In fact at the time of the BKS proposal *there did not exist any direct experimental proof of energy-momentum conservation nor of causality in any individual process.* This is one of the reasons why the objections to BKS (held by many, "perhaps the majority" (Pauli, 1924), of physicists[61]) were initially expressed in a somewhat muted fashion. Thus Pauli (1924) wrote to Bohr that he did not believe in his theory but that "one cannot prove anything logically and also the available data are not sufficient to decide for or against your view." All this was to change soon.

There was a second reason, I believe, for the subdued character of comments by others. The physics community was witness to a rare occurrence. Here were the two leading authorities of the day locked in conflict. (The term conflict was used by Einstein himself.[62]) To take sides meant to choose between the two most revered physicists. Ideally, personal considerations of this kind ought to play no role in matters scientific. But this ideal is not always fully realized. Pauli (1924) reflected on this in a letter concerning the BKS issue: "Even if it were psychologically possible for me to form a scientific opinion on the grounds of some sort of belief in authority (which is not the case, however, as you know), this would be logically impossible (at least in this case) since here the opinions of two authorities are so very contradictory."

Even the interaction between the two protagonists was circumspect during that period. They did not correspond on the BKS issue (Einstein, 1924c). Nor (as best I know) were there personal meetings between them in those days even though Bohr had told Pauli repeatedly how much he would like to know Einstein's opinion (Pauli, 1924). Werner Heisenberg (1901–1976) wrote (1924) to Pauli that he had met Einstein in Göttingen and that the latter had "a hundred objections." Sometime later Pauli also met Einstein whereupon he sent Bohr a detailed list of Einstein's criticisms (Pauli, 1924).

Einstein of course never cared for BKS. He had given a colloquium on this paper at which he had raised objections. The idea (he wrote Ehrenfest (Einstein, 1924d)) "is an old acquaintance of mine, which I do not hold to be the real fellow however" ("... den ich aber für Keinen reellen Kerl halte.") At about that time he drew up a list of nine objections (Einstein, 1924e) which I shall not reproduce here in detail. Samples: "what should condition the virtual field which corresponds to the return of a previously free electron to a Bohr orbit? (very questionable)... Abandonment of causality *as a matter of principle* should only be permitted in the most extreme emergency." The causality issue (which had plagued him already for seven years by then) was clearly the one to which he took exception most strongly. He confided to Born (Einstein, 1924f) that the thought was unbearable to him that an electron could choose freely the moment and direction in which to move. The causality question would continue to nag him long after experiment revealed that the BKS answers to both paradoxes were incorrect.

The experimental verdict on causality. The BKS ideas stimulated Walther Bothe (1891–1957) and Hans Geiger (1882–1945) (Bothe and Geiger, 1924) to develop counter coincidence techniques for the purpose of measuring whether (as causality demands) the secondary photon and the knocked-on electron are produced simultaneously in the Compton effect. Their result (Bothe and Geiger, 1925a, 1925b): these two particles are both created in a time interval $\leq 10^{-3}$ sec. Within the limits of accuracy causality had been established and the randomness (demanded by BKS) of the relative creation times disproved. Since then this time interval has been narrowed down experimentally (Bay, Henri, and McLennon, 1955) to $\leq 10^{-11}$ sec.

The experimental verdict on energy-momentum conservation. The validity of these conservation laws in individual elementary processes was established for the Compton effect by Compton and A. W. Simon. From cloud chamber observations on photoelectrons and knock-on electrons they could verify (Compton and Simon, 1925) the validity of the relation

[61]Born, Schroedinger, and R. Ladenburg were among the physicists who initially believed that BKS might be a step in the right direction.

[62]On October 25, 1924, the Danish newspaper Politiken carried a news item on the Bohr–Einstein controversy. This led the editor of a German newspaper to send a query to Einstein (Joel, 1924). Einstein (1924c) sent a brief reply acknowledging that a conflict existed, but also stating that no written exchanges between himself and Bohr had ensued.

$$\tan\phi = -\left\{\left(1 + \frac{h\nu}{mc^2}\right)\tan\tfrac{1}{2}\theta\right\}^{-1} \tag{57}$$

in individual events, where ϕ, θ are the scattering angles of the electron and photon, respectively, and ν is the incident frequency.

And so the last resistance to the photon came to an end. Einstein's views had been fully vindicated. The experimental news was generally received with great relief (see, e.g., Pauli, 1925a).[63] Bohr (1925b) took the outcome in good grace and proposed "to give our revolutionary efforts as honourable a funeral as possible." He was now prepared for an even more drastic resolution of the quantum paradoxes. In July 1925 he wrote (1925c): "One must be prepared for the fact that the required generalization of the classical electrodynamical theory demands a profound revolution in the concepts on which the description of nature has until now been founded."

These remarks by Bohr end with references to de Broglie's thesis and also to Einstein's work on the quantum gas (the subject of the next section): the profound revolution had begun.

VI. A LOSS OF IDENTITY: THE BIRTH OF QUANTUM STATISTICS

A. Boltzmann's axiom

This episode begins with a letter (Bose, 1924a) dated June 1924, written by a young Bengali physicist from the University of Dacca, now in Bangladesh. His name was Satyendra Nath Bose (1894–1974). The five papers he had published by then were of no particular distinction. The subject of his letter was his sixth paper. He had sent it to the Philosophical Magazine. A referee had rejected it (Blanpied, 1972). Bose's letter was addressed to Einstein, then forty-five years old, and already recognized as a world figure by his colleagues and by the public at large. In this section, I shall describe what happened in the scientific lives of these two men during the half-year following Einstein's receipt of Bose's letter. For Bose the consequences were momentous. From virtually an unknown he became a physicist whose name will always be remembered. For Einstein this period was only an interlude.[64] Already he was deeply engrossed in a project which he was not to complete: his search for a unified field theory. Such is the scope of Einstein's oeuvre that his discoveries in that half-year do not even rank among his five main contributions, yet they alone would have sufficed to remember Einstein forever as well.

Bose's sixth paper deals with a new derivation of Planck's law. Along with his letter he had sent Einstein a copy of his manuscript, written in English, and asked him to arrange for publication in the Zeitschrift für Physik, if he were to think the work of sufficient merit. Einstein acceded to Bose's request. He personally translated the paper into German and submitted it to the Zeitschrift für Physik. He also added this translator's note: "In my opionion Bose's derivation of the Planck formula constitutes an important advance. The method used here also yields the quantum theory of the ideal gas, as I shall discuss elsewhere in more detail."

It is not the purpose of this Section to discuss the history of quantum statistics but rather to describe Einstein's contribution to the subject. Nevertheless I include a brief outline of Bose's work, in part (B). There are numerous reasons for doing so. (1) It will give us some insight into what made Einstein diverge temporarily from his main pursuits. (2) It will facilitate the account of Einstein's own research on the molecular gas. This work is discussed in part (C), with the exception of one major point which is reserved for the next section: Einstein's last encounter with fluctuation questions. (3) It will be of help to explain Einstein's ambivalence to Bose's work. In a letter to Ehrenfest, written in July, Einstein (1924g) did not withdraw, but rather qualified, his praise of Bose's paper: Bose's "derivation is elegant but the essence remains obscure." (4) It will help to make clear how novel the photon concept still was at that time and it will throw an interesting sidelight on the question of photon spin.

Bose recalled, many years later, that he had not been aware of the extent to which his paper defied classical logic.[65] "I had no idea that what I had done was really novel. . . I was not a statistician to the extent of really knowing that I was doing something which was really different from what Boltzmann would have done, from Boltzmann statistics. Instead of thinking of the light-quantum just as a particle, I talked about these states. Somehow this was the same question which Einstein asked when I met him[66]: How had I arrived at this method of deriving Planck's formula?" (Mehra, 1975). In order to answer Einstein's question it is necessary (though it may not be sufficient) to understand what gave Bose the idea that he was doing what Ludwig Boltzmann would have done. This in turn demands a brief digression on classical statistics.

Suppose I show someone two identical balls lying on a table. Next I ask him to close his eyes and a few moments later to open them again. I then ask him whether or not I have meanwhile exchanged the two balls. He cannot tell, since the balls are identical. Yet I do know the answer. If I have exchanged the balls then I have been able to follow the *continuous* motion which brought the balls from the initial to the final configuration. This simple example illustrates Boltzmann's first axiom of classical mechanics which says, in essence, that identical particles which cannot come infinitely close to each other can be distinguished by their initial conditions and by the continuity of their motion. This assumption, Boltzmann (1897) stressed, "gives us the sole possibility of recognizing the same material point at different times." As Schroedinger (1955) has emphasized, "Nobody before Boltzmann held it necessary to define what one means by [the term] the same material point."

[63]Pauli's description of BKS, written early in 1925, is found in Pauli (1964) pp. 83–86.

[64]In 1925 Einstein said of his work on quantum statistics: "That's only by the way" (Salaman, 1979).

[65]Such a lack of awareness is not uncommon in times of transition. But it is not the general rule. Einstein's light-quantum paper of 1905 is an example of a brilliant exception.

[66]In October or November 1925.

Thus we may speak classically of a gas with energy E consisting of N identical distinguishable molecules. Let there be n_1 particles with energy $\varepsilon_1, \ldots n_i$ particles with energy $\varepsilon_i, \ldots$ so that

$$N=\sum n_i, \quad E=\sum n_i \varepsilon_i . \tag{58}$$

The number of states corresponding to this partition is given by

$$w=N!\left(\prod_i n_i!\right)^{-1}, \tag{59}$$

where the factors $n_i!$ reflect the application of Boltzmann's axiom. These states are the microstates of the system and the description in terms of them is the so-called fine-grained description. For the limited purpose of analyzing the macroscopic properties of the system one contracts this description to the so-called coarse-grained description.

Divide the one-particle phase space into cells $\omega_1, \omega_2, \ldots$ such that a particle in ω_A has the *mean* energy E_A. Partition the N particles such that there are N_A particles in ω_A,

$$N=\sum N_A, \tag{60}$$

$$E=\sum N_A E_A . \tag{61}$$

The set (N_A, E_A) defines a coarse-grained state. For the special case of an ideal gas the relative probability of this state is given by

$$W=N! \prod_A \frac{\omega_A^{N_A}}{N_A!} . \tag{62}$$

Equation (62) rests on two independent ingredients: (1) Boltzmann's distinguishability axiom and (2) statistical independence; that is, for an ideal classical gas the individual molecules have no a priori preference for any particular region in phase space. Boltzmann's principle states that in thermodynamic equilibrium the entropy is given by

$$S=k \ln W_{\max}+C, \tag{63}$$

where C is an arbitrary additive constant; that is, C does not depend on the n_i. $W_{\max}$ is the maximum of W as a function of the N_A subject to the constraints (60), (61). The classical Boltzmann distribution then follows from the extremal conditions

$$\sum \delta N_A (\ln \omega_A - \ln N_A + \lambda + \beta^{-1} E_A)=0, \tag{64}$$

and $\beta=kT$ follows from $\partial S/\partial E = T^{-1}$. All these elementary facts have been recalled in order to accentuate the differences between classical and quantum statistics.

Both logically and historically classical statistics developed via the sequence:

Fine-grained counting $\rightarrow$ coarse-grained counting.

This is of course the logic of quantum statistics as well, but its historical development went the reverse way, from coarse-grained to fine-grained. For the oldest quantum statistics, the Bose-Einstein (BE) statistics, the historical order of events was as follows.

(a) 1924–5: Introduction of a new coarse grained counting by first Bose, then Einstein.

(b) 1925–6: Discovery of nonrelativistic quantum mechanics. It is not at once obvious (see Heisenberg, 1926a) how one should supplement the new theory with a fine-grained counting principle which leads to BE statistics.

(c) 1926. This principle is discovered by Paul Adrien Maurice Dirac (1902–): Equation (59) is to be replaced by

$$w=1, \text{ independent of the } n_i; \tag{65}$$

only the single microstate which is symmetric in the N particles is allowed. Dirac notes that (65) leads to Planck's law (Dirac, 1926). After more than a quarter of a century the search for the foundations of this law comes to an end.

Equation (65) was of course not known when Bose and Einstein embarked on their explorations of a new statistics. Let us next turn to Bose's contribution.

B. Bose

The paper by Bose (1924b) is the fourth and last of the revolutionary papers of the old quantum theory. [The other three are, respectively, by Planck (1900b), Einstein (1905a) and Bohr (1913a).] Bose's arguments divest Planck's law of all supererogatory elements of electromagnetic theory and base its derivation on the bare essentials. It is the thermal equilibrium law for particles with the following properties: they are massless; they have two states of polarization; the number of particles is not conserved; and the particles obey a new statistics. In Bose's paper two new ideas enter physics almost stealthily. One, the concept of a *particle* with two states of polarization, mildly puzzled Bose. The other is the non-conservation of photons. I do not know whether Bose even noticed this fact. It is not explicitly mentioned in his paper.

Bose's letter to Einstein (Bose, 1924a) begins as follows: "Respected Sir, I have ventured to send you the accompanying article for your perusal. I am anxious to know what you will think of it. You will see that I have ventured to deduce the coefficient $8\pi\nu^2/c^3$ in Planck's law independent of the classical electrodynamics. . . ." Einstein's letter to Ehrenfest (Einstein, 1924g) contains the phrase: "The Indian Bose has given a beautiful derivation of Planck's law including the constant [i. e. $8\pi\nu^2/c^3$]." Neither letter mentions the other parts of Planck's formula. Why this emphasis on $8\pi\nu^2/c^3$?

In deriving Planck's law one needs to know the number of states Z^s in the frequency interval between ν^s and $\nu^s+d\nu^s$. It was customary to compute Z^s by counting the number of standing *waves* in a cavity with volume V which gives

$$Z^s=8\pi(\nu^s)^2 V d\nu^s/c^3 . \tag{66}$$

Bose was so pleased because he had found a new derivation of this expression for Z^s which enabled him to give a new meaning to this quantity in terms of *particle* language. His derivation rests on the replacement: counting wave frequencies $\rightarrow$ counting cells in one-particle phase space. He proceeded as follows. Integrate the one-particle phase space element $d\vec{x}\,d\vec{p}$ over V and over all momenta between p^s and $p^s + dp^s$. Supply a further factor two to count polarizations. This produces the quantity $8\pi V(p^s)^2 dp^s$ which equals $h^3 Z^s$ by virtue of the relation $p^s = h\nu^s/c$. Hence Z^s is the number of cells of size h^3 which is contained in the particle phase space region considered. How innocent it looks, yet how new it was. Recall that the kinematics of the Compton effect had only been written down about a year and a half earlier. Here was a new application of $p = h\nu/c$!

Before I turn to the rest of Bose's derivation I should like to digress briefly on the subject of photon spin. When Bose (1924b) introduced his polarization factor of two he noted that "it seems required" to do so. This slight hesitation is understandable. Who in 1924 had ever heard of a particle with two states of polarization? For some time this remained a rather obscure issue. After the discovery of the electron spin, Ehrenfest (1926) asked Einstein "to tell me how the analogous hypothesis is to be stated for light-corpuscles, in a relativistically correct way." As is well known this is a delicate problem since there exists, of course, no rest frame definition of spin in this instance. Moreover, gauge invariance renders ambiguous the separation into orbital and intrinsic angular momentum (see, e.g., Jauch and Rohrlich, 1955). It is not surprising that in 1926 the question of photon spin seemed quite confusing to Einstein. In fact he went as far as to say that he was "inclined to doubt whether the angular momentum law can be maintained in the quantum theory. At any rate its significance is much less deep than the momentum law" (Einstein, 1926a). I believe this is an interesting comment on the state of the art some fifty years ago and that otherwise not too much should be made of it.

Let us return to Bose. His new interpretation of Z^s was in terms of "number of cells," not "number of particles". This must have led him to follow Boltzmann's counting but *to replace everywhere "particles" by "cells,"* a procedure which he neither did, nor could justify—but which gave the right answer. He partitioned Z^s into numbers p_r^s, where p_r^s is defined as the number of cells which contain r quanta with frequency ν^s. Let there be N^s photons in all with this frequency and let E be the total energy. Then

$$Z^s = \sum_r p_r^s , \tag{67}$$

$$N^s = \sum_r r p_r^s , \tag{68}$$

$$E = \sum_s N^s h\nu^s , \tag{69}$$

while

$$N = \sum_s N^s \tag{70}$$

is the total number of photons. Next Bose introduced a new coarse-grained counting:

$$W = \prod_s \frac{Z^s!}{p_0^s!\, p_1^s! \ldots} . \tag{71}$$

He then maximized W as a function of the p_r^s, holding Z^s and E fixed so that

$$\sum_{s,r} \delta p_r^s \left\{ 1 + \ln p_r^s + \lambda^s + \frac{1}{\beta} r h\nu^s \right\} = 0 , \tag{72}$$

then derived Planck's law for $E(\nu, T)$ by standard manipulations—and therewith concluded his paper without further comments.

Bose (1924b) considered his Ansatz (71) to be "evident." I venture to guess that to him the cell counting (71) was the perfect analog of Boltzmann's particle counting (62) and that his cell constraint: hold Z^s fixed, was similarly the analog of Boltzmann's particle constraint: hold N fixed. Likewise the two Lagrange parameters in (72) are his analogs of the parameters in (64). Bose's replacement of fixed N by fixed Z^s already implies that N is not conserved. The final irony is that the constraint of fixed Z^s is irrelevant: If one drops this constraint then one must drop λ^s in Eq. (72). Even so, it is easily checked that one still finds Planck's law! This is in accordance with the now familiar fact that Planck's law follows from Bose statistics with E held fixed as the only constraint. In summary, Bose's derivation introduces three new features:

(1) Photon number nonconservation.

(2) Bose's cell partition numbers p_r^s are defined by asking how many particles are in a cell, not which particles are in a cell. Boltzmann's axiom is gone.

(3) The Ansatz (71) implies statistical independence of cells. Statistical independence of particles is gone.

The astounding fact is that Bose was correct on all three counts. (In his paper he commented on none of them.) I believe there has been no such successful shot in the dark since Planck introduced the quantum in 1900.

C. Einstein

As long as Einstein lived he never ceased to struggle with quantum physics. Insofar as his constructive contributions to this subject are concerned, these came to an end with a connected triple of papers, the first published in September 1924, the last two in early 1925. In the true Einstein style, their conclusions are once again reached by statistical methods, as was the case for all his important earlier contributions to the quantum theory. The best-known result is his derivation of the Bose–Einstein (BE) condensation phenomenon. I shall discuss this topic next and shall leave for the subsequent section another result contained in these papers which is perhaps not as widely remembered although it is even more profound.

First, a postscript to Einstein's light-quantum paper of 1905. Its logic can be schematically represented in the following way.

$$\text{Einstein 1905: } \left.\begin{matrix}\text{Wien's law} \\ \text{Gas analogy}\end{matrix}\right\} \rightarrow \text{Light-quanta.}$$

An issue raised at the end of Sec. II.D should be dealt with now. We know that BE is the correct statistics when radiation is treated as a photon gas. Then how could Einstein correctly have conjectured the existence of light-quanta using Boltzmann statistics? Answer: According to BE statistics, the most probable value $\langle n_i \rangle$ of the n_i for photons is given by $\langle n_i \rangle = \{\exp(h\nu_i/kT) - 1\}^{-1}$. This implies $\langle n_i \rangle \ll 1$ in the Wien régime $h\nu_i \gg kT$. Therefore up to an irrelevant[67] factor $N!$ Equations (59) and (65) coincide in the Wien limit. This asymptotic relation in the Wien region fully justifies, ex post facto, Einstein's extraordinary step forward in 1905!

Bose's reasoning in 1924 went as follows:

$$\left.\begin{array}{l}\text{Bose 1924: Photons}\\ \\ \text{Quantum statistics}\end{array}\right\} \rightarrow \text{Planck's law,}$$

and in 1924–5 Einstein came full circle:

$$\left.\begin{array}{l}\text{Einstein 1924–5: Bose statistics}\\ \\ \text{Photon analogy}\end{array}\right\} \rightarrow \text{The quantum gas}\,.$$

It was inevitable, one might say, that he would do so. "If it is justified to conceive of radiation as a quantum gas, then the analogy between the quantum gas and a molecular gas must be a complete one" (Einstein, 1925a).

In his 1924 paper, Einstein (1924h) adopted Bose's counting formula (71), but with two modifications. He needed of course the Z^s appropriate for nonrelativistic particles with mass m:

$$h^3 Z^s = 2\pi V(2m)^{3/2}(E^s)^{1/2}\, dE^s,\ \ 2mE^s = (p^s)^2\,. \tag{73}$$

Secondly (and unlike Bose!) he needed the constraint that N is held fixed. This is done by adding a term

$$-r \ln A \tag{74}$$

inside the curly brackets of Eq. (72).[68] One of the consequences of the so-modified Eq. (72) is that the Lagrange multiplier $(-\ln A)$ is determined by

$$N = \sum_s N^s = \sum_s \left[\frac{1}{A}\exp\left(\frac{E^s}{kT}\right) - 1\right]^{-1}. \tag{75}$$

Hence, Einstein noted, the "degeneracy parameter" A^{-1} must satisfy

$$A \leqslant 1\,. \tag{76}$$

In his first paper (1924h) Einstein discussed the regime in which A does not reach the critical value one. He proceeds to the continuous limit in which the sum in Eq. (75) is replaced by an integral over phase space and finds[69]

$$\text{if } A<1: \frac{1}{v} = \frac{\phi_{3/2}(A)}{3}, \frac{p}{kT} = \frac{\phi_{5/2}(A)}{3}, \tag{77}$$
$$\phi_n(A) = \sum_{m=1}^{\infty} m^{-n} A^m,$$

with $\lambda^2 = h^2/2\pi mkT$ and $v = V/N$. He then discusses the region $A \ll 1$ where the equation of state [obtained by eliminating A between the two equations (77)] shows perturbative deviations from the classical ideal gas. All this is good physics, though unusually straightforward for a man like Einstein.

In his second paper (1925a), the most important one of the three, Einstein begins with the $v-T$ relation at $A=1$

$$kT_0 = \frac{h^2}{2m[v_0\phi_{3/2}(1)]^{3/2}}, \tag{78}$$

and asks what happens if T drops below T_0 (for given v_0). His answer:

"I maintain that in this case a number of molecules steadily growing with increasing density goes over in the first quantum state (which has zero kinetic energy) while the remaining molecules distribute themselves according to the parameter value $A=1$. . . a separation is effected; one part condenses, the rest remains a 'saturated ideal gas'" (Einstein, 1925a). Einstein had come upon the first purely statistically derived example of a phase transition which is now called Bose-Einstein condensation. I defer a few comments on this phenomenon to the postscript at the end of this section and turn next to other important facets of the three Einstein papers.

(1) *Einstein on statistical dependence*. After the papers by Bose (1924b) and the first one by Einstein (1924h) had come out, Ehrenfest and others objected (so we read in Einstein's second paper (1925a)) that "the quanta and molecules respectively are not treated as statistically independent, a fact which is not particularly emphasized in our papers" (i.e., Bose, 1924b, and Einstein, 1924h). Einstein replied (1925a): "This [objection] is entirely correct." He went on to stress that the differences between the Boltzmann and the BE counting "express indirectly a certain hypothesis on a mutual influence of the molecules which for the time being is of a quite mysterious nature." With this remark Einstein came to the very threshold of the quantum mechanics of identical particle systems. The mysterious influence is of course the correlation induced by the requirement of totally symmetric wave functions.

(2) *Einstein on indistinguishability*. In order to illustrate further the differences between the new and the old counting of macrostates, Einstein (1925a) cast W in a form alternative to Eq. (71). He counted the number of ways in which N^s indistinguishable particles in the dE^s-interval can be partitioned over the Z^s cells. This yields

$$W = \prod_s \frac{(N^s + Z^s - 1)!}{N^s!\,(Z^s-1)!}\,. \tag{79}$$

Einstein's Eq. (79) rather than Bose's Eq. (71) is the one now used in all textbooks.

(3) *Einstein on the third law of thermodynamics*. As was noted at the end of Sec. III.C, in 1914 Nernst introduced the hypothesis that the third law of thermodynamics applies to gases. It was also mentioned that no sensible model of a gas with that property was available at that time. In 1925 Einstein made his last contribution to thermodynamics by pointing out that the BE gas does satisfy the third law. (A Boltzmann gas does not do so, Einstein remarked.) Indeed, since all par-

[67] The $N!$ is irrelevant since it only affects C in Eq. (63). The constant C is interesting nevertheless. For example, its value bears on the possibility of defining S in such a way that it becomes an extensive thermodynamic variable. The interesting history of these normalization questions has been discussed in detail by M. Klein (1958).

[68] $A^{-1} \equiv \exp(-\mu/kT)$, μ is the chemical potential. Einstein of course never introduced the superfluous λ^s in these curly brackets. In Eqs. (74)–(79) I deviate from Einstein's notations.

[69] All technical steps are now found in standard textbooks.

ticles go into the zero energy state as $T \to 0$, we have in this limit $N^0 = N$, all other $N^s = 0$. Hence $W \to 1$ and $S \to 0$ as $T \to 0$. It was as important to him that a molecular BE gas yield Nernst's law as that a BE photon gas yield Planck's law.

(4) *Einstein and nonconservation of photons*. After 1917 Einstein ceased to write scientific articles on questions related to radiation.[70] The only mention of radiation in the 1924/5 papers is that "the statistical method of Herr Bose and myself is by no means beyond doubt, but seems only a posteriori justified by its success for the case of radiation" (Einstein, 1925b).

There can be no doubt that he must have noted the nonconservation of photons. In his language this is implemented by putting $A = 1$ in Eq. (74). Yet I have not found any reference to nonconservation, either in his scientific writings or in the correspondence I have seen. I cannot state with certainty why he chose to be silent on this and all further issues regarding photons. However I do believe it is a fair guess that Einstein felt he would have nothing fundamental to say about photons until such time as he could find his own way of dealing with the lack of causality he had noted in 1917.

Such a time never came.

D. Postscripta on Bose-Einstein condensation

(1) In 1925 Einstein (1925a) mentioned hydrogen, helium, and the electron gas as the best possible candidates to observe his condensation phenomenon. In 1925 these were, of course, sensible proposals. Recall that the Fermi Dirac statistics was not discovered[71] until 1926 (Fermi, 1926; Dirac, 1926), following Pauli's enunciation of the exclusion principle in 1925 (Pauli, 1925b). Even then it took some time until it was sorted out when BE and FD statistics apply respectively: Referring to Dirac's paper, (1926) Pauli (1927) wrote in December 1926: "We shall take the point of view also advocated by Dirac, that the Fermi—and not the Einstein–Bose statistics applies to the material gas." These matters were cleared up by 1927.

(2) In his 1925 paper, Einstein (1925a) did not call the condensation phenomenon a phase transition. According to George Eugene Uhlenbeck (1900–) nobody realized in 1925 that the existence of a phase transition was a "deep" problem (private communication). In 1926 Uhlenbeck (1927) himself raised an objection to Einstein's treatment of the condensation problem. This critique was to lead to a more precise theoretical formulation of the conditions under which phase transitions can occur. Uhlenbeck noted that the quantity N^0 in Eq. (75) $\to \infty$ as $A \to 1$ (for fixed T), hence also $N \to \infty$. Thus if $A = 1$ it is impossible to implement the constraint that N is a fixed *finite* number. Therefore $A = 1$ can only be reached asymptotically and there is no two phase regime. [Uhlenbeck (1979) has recently described the communications between Ehrenfest and Einstein on this question.] Uhlenbeck and Einstein were both right, however. The point is that a sharp phase transition can only occur in the so-called thermodynamic limit $N \to \infty$, $V \to \infty$, v fixed. This view emerged in a morning-long debate which took place during the van der Waals Centenary Conference in November 1937. The issue was: does the partition function contain the information necessary to describe a sharp phase transition? The transition implies the existence of analytically distinct parts of isotherms. It was not clear how this could come about. The debate was inconclusive and Kramers, the chairman, put the question to a vote. Uhlenbeck recalls that the ayes and nays were about evenly divided. However, Kramers' suggestion to go to the thermodynamic limit eventually was realized to be the correct answer. Shortly afterward, Uhlenbeck withdrew his objections to Einstein's result, in a joint paper with his gifted student, the late Boris Kahn (a Nazi victim) (Kahn and Uhlenbeck, 1938).

(3) Until 1928 the BE condensation had "the reputation of having only a purely imaginary character" (London, 1938a). Recall that the HeI–HeII phase transition was not discovered until 1928, by Willem Hendrik Keesom (1876–1956) (see Keesom, 1942). In 1938 Fritz London (1900–1954) proposed to interpret this He-transition as a BE condensation. Experimentally, the transition point lies at 2.19°K. It is most encouraging that Eq. (78) gives $T = 3.1$°K (London, 1938b). It is generally believed but not proved that the difference between these two values is due to the neglect of intermolecular forces in the theoretical derivations.

VII. EINSTEIN AS A TRANSITIONAL FIGURE: THE BIRTH OF WAVE MECHANICS

We now leave the period of the old quantum theory and turn to the time of transition during which matter waves were being discussed by a tiny group of physicists at a time when matter wave mechanics had not yet been discovered. This period begins in September 1923 with two brief communications (1923b, 1923c) by Louis de Broglie (1892–) to the French Academy of Sciences. It ends in January 1926 with Erwin Schroedinger's (1887–1961) first paper (1962a) on wave mechanics. The main purpose of this section is to stress Einstein's key role in these developments, his influence on de Broglie, de Broglie's subsequent influence on him, and finally, the influence of both on Schroedinger.

Neither directly nor indirectly did Einstein contribute to an equally fundamental development which preceded Schroedinger's discovery of wave mechanics: the discovery of matrix mechanics by Heisenberg (1926b). Therefore I shall have no occasion in this article to comment in any detail on Heisenberg's major achievements.

A. From Einstein to de Broglie

During the period which began with Einstein's work on needle rays (1917) and ended with Debye's and Compton's papers on the Compton effect (1923) there were a few other theoreticians who did research on

[70] Except for a brief refutation of an objection to his work on needle radiation (Einstein, 1925c). I found a notice by Einstein (1930a) in 1930 announcing a new paper on radiation fluctuations. This paper was never published, however.

[71] Dirac (1977) has given a charming account of the time sequence of these discoveries.

photon questions. Among those the only one[72] whose contribution lasted was de Broglie.

de Broglie had finished his studies before the First World War. In 1919, after a long tour of duty with the French forces he joined the physics laboratory headed by his brother Maurice (1875–1960), where x-ray photoeffects and x-ray spectroscopy were the main topics of study. Thus he was much exposed to questions conerning the nature of electromagnetic radiation, a subject on which he published several papers. In one of these de Broglie (1923a) evaluated independently of Bose (and published before him) the density of radiation states in terms of particle (photon) language (see Sec. VI. B). That was in October 1923–one month after his enunciation of the epochal new principle that particle-wave duality should apply not only to radiation but also to matter. "After long reflection in solitude and meditation I suddenly had the idea, during the year 1923, that the discovery made by Einstein in 1905 should be generalized in extending it to all material particles and notably to electrons" (de Broglie, 1963).

He made the leap in his September 10, 1923 paper (1923b): $E=h\nu$ shall hold not only for photons but also for electrons, to which he assigns a "fictitious associated wave." In his September 24 paper (1923c) he indicated the direction in which one "should seek experimental confirmations of our ideas": A stream of electrons traversing an aperture whose dimensions are small compared with the wavelength of the electron waves "should show diffraction phenomena."

Other important aspects of de Broglie's work are beyond the scope of this paper. The mentioned articles were extended to form his doctor's thesis (see de Broglie, 1963) which he defended on November 25, 1924. Einstein received a copy of this thesis from one of de Broglie's examiners, Paul Langevin (1872–1946). A letter to Lorentz (in December) shows that Einstein (1924i) was impressed and also that he had found a new application of de Broglie's ideas: "A younger brother of. . . de Broglie has undertaken a very interesting attempt to interpret the Bohr-Sommerfeld quantum rules (Paris Dissertation 1924). I believe it is a first feeble ray of light on this worst of our physics enigmas. I too have found something which speaks for his construction."

B. From de Broglie to Einstein

In 1909 and again in 1917 Einstein had drawn major conclusions about radiation from the study of fluctuations around thermal equilibrium. It goes without saying that he would again examine fluctuations when, in 1924, he turned his attention to the molecular quantum gas.

In order to appreciate what he did this time it is helpful to copy, one last time, the formula given earlier, for the mean square energy of electromagnetic radiation (see Sec. II. G):

$$\langle \varepsilon^2 \rangle = \left(\rho h\nu + \frac{c^3\rho^2}{8\pi\nu^2}\right) V\,d\nu\,. \tag{37}$$

Put $V\rho\,d\nu = n(\nu)h\nu$ and $\varepsilon^2 = \Delta(\nu)^2(h\nu)^2$. $n(\nu)$ can be interpreted as the average number of quanta in the energy interval $d\nu$, and $\Delta(\nu)^2$ as the mean square fluctuation of this number. One can now write (37) as

$$\Delta(\nu)^2 = n(\nu) + \frac{n(\nu)^2}{Z(\nu)}, \tag{80}$$

where $Z(\nu)$ is the number of states per interval $d\nu$ given in Eq. (66).[73] In his paper submitted on January 8, 1925, Einstein (1925a) showed that Eq. (80) holds equally well for his quantum gas, as long as one defines ν in the latter case by $E=h\nu=p^2/2m$ and uses Eq. (73) instead of Eq. (66) for the number of states.

When discussing radiation, in 1909, Einstein recognized the second term of Eq. (37) as the familiar wave term and the first one as the unfamiliar particle term. When, in 1925, he revisited the fluctuation problem for the case of the quantum gas, he noted a reversal of roles. The first term, at one time unfamiliar for radiation, was now the old fluctuation term for a Poisson distribution of (distinguishable) particles. What to do with the second term (which incorporates indistinguishability effects of particles) for the gas case? Since this term was associated with waves in the case of radiation, Einstein (1925a) was led to "interpret it in a corresponding way for the gas, by associating with the gas a radiative phenomenon." He added: "I pursue this interpretation further since I believe that here we have to do with more than a mere analogy."

But what were the waves?

At that point Einstein turned to de Broglie's thesis (see de Broglie, 1963), "a very notable publication." He suggested that a de Broglie-type wave field should be associated with the gas and pointed out that this assumption enabled him to interpret the second term in Eq. (80). Just as de Broglie had done, he also noted that a molecular beam should show diffraction phenomena but added that the effect should be extremely small for manageable apertures. Einstein also remarked that the de Broglie-wave field had to be a scalar. [The polarization factor equals two for Eq. (66), as noted above, but it equals one for Eq. (73)!].

It is another Einstein feat that he would be led to state the necessity of the existence of matter waves from the analysis of fluctuations. One may wonder what the history of twentieth century physics would have been like had Einstein pushed the analogy still further. However, with the achievement of an independent argument for the particle-wave duality of matter, the twenty-year period of highest scientific creativity in Einstein's life, at a level probably never equalled, came to an end.

Postscript, summer of 1978. In the course of preparing this article I noticed a recollection by Pauli (1949) of a statement made by Einstein during a physics meeting held in Innsbruck in 1924. According to Pauli, Einstein proposed in the course of that meeting "to search for interference and diffraction phenomena with molecular beams." On checking the dates of that meeting I found them to be September 21–27. This intrigued me. Einstein came to the particle–wave duality of matter via a route which was independent of the one

[72]The other ones I know of are Brillouin (1921), Wolfke (1921), Bothe (1923), Bateman (1923), Ornstein and Zernike (1919).

[73]In Eq. (80) I drop the index s occurring in Eq. (66).

taken by de Broglie. The latter defended his thesis in November. If Pauli's memory is correct, then Einstein made his remark about two months prior to that time. Could Einstein have come upon the wave properties of matter independently of de Broglie? After all, Einstein had been thinking about the molecular gas since July. The questions arise: When did Einstein become aware of de Broglie's work? In particular, when did Einstein receive de Broglie's thesis from Langevin? Clearly it would be most interesting to know what Professor de Broglie might have to say about these questions. Accordingly I wrote to him. He was kind enough to reply. With his permission I quote from his answers.

de Broglie does not believe that Einstein was aware of his three short publications (1923a, b, c) written in 1923. "Nevertheless, since Einstein would receive the Comptes Rendus and since he knew French very well, he might have noticed my articles" (de Broglie, 1978a). de Broglie noted further that he had given Langevin the first typed copy of his thesis early in 1924. "I am certain that Einstein knew of my Thèse since the spring of 1924" (de Broglie, 1978b). This is what happened. "When in 1923 I had written the text of the Thèse de Doctorat which I wanted to present in order to obtain the Doctorat ès Sciences, I had three typed copies made. I handed one of these to M. Langevin so that he might decide whether this text could be accepted as a Thèse. M. Langevin, 'probablement un peu étonné par la nouveauté de mes idées,'[74] asked me to furnish him with a second typed copy of my Thèse for transmittal to Einstein. It was then that Einstein declared, after having read my work, that my ideas seemed quite interesting to him. This made Langevin decide to accept my work" (de Broglie, 1978a).

Thus Einstein was not only one of the three fathers of the quantum theory, but also the sole godfather of wave mechanics.

C. From de Broglie and Einstein to Schroedinger

Late in 1925 Schroedinger completed an article (1926b) entitled "On Einstein's gas theory." It was his last paper prior to his discovery of wave mechanics. Its content is crucial to an understanding of the genesis of that discovery (Klein, 1964).

In order to follow Schroedinger's reasoning it is necessary to recall first a derivation of Planck's formula given by Debye (1910) in 1910. Consider a cavity filled with radiation oscillators in thermal equilibrium. The spectral density is $8\pi\nu^2\varepsilon(\nu,T)/c^3$. ε is the equilibrium energy of a radiation field oscillator with frequency ν. Debye introduced the quantum prescription that the only admissible energies of the oscillator shall be $nh\nu$, $n=0,1,2,\ldots$. In equilibrium the nth energy level is weighted with its Boltzmann factor. Hence $\varepsilon = \sum nh\nu y^n/\sum y^n$, $y=\exp(-h\nu/kT)$. This yields Planck's law.[75]

[74] "Probably a bit astonished by the novelty of my ideas."

[75] This derivation differs from Planck's in that the latter quantized material rather than radiation oscillators. It differs from the photon gas derivation in that the energy $nh\nu$ is interpreted as the nth state of a single oscillator, not as a state of n particles each with energy $h\nu$.

Now back to Schroedinger. By his own admission (Schroedinger, 1926b) he was not much taken with the new BE statistics. Instead, he suggested, why not evade the new statistics by treating Einstein's molecular gas according to the Debye method? That is, *why not start from a wave picture of the gas* and superimpose on that a quantization condition à la Debye? Now comes the key sentence in the article (1926b): "That means nothing else but taking seriously the de Broglie-Einstein wave theory of moving particles." And that is just what Schroedinger did. It is not necessary to discuss further details of this article, which was received by the publisher on December 25, 1925.

Schroedinger's next paper (1926c) was received on January 27, 1926. It contains his equation for the hydrogen atom. Wave mechanics was born. In this new paper Schroedinger (1926c, p. 373) acknowledged his debt to de Broglie and Einstein: "I have recently shown [1926b] that the Einstein gas theory can be founded on the consideration of standing waves which obey the dispersion law of de Broglie. . . . The above considerations about the atom could have been presented as a generalization of these considerations". In April 1926 Schroedinger (1926d) again acknowledged the influence of de Broglie and "brief but infinitely far seeing remarks by Einstein."

VIII. EINSTEIN'S RESPONSE TO THE NEW DYNAMICS

Everyone familiar with modern physics knows that Einstein's attitude regarding quantum mechanics was one of skepticism. No biography of him fails to mention his saying that God does not throw dice. He was indeed given to such utterances (as I know from experience), and stronger ones such as "It seems hard to look in God's cards. But I cannot for a moment believe that he plays dice and makes use of 'telepathic' means (as the current quantum theory alleges he does" (Einstein, 1942b). However, remarks such as these should not create the impression that Einstein had abandoned active interest in quantum problems in favor of his quest for a unified field theory. Far from it. In fact even in the search for a unified theory the quantum riddles were very much on his mind, as I shall discuss later on in Sec. X. In the present section, I shall attempt to describe how Einstein's position concerning quantum mechanics evolved in the course of time. To some extent this is reflected in his later scientific papers. It becomes evident more fully in several of his more autobiographical writings and in his correspondence. My own understanding of his views have been helped much by discussions with him.

To begin with I shall turn to the period 1925-1933 during which he was much concerned with the question: is quantum mechanics consistent?

A. 1925–1933. The debate begins

As I mentioned earlier, the three papers of 1924–5 on the quantum gas were only a temporary digression from Einstein's program begun several years earlier to unify gravitation with electromagnetism. During the very early days of quantum mechanics[76] we find him "working strenuously on the further development of a theory on the connection between gravitation and electricity" (Einstein, 1925d). Yet the great importance of the new developments in quantum theory was not lost on him. Bose, who visited Berlin in November 1925, has recalled (Mehra, 1975) that "Einstein was very excited about the new quantum mechanics. He wanted me to try to see what the statistics of light-quanta and the transition probabilities of radiation would look like in the new theory."[77]

Einstein's deep interest in quantum mechanics must have led him to write to Heisenberg rather soon after the latter's paper (1926b) had been published. All the letters of Einstein to Heisenberg have been lost.[78] However, a number of letters by Heisenberg to Einstein are extant. One of these (dated November 30, 1925) is clearly in response to an earlier lost letter by Einstein to Heisenberg in which Einstein appears to have commented on the new quantum mechanics.[79] One remark by Heisenberg (1925) is of particular interest. "You are probably right that our formulation of the quantum mechanics is more adapted to the Bohr–Kramers–Slater attitude; but this [BKS theory] constitutes in fact one aspect of the radiation phenomena. The other is your light-quantum theory and we have the hope that the validity of the energy and momentum laws in our quantum mechanics will one day make possible the connection with your theory." I find it remarkable that Einstein apparently sensed that there was some connection between the BKS theory and quantum mechanics. No such connection exists of course. Nevertheless the BKS proposal contains statistical features,[80] as we have seen. Could Einstein have surmised as early as 1925 that *some* statistical element is inherent in the quantum-mechanical description?!

During the following months, Einstein vacillated in his reaction to the Heisenberg theory. In December 1925 he expressed misgivings (Einstein, 1925e). But in March 1926 he wrote to the Borns (Einstein, 1926b): "The Heisenberg–Born concepts leave us all breathless, and have made a deep impression on all theoretically oriented people. Instead of a dull resignation, there is now a singular tension in us sluggish people." The next month he expressed again his conviction that the Heisenberg–Born approach was off the track. That was in a letter in which he congratulated Schroedinger on his new advance (Einstein, 1926c). In view of the scientific links between Einstein's and Schroedinger's work it is not surprising that Einstein (1926d) would express real enthusiasm about wave mechanics: "Schroedinger has come out with a pair of wonderful papers on the quantum rules." It was the last time he would write approvingly about quantum mechanics.

There came a parting of ways.

Nearly a year passed after Heisenberg's paper before there was a first clarification of the conceptual basis of quantum mechanics. It began with Max Born's (1882–1970) observation (Born, 1926) in June 1926 that the absolute square of a Schroedinger wave function is to be interpreted as a probability density. Born's brief and fundamental paper goes to the heart of the problem of determinism. Regarding atomic collisions he wrote: "One does not get an answer to the question "what is the state after collision" but only to the question "how probable is a given effect of the collision". . . . From the standpoint of our quantum mechanics there is no quantity [Grösze] which causally fixes the effect of a collision in an individual event. Should we hope to discover such properties later. . . and determine [them] in individual events? . . . . I myself am inclined to renounce determinism in the atomic world. But that is a philosophical question for which physical arguments alone do not set standards."

Born's paper had a mixed initial reception. Several leading physicists found it hard if not impossible to swallow the abandonment of causality in the classical sense, among them Schroedinger. More than once Bohr mentioned to me that Schroedinger told him he might not have published his papers had he been able to foresee what consequences they would unleash.[81] Einstein's position in the years to follow can be summarized succinctly by saying that he took exception to every single statement contained in the lines I have quoted from Born. His earliest expressions of discomfort I know of date from late 1926 when he wrote Born (Einstein, 1926e): "Quantum mechanics is certainly imposing. But an inner voice tells me that it is not yet the real thing. The theory says a lot, but does not really bring us closer to the secret of the 'old one'."

"Einstein's verdict. . . . came as a hard blow" to Born (Born, 1971, pg. 1). There are other instances as well in which Einstein's reactions were experienced with a sense of loss, of being abandoned by a venerated leader in battle. Thus Samuel Goudsmit (1902–1978) told me of a conversation which took place in mid-1927 [to the best of his recollection (Goudsmit, 1978)] between Ehrenfest and himself. In tears, Ehrenfest said that he had to make a choice between Bohr's and Einstein's position and that he could not but agree with Bohr. Needless to say, Einstein's reactions affected the older generation more intensely than the younger.

[76]Recall that Heisenberg's first paper on this subject (1926b) was completed in July 1925, Schroedinger's (1926c) in January 1926.

[77]It was not for Bose but for Dirac to answer this question. In his 1927 paper which founded quantum electrodynamics Dirac (1927) gave the dynamical derivation of expressions for the Einstein A and B coefficients.

[78]According to Helen Dukas. The letters by Heisenberg to Einstein referred to are now in the Einstein Archives in Princeton.

[79]Heisenberg (1925) begins by thanking Einstein for his letter and then proceeds with a rather lengthy discussion of the role of the zero point energy in the new theory. This seems to be in response to a point raised by Einstein. It is not clear to me from Heisenberg's letter what Einstein had in mind.

[80]Heisenberg remarked much later (1955) that "the attempt at interpretation by Bohr, Kramers and Slater nevertheless contained some very important features of the later, correct interpretation [of quantum mechanics]." I do not share this view but shall not argue this issue beyond what has been said in Sec. III.

[81]Schroedinger retained reservations on the interpretation of quantum mechanics for the rest of his life (see Scott, 1967).

Of the many important events in 1927, three are particularly significant for the present account.

March 1927. Heisenberg states the uncertainty principle. In June Heisenberg (1927a) writes a letter to Einstein which begins as follows: "Many cordial thanks for your kind letter; although I really do not know anything new, I would nevertheless like to write once more why I believe that indeterminism, that is the nonvalidity of rigorous causality, is *necessary* [his italics] and not just consistently possible." This letter is apparently in response to another lost letter by Einstein triggered, most probably, by Heisenberg's work in March. I shall return to Heisenberg's important letter in Sec. X. I only mention its existence at this point to emphasize that once again Einstein did not react to these new developments as a passive bystander. In fact at just about that time he was doing his own research on quantum mechanics (his first, I believe). "Does Schroedinger's wave mechanics determine the motion of a system completely or only in the statistical sense?"[82] he asked. Heisenberg had heard indirectly that Einstein "had written a paper in which you. . . . advocate the view that it should be possible after all to know the orbits of particles more precisely than I would wish." He asked for more information "especially because I myself have thought so much about these questions and only came to believe in the uncertainty relations after many pangs of conscience, though now I am entirely convinced" (Heisenberg, 1927b). Einstein eventually withdrew his paper.[83]

September 16, 1927. At the Volta meeting in Como[84] Bohr (1928) enunciates for the first time the principle of complementarity: "The very nature of the quantum theory. . . forces us to regard the space-time coordination and the claim of causality, the union of which characterises the classical theories, as complementary but exclusive features of the description, symbolising the idealisation of observation and definition respectively."

October 1927. The fifth Solvay Conference convenes. All the founders of the quantum theory were there, from Planck, Einstein, and Bohr to de Broglie, Heisenberg, Schroedinger, and Dirac. During the sessions "Einstein said hardly anything beyond presenting a very simple objection to the probability interpretation. . . Then he fell back into silence" (de Broglie, 1962).

However, the formal meetings were not the only place of discussion. All participants were housed in the same hotel and there, in the dining room, Einstein was much livelier. Otto Stern has given this first-hand account[85]: "Einstein came down to breakfast and expressed his misgivings about the new quantum theory, every time [he] had invented some beautiful experiment from which one saw that it did not work. . . . Pauli and Heisenberg who were there did not react to these matters, "ach was, das stimmt schon, das stimmt schon" [ah well, it will be alright, it will be alright]. Bohr on the other hand reflected on it with care and in the evening, at dinner, we were all together and he cleared up the matter in detail."

Thus began the great debate between Bohr and Einstein. Both men refined and sharpened their positions in the course of time. No agreement between them was ever reached. Between 1925 and 1931 the only objection by Einstein which appeared in print in the scientific literature is the one at the 1927 Solvay Conference (Einstein, 1928). However, there exists a masterful account of the Bohr–Einstein dialog during these years, published by Bohr (1949) in 1949. I have written elsewhere about the profound role which the discussions with Einstein played in Bohr's life (Pais, 1967).

The record of the Solvay meeting contains only minor reactions to Einstein's comments. Bohr's later article (1949) analyzed them in detail. Let us consider next the substance of Einstein's remarks.

Einstein's opening phrase (1928) tells more about him than does many a book: "Je dois m'excuser de n'avoir pas approfondi la mécanique des quanta" ("I must apologize for not having examined quantum mechanics in depth").

He then discusses an experiment in which a beam of electrons hits a (fixed) screen with an aperture in it. The transmitted electrons form a diffraction pattern which is observed on a second screen. Question: does quantum mechanics give a complete description of the *individual* electron events in this experiment? His answer: This cannot be. For let A and B be two distinct spots on the second screen. If I know that an individual electron arrives at A then I know instantaneously that it did not arrive at B. But this implies a peculiar instantaneous action at a distance between A and B contrary to the relativity postulate. Yet (Einstein notes) in the Geiger–Bothe experiment on the Compton effect (Bothe and Geiger, 1925a, b) there is no limitation of principle to the accuracy with which one can observe coincidences in individual processes, and that without appeal to action at a distance. This circumstance adds to the sense of incompleteness of the description for diffraction.

Quantum mechanics provides the following answer to Einstein's query. It does apply to individual processes but the uncertainty principle defines and delimits the optimal amount of information which is obtainable *in a given experimental arrangement.* This delimitation differs incomparably from the restrictions on information inherent in the coarse-grained description of events in classical statistical mechanics. There the restrictions are wisely self-imposed in order to obtain a useful approximation to a description in terms of an ideally knowable complete specification of momenta and positions of individual particles. In quantum mechanics the delimitations mentioned earlier are not self-imposed but are renunciations of first principle (on the fine-grained level, one might say). It is true that one would need action at a distance if one were to insist on a fully causal description involving the localization of the elec-

[82] This is the title of a paper which Einstein submitted at the May 5, 1927 meeting of the Prussian Academy in Berlin.[83]

[83] The records show that this paper was in print when Einstein requested by telephone that it be withdrawn. The unpublished manuscript is in the Einstein Archives. I would like to thank Dr. John Stachel for bringing this material to my attention.

[84] Einstein had been invited to this meeting but did not attend.

[85] In a discussion with R. Jost, taped on December 2, 1961. I am very grateful to R. Jost for making available to me a transcript of part of this discussion.

tron at every stage of the experiment in hand. Quantum mechanics denies that such a description is called for and asserts that *in this experiment* the final position of an individual electron cannot be predicted with certainty. Quantum mechanics nevertheless makes a prediction in this case, concerning the probability for an electron to arrive at a given spot on the second screen. The verification of this prediction demands of course that the "one electron experiment" be repeated as often as is necessary to obtain this probability distribution with a desired accuracy.

Nor is there a conflict with Geiger–Bothe, since now one refers to *another experimental arrangement* in which localization in space-time is achieved, but this time at the price of renouncing information on sharp energy-momentum properties of the particles observed in coincidence. From the point of view of quantum mechanics these renunciations are expressions of laws of nature. They are also applications of the saying "Il faut reculer pour mieux sauter" ("It is necessary to take a step back in order to jump better"). As we shall see, what was and is an accepted renunciation to others was an intolerable abdication in Einstein's eyes. On this score he was never prepared to give up anything.

I have dwelt at some length on this simple problem since it contains the germ of Einstein's position which he stated more explicitly in later years. Meanwhile the debate in the corridors between Bohr and Einstein continued during the sixth Solvay conference (on magnetism) in 1930. This time Einstein thought he had found a counterexample to the uncertainty principle. The argument was ingenious. Consider a box with a hole in its walls which can be opened or closed by a shutter controlled by a clock inside the box. The box is filled with radiation. Weigh the box. Set the shutter to open for a brief interval during which a single photon escapes. Weigh the box again, some time later. Then (in principle) one has found to arbitrary accuracy both the photon energy and its time of passage, in conflict with the energy-time uncertainty principle.

"It was quite a shock for Bohr ... he did not see the solution at once. During the whole evening he was extremely unhappy, going from one to the other and trying to persuade them that it couldn't be true, that it would be the end of physics if Einstein were right; but he couldn't produce any refutation. I shall never forget the vision of the two antagonists leaving the club [of the Fondation Universitaire]: Einstein a tall majestic figure, walking quietly, with a somewhat ironical smile, and Bohr trotting near him, very excited .... The next morning came Bohr's triumph" (Rosenfeld, 1968).

Bohr (1949) later illustrated his argument with the help of the experimental arrangement reproduced in Fig. 4. The initial weighing is performed by recording the position of the pointer attached to the box relative to the scale attached to the fixed frame. The loss of weight due to the escape of the photon is compensated by a load (hung underneath the box) which returns the pointer to its initial position with a latitude Δq. Correspondingly, the weight measurement has an uncertainty Δm. The added load imparts a momentum to the box which we can measure with an accuracy Δp delimited by $\Delta p\,\Delta q \approx h$. Obviously $\Delta p \lesssim Tg\,\Delta m$, where T is the

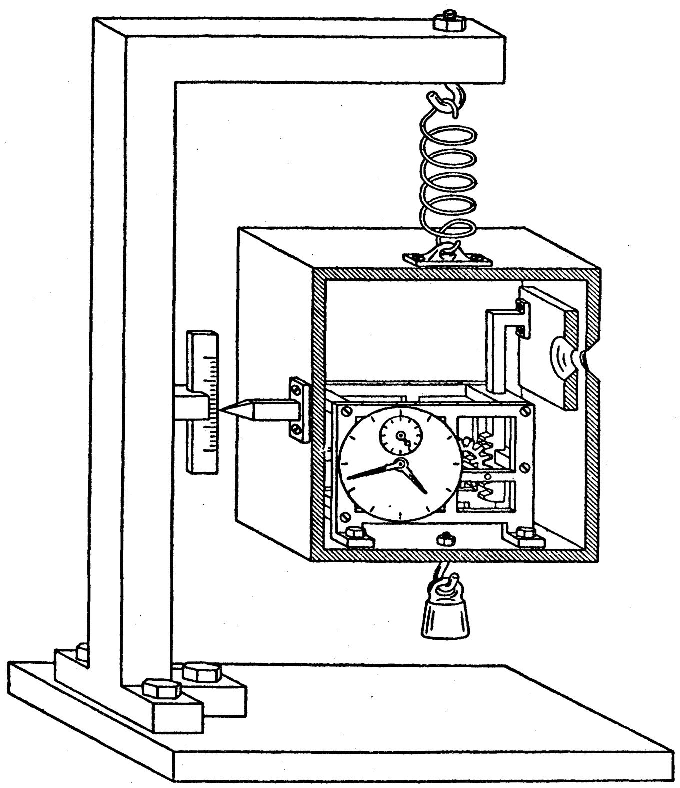

FIG. 4. Bohr's drawing of Einstein's clock-in-the-box experiment (Bohr, 1949). (Reproduced with the kind permission of Professor A. Schilpp.)

time taken to readjust the pointer, and g is the gravitational acceleration. Thus $Tg\Delta m\,\Delta q \gtrsim h$. Next Bohr used the red shift formula[85a]: the uncertainty Δq of the position of the clock in the gravitational field implies an uncertainty $\Delta T = c^{-2}gT\,\Delta q$ in the determination of T. Hence $c^2\Delta m\,\Delta T = \Delta E\,\Delta T \gtrsim h$. Thus the accuracy with which the energy of the photon is measured restricts the precision with which its moment of escape can be determined, in accordance with the uncertainty relations for energy and time. (Note that every one of the many details in Fig. 4 serves an experimental purpose: the heavy bolts fix the position of the scale along which the pointer moves; the spring guarantees the mobility of the box in the gravitational field; the weight attached to the box serves to readjust its position; and so on. There was nothing fanciful in Bohr's insistence on such details. Rather he had them drawn in order to illustrate that, since the results of all physical measurements are expressed in classical language, it is necessary to specify in detail the tools of measurement in that same language as well.)

After this refutation by Bohr, Einstein ceased his search for inconsistencies. By 1931 his position on quantum mechanics had undergone a marked change.

First of all, his next paper on quantum mechanics (Einstein, Tolman, and Podolsky, 1931), submitted in February 1931, shows that he had accepted Bohr's

[85a]Recall that the only ingredients for the derivation of this formula are the special relativistic time dilation and the equivalence principle.

criticism.[86] It deals with a new variant of the clock-in-the-box experiment. Experimental information about one particle is used to make predictions about a second particle. This paper, a forerunner of the Einstein–Podolsky–Rosen article to be discussed below, need not be remembered for its conclusions.[87]

A far more important expression of Einstein's opinions is found in a letter he wrote the following September. In this letter, addressed to the Nobel committee in Stockholm, Einstein (1931a) nominates Heisenberg and Schroedinger for the Nobel Prize. In his motivation he says about quantum mechanics: "Diese Lehre enthält nach meiner Überzeugung ohne Zweifel ein Stück endgültiger Wahrheit."[88] Einstein himself was never greatly stirred by honors and distinctions. Even so, his nominations do not only reveal extraordinarily clearly what his thoughts were but are also deeply moving as an expression of his freedom of spirit and generosity of mind. They show that Einstein had come to accept that quantum mechanics was not an aberration but rather a truly professional contribution to physics.

Not that from then on he desisted from criticizing quantum mechanics. He had recognized it to be part of the truth but was and forever remained deeply convinced that it was not the whole truth. From 1931 on, the issue for him was no longer the consistency of quantum mechanics but rather its completeness.

During the last twenty-five years of his life Einstein maintained that quantum mechanics was incomplete. He no longer believed that quantum mechanics was wrong but that the common view of the physics community was wrong in ascribing to the postulates of quantum mechanics a degree of finality which he held to be naive and unjustified. The content and shape of his dissent will gradually unfold in what is to follow.

In November 1931 Einstein (1932) gave a colloquium in Berlin "On the uncertainty relation." The report of this talk does not state that Einstein objected to Heisenberg's relations. Rather it conveys a sense of his discomfort about the freedom of choice to measure precisely either the color of a light ray or its time of arrival. Casimir (1977) has written to me about a colloquium which Einstein gave in Leiden with Ehrenfest in the chair. To the best of Casimir's recollection this took place in the winter of 1931–32. In his talk Einstein discussed several aspects of the clock-in-the-box experiments. In the subsequent discussion it was mentioned that no conflict with quantum mechanics exists. Einstein reacted to this statement as follows: "Ich weiss es, widerspruchsfrei ist die Sache schon, aber sie enthält meines Erachtens doch eine gewisse Härte" ("I know, this business is free of contradictions, yet in my view it contains a certain unreasonableness").

[86]The Gedankenexperiment in this paper involves a time measurement. The authors take care to arrange things so that "the rate of the clock. . . is not disturbed by the gravitational effects involved in weighing the box."

[87]The authors are "forced to conclude that there can be no method for measuring the momentum of a particle without changing its value." This statement is of course unacceptable.

[88]"According to my conviction this tenet contains without doubt a part of the ultimate truth."

By 1933 Einstein stated explicitly his conviction that quantum mechanics does not contain logical contradictions. In his Spencer lecture (1933) he said of the Schroedinger wave functions: "These functions are only supposed to determine in a mathematical way the probabilities of encountering those objects in a particular place or in a particular state of motion, if we make a measurement. This conception is logically unexceptionable and has led to important successes."

It was in 1935 that Einstein stated for the first time his own desiderata in a precise form. This is the criterion of objective reality. He continued to subscribe to this for the rest of his life.

1935 was also the year in which Einstein moved into his final home, 112 Mercer Street in Princeton. In 1932 he had been appointed to a professorship at the Institute for Advanced Study. The original intent was that each year he would divide his time evenly between Princeton and Berlin. In December 1932 he left Germany for a visit to Caltech. He never set foot in Germany again, for well-known reasons. He and his family spent the summer of 1933 in Belgium. On October 17 they arrived back in the United States and came to Princeton that same day. Shortly thereafter they moved to 2 Library Place and from there, in 1935, to Mercer Street. Except for one brief trip to Bermuda, Einstein never again left the United States.

B. Einstein on objective reality

In his Como address Bohr (1928) had remarked that quantum mechanics, like relativity theory, demands refinements of our everyday perceptions of inanimate natural phenomena: "We find ourselves here on the very path taken by Einstein of adapting our modes of perception borrowed from the sensations to the gradually deepening knowledge of the laws of Nature." Already then, in 1927, he emphasized that we have to treat with extreme care our use of language in recording the results of observations which involve quantum effects. "The hindrances met with on this path originate above all in the fact that, so to say, every word in the language refers to our ordinary perception." Bohr's deep concern with the role of language in the appropriate interpretation of quantum mechanics never ceased. In 1948 he put it as follows (Bohr, 1948): "Phrases often found in the physical literature, as 'disturbance of phenomena by observation' or 'creation of physical attributes of objects by measurements' represent a use of words like 'phenomena' and 'observation' as well as 'attribute' and 'measurement' which is hardly compatible with common usage and practical definition and, therefore, is apt to cause confusion. As a more appropriate way of expression, one may strongly advocate limitation of the use of the word *phenomenon* to refer exclusively to observations obtained under specified circumstances, including an account of the whole experiment." This usage of "phenomenon," if not generally accepted, is the one to which nearly all physicists now adhere.

In contrast to the view that the concept of phenomenon *irrevocably* includes the specifics of the experimental conditions of observation, Einstein held that one should seek for a deeper lying theoretical framework which permits the description of phenomena independently of these conditions. That is what he meant by the term "objective reality." After 1933 it was his almost solitary position that quantum mechanics is logically consistent but that it is an incomplete manifestation of an underlying theory in which an objectively real description is possible.

In an article written in 1935 together with Boris Podolsky and Nathan Rosen (Einstein, Podolsky, and Rosen, 1935), Einstein gave reasons for his position by discussing an example, simple as always. This paper "created a stir among physicists and has played a large role in philosophical discussions (Bohr, 1949, p. 232)."[89] It contains the following definition. "If without in any way disturbing a system, we can predict with certainty (i.e., with a probability equal to unity) the value of a physical quantity, then there exists an element of physical reality corresponding to this physical quantity." The authors then consider the following problem. Two particles with respective momentum and position variables (p_1, q_1) and (p_2, q_2) are in a state with definite total momentum $P = p_1 + p_2$ and definite relative distance $q = q_1 - q_2$. This of course is possible since P and q commute. The particles are allowed to interact. Observations are made on particle 1 long after the interaction has taken place. Measure p_1 and one knows p_2 without having disturbed particle 2. Therefore (in their language) p_2 is an element of reality. Next measure q_1 and one knows q_2 again without having disturbed particle 2. Therefore q_2 is also an element of reality. Therefore both p_2 and q_2 are elements of reality. But quantum mechanics tells us that p_2 and q_2 cannot be simultaneously elements of reality because of the noncommutativity of the momentum and position operators of a given particle. Therefore quantum mechanics is incomplete.

The authors stress that they "would not arrive at our conclusion if one insisted that two ... physical quantities can be regarded as simultaneous elements of reality *only when they can be simultaneously measured or predicted*" (their italics). Then follows a remark which is the key to Einstein's philosophy and which I have italicized in part.

"This [simultaneous predictability] makes the reality of p_2 and q_2 depend upon the process of measurement carried out on the first system which does not disturb the second system in any way. *No reasonable definition of reality could be expected to permit this*." The only part of this article which will ultimately survive, I believe, is this last phrase which so poignantly summarizes Einstein's views on quantum mechanics in his later years. The content of this paper has been referred to on occasion as the Einstein–Podolsky–Rosen paradox. It should be stressed that this paper contains neither a paradox nor any flaw of logic. It simply concludes that objective reality is incompatible with the assumption that quantum mechanics is complete. This conclusion has not affected subsequent developments in physics and it is doubtful that it ever will.

"It is only the mutual exclusion of any two experimental procedures, permitting the unambiguous definition of complementary physical quantities which provides room for new physical laws", Bohr (1935) wrote in his rebuttal. He did not believe that the Einstein–Podolsky–Rosen paper called for any change in the interpretation of quantum mechanics. Most physicists (including the writer) agree with this opinion. I shall reserve for the next section a further comment on the completeness of quantum theory.

This concludes an account of Einstein's position. He returned to his criterion for objective reality in a number of later papers (1948a, 1949a, 1951b, 1953) in which he repeated the EPR argument on several occasions. These papers add nothing substantially new. In one of them (1951b) he discussed a further example (I omit the details) stimulated by the question whether the quantum-mechanical notion of phenomenon should also apply to bodies of everyday size. The answer is of course in the affirmative.

Bohr was, of course, not the only one to express opposition to objective reality; nor was Einstein the only one critical of the complementarity interpretation.[90] I have chosen to confine myself to the exchanges between Einstein and Bohr because I believe that Einstein's views come out most clearly in juxtaposing them with Bohr's. Moreover I am well acquainted with their thoughts on these issues because of discussions with each of them. Bohr was in Princeton when he put the finishing touches to his 1949 article (1949) and we did discuss these matters often at that time. [It was during one of these discussions that Einstein sneaked in to steal some tobacco (Pais, 1967).] However, it needs to be stressed that other theoretical physicists and mathematicians have made important contributions to this area of problems. Experimentalists have actively participated as well. A number of experimental tests of quantum mechanics in general and also of the predictions of specific alternative schemes have been made.[91] This has not led to surprises.

The foregoing was a brief sketch of the substance of

[89]This stir reached the press. On May 4, 1935 the *New York Times* carried an article under the heading "Einstein attacks quantum theory," which also includes an interview with a physicist. Its May 7 issue contains a statement by Einstein in which he deprecated this release, which did not have his authorization.

[90]In 1950 Einstein (1950) mentioned Schroedinger and von Laue as the only ones who shared his views. There were of course many others who at that time (and later) had doubts about the complementarity interpretation, but their views and Einstein's did not necessarily coincide or overlap (see Einstein, 1952). Note also that the term "hidden variable" does not occur in any of Einstein's papers or letters, as far as I know.

[91]Typically, these tests deal with variants of the EPR arrangement, such as long-range correlations between spins or polarizations. I must admit to being insufficiently familiar with the extensive theoretical and experimental literature on these topics. My main guides have been a book by Jammer (1974) and a review article by Pipkin (1978). Both contain extensive references to other literature.

Einstein's arguments. There is another question which at least to this writer is far more fascinating, the one of motivation. What drove Einstein to use methods which he himself (1954b) called "quite bizarre as seen from the outside?" Why would he continue "to sing my solitary old little song" (Einstein, 1949b) for the rest of his life? As I shall discuss in Sec. X the answer has to do with a grand design for a synthetical physical theory which Einstein conceived early (before the discovery of quantum mechanics). He failed to reach this synthesis.

So to date have we all.

The phenomena to be explained by a synthetical theory of particles and fields have become enormously richer since the days when Einstein embarked on his unified theory. Theoretical progress has been very impressive but an all embracing theory does not exist. The need for a new synthesis is felt ever more keenly as the phenomena grow in complexity.

Therefore, in the year of Einstein's centennial, any assessment of Einstein's visions can only be made from a vantage point which is necessarily tentative. It may be useful to record ever so briefly what this vantage point appears to be to at least one physicist. This will be done next, with good wishes for the second centennial.

IX: A TIME CAPSULE[92]

> Einstein's life ended ... with a demand on us for synthesis.
>
> W. Pauli (1958)

When Einstein and others embarked on programs of unification, three particles (in the modern sense) were known to exist, the electron, the proton, and the photon, and there were two fundamental interactions, electromagnetism and gravitation. At present the number of particles runs into the hundreds. A further reduction to more fundamental units appears inevitable. The number of fundamental interactions is now believed to be at least four. The grand unification of all four types of forces, gravitational, electromagnetic, weak and strong interactions, is an active topic of current exploration. It has not been achieved as yet.

Relativistic quantum field theories (in the sense of special relativity) are the principal tools for these explorations. Our confidence in the general field theoretic approach rests first and foremost on the tremendous success of quantum electrodynamics (QED). One number, the g factor of the electron, may illustrate the level of predictability which this theory has reached:

$$\tfrac{1}{2}(g-2)=\begin{cases} 1\,159\,652\,375(261V\times10^{-12}\,, & \text{predicted by pure QED,}^{93} \\ 1\,159\,652\,410(200)\times10^{-12}\,, & \text{observed}\,. \end{cases}$$

It is nevertheless indicated that this branch of field theory has to merge with the theory of other fields.

"If we could have presented Einstein with a synthesis of his general relativity and the quantum theory, then the discussion with him would have been considerably easier" (Pauli, 1958). To date this synthesis is beset with conceptual and technical difficulties. The existence of singularities associated with gravitational collapse is considered by some as an indication for the incompleteness of the general relativistic equations. It is not known whether or not these singularities are smoothed out by quantum effects.

The ultimate unification of weak and electromagnetic interactions has not been achieved but a solid beachhead appears to have been established in terms of local non-Abelian gauge theories with spontaneous symmetry breakdown. As a result it is now widely believed that weak interactions are mediated by massive vector mesons. Current expectations are that such mesons will be observed within another decade from now.

It is widely believed that strong interactions are also mediated by local non-Abelian gauge fields. Their symmetry is supposed to be unbroken so that the corresponding vector mesons are massless. The dynamics of these "non-Abelian photons" is supposed to prohibit their creation as single free particles. The technical exploration of this theory is only in its very early stages.

Since electromagnetism and gravitation are also associated with local gauge fields it is commonly held that the grand unification will eventually be achieved in terms of a multicomponent field of this kind. This may be said to represent a program of geometrization which bears resemblance to Einstein's attempts, although the manifold subject to geometrization is larger than he anticipated.

In the search for the correct field theory, model theories have been examined which reveal quite novel possibilities for the existence of extended structures (solitons, instantons, monopoles). In the course of these investigations, topological methods have entered this area of physics. More generally, it has become clear in the past decade that quantum field theory is much richer in structure than was appreciated earlier. The renormalizability of non-Abelian gauge fields with spontaneous symmetry breakdown, asymptotic freedom, and supersymmetry are cases in point.

The proliferation of new particles has led to attempts at a somewhat simplified underlying description. According to the current picture the basic constituents of matter are: two families of spin-$\frac{1}{2}$ particles, the leptons and the quarks; a variety of spin-1 gauge bosons, some massless, some massive; and (more tentatively) some fundamental spin-zero particles. The only gauge

[92] This section is meant to provide a brief record without any attempt at further explanation or reference to literature. It can be skipped without loss of continuity.

[93] In this prediction (which does not include small contributions from muons and hadrons) the best value of the fine structure constant α has been used as an input: $\alpha^{-1}=137.035\,987(29)$. The principal source of uncertainty (Kinoshita, 1978) in the predicted value of $g-2$ stems from the experimental uncertainties of α.

boson observed so far is the photon. To date three kinds of charged leptons have been detected. The quarks are hypothetical constituents of the observed hadrons. To date at least five species of quarks are needed. The dynamics of the strong interactions is supposed to prohibit the creation of quarks as single free particles. This prohibition, confinement, has not as yet been implemented theoretically in a convincing way. No criterion is known which would enable one to state how many species of leptons and of quarks should exist.

Weak, electromagnetic, and strong interactions have distinct intrinsic symmetry properties, but this hierarchy of symmetries is not well understood theoretically. Perhaps the most puzzling are the noninvariance under space reflexion at the weak level and the noninvariance under time reversal at an even weaker level. It adds to the puzzlement that the latter phenomenon has been observed so far in only a single instance, namely in the $K^0-\bar{K}^0$ system. [These phenomena were first observed after Einstein's death. I have often wondered what might have been his reactions to these discoveries, given his "conviction that pure mathematical construction enables us to discover the concepts and the laws connecting them" (Einstein, 1933).]

It is not known why electric charge is quantized but it is plausible that this will be explicable in the framework of a future gauge theory.

In summary, at the time of the centenary of the death of James Clerk Maxwell (June 13, 1831–November 5, 1879) and the birth of Albert Einstein (March 14, 1879–April 18, 1955) the evidence is overwhelming that the theory of particles and fields is incomplete. Einstein's earlier complaint (1956a) remains valid to this day: "The theories which have gradually been connected with what has been observed have led to an unbearable accumulation of independent assumptions." At the same time no experimental evidence or internal contradiction exists to indicate that the postulates of general relativity, of special relativity, or of quantum mechanics are in mutual conflict or in need of revision or refinement. We are therefore in no position to affirm or deny that these postulates will forever remain unmodified.

X. PARTICLES, FIELDS AND THE QUANTUM THEORY: EINSTEIN'S VISION

> Apart. . . . 6. Away from common use for a special purpose.
>
> Oxford English Dictionary

A. Some reminiscences

The rest of this paper is based in part on what I learned from discussions with Einstein. I should like to mention first some reminiscences of my encounters with him.

I knew Einstein from 1946 until the time of his death[94]. I would visit with him in his office or accompany him (often together with Kurt Gödel) at lunchtime on his walk home to 112 Mercer Street. Less often I would visit him there. In all I saw him about once every few weeks. We always talked German, the language best suited to grasp the nuances of what he had in mind as well as the flavor of his personality. Only once did he visit my apartment. The occasion was a meeting of the Institute for Advanced Study faculty for the purpose of drafting a statement of our position in the 1954 Oppenheimer affair. I shall not go into Einstein's outspoken opinions on world affairs and public policy.

Einstein's company was comfortable and comforting to those who knew him. Of course he knew well that he was a legendary figure in the eyes of the world. He accepted this as a fact of his life. There was nothing in his personality to promote such attitudes. Nor did he relish them. Privately he would express annoyance if he felt that his position was misused. I recall the case of Professor X who had been quoted by the newspapers as having found solutions to Einstein's generalized equations of gravitation. Einstein said to me "Der Mann ist ein Narr" and added that in his opinion X could calculate but could not think. X had visited Einstein to discuss this work and Einstein, always courteous, said to him that if his results were true they would be important. Einstein was chagrined that he had been quoted in the papers without this proviso. He said that he would keep silent on the matter but would not receive X again. According to Einstein the whole thing started because, in his enthusiasm, X had told some colleagues who saw the value of it as publicity for their university.

To those physicists who could follow his scientific thought and who knew him personally, the legendary aspect was never in the foreground—yet it was never wholly absent. I recall an occasion, in 1947, when I gave a talk at the Institute about the newly discovered π and μ mesons. Einstein walked in just after I had begun. I remember that I was speechless for the brief moment necessary to overcome a sense of the unreal. I recall a similar moment during a symposium[95] held in Frick Chemical laboratory in Princeton on March 19, 1949. The occasion was Einstein's seventieth birthday. Most of us were in our seats when Einstein entered the

[94]In 1941, I received my Ph.D. in Utrecht with L. Rosenfeld. Some time later I went into hiding in Amsterdam. Eventually I was caught and sent to the Gestapo prison there. Those who were not executed were released a few days before V.E. Day. Immediately after the war, I applied for a postdoctoral fellowship at Niels Bohr's Institute as well as at the Institute for Advanced Study in Princeton where I hoped to work with Pauli. I received a letter from Pauli saying he would support my application. I was accepted at both places and went first for one year to Copenhagen. When I finally arrived at Princeton in September 1946 I found that Pauli had in the meantime gone to Zürich. Bohr also came to Princeton that same month. Both of us attended the Princeton Bicentennial Conference (where P. A. Schilpp approached Bohr for a contribution to the Einstein biography). Shortly thereafter Bohr introduced me to Einstein.

My stay at the Institute had lost much of its attraction because Pauli was no longer there. As I was contemplating returning to Copenhagen the next year, Oppenheimer contacted me to inform me that he had been approached for the directorship of the Institute and to ask me to join him in building up physics there. I accepted. A year later I was appointed to a five-year membership at the Institute and in 1950 to Professor. I remained at the Institute until 1963.

[95]The speakers were J. R. Oppenheimer, I. I. Rabi, E. P. Wigner, H. P. Robertson, S. M. Clemence, and H. Weyl.

hall. Again there was this brief hush before we stood to greet him.

Nor do I believe that such reactions were typical only of those who were much younger than he. There were a few occasions when Pauli and I were both together with him. Pauli, not known for an excess of awe, was just slightly different in Einstein's company. One could perceive his sense of reverence. I have also seen Bohr and Einstein together. Bohr too was affected in a somewhat similar way, differences in scientific outlook notwithstanding.

Whenever I met Einstein our conversations might range far and wide but invariably the discussion would turn to physics. Such discussions would touch only occasionally on matters having to do with the period before 1925 and then they would mainly concern relativity. I recall asking Einstein once what influence Poincaré's work had had on him. Einstein replied that he never had read Poincaré. On the other hand Einstein held no one in higher esteem than Lorentz. He once told me that without Lorentz he would never have been able to take "den Schritt" (" the step"), (Einstein always talked about relativity in an impersonal way.) Lorentz was to Einstein the most well-rounded and harmonious personality he had met in his entire life. He had also a great veneration for Planck.

Our discussions, however, centered first and foremost on the present and the future. When relativity was the issue he would often talk of his efforts to unify gravitation and electromagnetism and of his hopes for the next steps. His faith rarely wavered in the path he had chosen. Only once did he express a reservation to me when he said, in essence: I am not sure that differential geometry is the framework for further progress, but if it is, then I believe I am on the right track. (This remark must have been made sometime during his last few years.)

The main topic of discussion was quantum physics, however. Einstein never ceased to ponder the meaning of the quantum theory. Time and time again the argument would turn to quantum mechanics and its interpretation. He was explicit in his opinion that the most commonly held views on this subject could not be the last word, but he had also more subtle ways of expressing his dissent. For example he would never refer to a wave function as "die Wellenfunktion" but would always use a mathematical terminology: "die Psifunktion." We often discussed his notions on objective reality. I recall that during one walk Einstein suddenly stopped, turned to me and asked whether I really believed that the moon exists only when I look at it. The rest of this walk was devoted to a discussion of what a physicist should mean by the term "to exist."

I was never able to arouse much interest in Einstein about the new particles. It was apparent that he felt that the time was not ripe to worry about such things and that these particles would eventually appear as solutions of the equations of a unified theory. In some sense he may well prove to be right.

It was even more difficult to discuss quantum field theory with him. He was willing to admit that quantum mechanics was successful on the nonrelativistic level. However, he did not believe that this theory provided a secure enough basis for relativistic generalizations (Einstein, 1934,1946). Relativistic quantum field theory was repugnant to him (Born, 1944).[96] Valentine Bargmann has told me that Einstein asked him to give a private survey of quantum field theory, beginning with second quantization. Bargmann did so for about a month. Thereafter Einstein's interest waned.

An unconcern with the past is a privilege of youth. In all the years I knew Einstein I never read a single one of his papers from the 1905–1925 period on the quantum theory. It is now clear to me that I might have asked him some interesting questions, had I been less blessed with ignorance. I might then have known some interesting facts by now, but at a price. My discussions with Einstein were not historical interviews, they concerned live physics. I am glad it never was otherwise.

B. Einstein, Newton, and success

"It seems to be clear ... that the Born statistical interpretation of the quantum theory is the only possible one," Einstein (1936) wrote in 1936. He has also called (1949a) "the statistical quantum theory ... the most successful physical theory of our period." Then why was he never convinced by it? I believe Einstein (1933) himself answered this indirectly in his 1933 Spencer lecture—perhaps the clearest and most revealing expression of his mode of thinking. The key is to be found in his remarks on Newton and classical mechanics.

In this lecture Einstein notes that "Newton felt by no means comfortable about the concept of absolute space, ... of absolute rest ... [and] about the introduction of action at a distance." Einstein then goes on to refer to the success of Newton's theory in these words: "The enormous practical success of his theory may well have prevented him and the physicists of the eighteenth and nineteenth centuries from recognizing the fictitious character of the principles of his system." It is important to note that by "fictitious," Einstein means free inventions of the human mind. Whereupon he compares Newton's mechanics with his own work on general relativity: "The fictitious character of the principles is made quite obvious by the fact that it is possible to exhibit two essentially different bases [Newtonian mechanics and general relativistic mechanics] each of which in its consequences leads to a large measure of agreement with experience."

Elsewhere Einstein (1949a) addressed Newton as follows. "Newton forgive me: you found the only way which, in your age, was just about possible for a man with the highest power of thought and creativity." Only one man in history could have possibly written that line.

In the Spencer lecture Einstein mentioned the success not only of classical mechanics but also of the statistical interpretation of quantum theory. "This conception is logically unexceptionable and has led to important successes." But, he added, "I still believe in the possibili-

[96]W. Thirring (1977) has written to me of conversations with Einstein in which "his objections became even stronger when it concerned quantum field theory and he did not believe in any of its consequences."

ty of giving a model of reality which shall represent events themselves and not merely the probability of their occurrence."

From this lecture as well as from discussions with him on the foundations of quantum physics I have gained the following impression. Einstein tended to compare the successes of classical mechanics with those of quantum mechanics. In his view both were on a par, being successful but incomplete. For more than a decade Einstein had pondered the single question of how to extend to general motions the invariance under uniform translations. His resulting theory, general relativity, had led to only small deviations from Newton's theory. (Instances where these deviations are large were discussed only much later.) He was likewise prepared to undertake his own search for objective reality, fearless of how long it would take. He was also prepared for the survival of the practical successes of quantum mechanics, with perhaps only small modifications. It is quite plausible that the very success of his highest achievement, general relativity, was an added spur to Einstein's apartness. Yet it should not be forgotten that this trait characterized his entire oeuvre and style.

Einstein was not oblivious to others' reactions to his own position. "I have become an obstinate heretic in the eyes of my colleagues" he wrote (1949c) to one friend and "in Princeton they consider me an old fool" he said to another (Born, 1971, p. 131). He knew, and on occasion would even say, that his road was a lonely one (Einstein, 1948b), yet he held fast. "Momentary success carries more power of conviction for most people than reflections on principle" (Einstein, 1949d).

Einstein was neither saintly nor humorless in defending his solitary position on the quantum theory. On occasion he could be acerbic. At one time he said that Bohr thought very clearly, wrote obscurely and thought of himself as a prophet (Shankland, 1963). Another time he referred to Bohr as a mystic (Einstein, 1939). On the other hand, in a letter to Bohr, Einstein (1949b) referred to his own position by quoting an old rhyme: "Über die Reden des Kandidaten Jobses/Allgemeines Schütteln des Kopses."[97] There were moments of loneliness. "I feel sure that you do not understand how I came by my lonely ways ..." (Einstein, 1948c). Einstein may not have expressed all his feelings on these matters. But that was his way. "The essential of the being of a man of my type lies precisely in *what* he thinks and *how* he thinks, not in what he does or suffers" (Einstein, 1949a).

The crux of Einstein's thinking on the quantum theory was not his negative position in regard to what others had done, but rather his deep faith in his own distinct approach to the quantum problems. His beliefs may be summarized as follows.

(1) Quantum mechanics represents a major advance, yet it is only a limiting case of a theory which remains to be discovered. "There is no doubt that quantum mechanics has seized hold of a beautiful element of truth, and that it will be a test stone for a future theoretical basis, in that it must be deducible as a limiting case from that basis, just as electrostatics is deducible from the Maxwell equations of the electromagnetic field or as thermodynamics is deducible from statistical mechanics" (Einstein, 1936).

(2) One should not try to find the new theory by beginning with quantum mechanics and trying to refine or reinterpret it. "I do not believe that quantum mechanics will be the starting point in the search for this basis just as one cannot arrive at the foundations of mechanics from thermodynamics or statistical mechanics" (Einstein, 1936).

(3) Instead—and this was Einstein's main point—*one should start all over again,* as it were, and endeavor to obtain the quantum theory as a byproduct of a general relativistic field theory. As an introduction to a further discussion of this last issue it is useful to comment first on the profound differences between Einstein's attitude to relativity and to the quantum theory.

C. Relativity theory and quantum theory

Einstein's paper on light-quanta was submitted in March 1905, his first two papers on relativity in June (1905c) and September (1905d) of that year, respectively. In a letter (1905e) to a friend written early in 1905 he promised him a copy of his March paper "about radiation and the energy properties of light [which] is very revolutionary." In the same letter he also mentioned that a draft of the June paper was ready and added that "the purely kinematic part of this work will surely interest you." It is significant that Einstein would refer to his light-quantum paper but not to his relativity paper as a revolutionary step.

If a revolutionary act consists in overthrowing an existing order, then to describe the light-quantum hypothesis in those terms is altogether accurate. It is likewise fitting not to apply these terms to relativity theory since it did not overthrow an existing order but rather brought immediate order to new domains.

Einstein was one of the freest spirits there ever was. But he was not a revolutionary, as the overthrow of existing order was never his prime motivation. It was his genius that made him ask the right question. It was his faith in himself that made him persevere until he had the answer. If he had a God it was the God of Spinoza. He had to follow his own reasoning regardless of where it would lead him. He had a deep respect for the traditions of physics. But if his own reasoning indicated answers which lay outside the conventional patterns, he accepted these answers, not for the sake of contradiction but because it had to be. He had been free to ask the question. He had no choice but to accept the answer. This deep sense of destiny led him farther than anyone before him—but not as far as finding his own answer to the quantum theory.

It is striking how, from the very beginning, Einstein kept his scientific writing on relativity theory separate from that on quantum theory. This was evident already in 1905. In his first relativity paper (1905c) Einstein noted: "It is remarkable that the energy and frequency of a light complex vary with the state of motion of the observer according to the same law." Here was an

[97]Roughly: "there was a general shaking of heads concerning the words of candidate Jobs."

obvious opportunity to refer to the relation $E = h\nu$ of his paper on light-quanta, finished a few months earlier. But Einstein did not do that. Also in the September paper (1905d) he referred to radiation but not to light-quanta. In his 1909 address at Salzburg (1909a) Einstein discussed his ideas both on relativity theory and on quantum theory but kept these two areas well separated. As we have seen, in his 1917 paper (1917a) Einstein ascribed to light-quanta an energy $E = h\nu$ and a momentum $p = h\nu/c$. This paper concludes with the following remark. "Energy and momentum are most intimately related; therefore a theory can only then be considered as justified if it has been shown that according to it the momentum transferred by radiation to matter leads to motions as required by thermodynamics." Why is only thermodynamics mentioned; why not also relativity?

I believe that the reason Einstein kept the quantum theory apart from relativity theory is that he considered the former to be provisional (as he said (1912d) already in 1911) while, on the other hand, relativity to him was the revealed truth. Einstein's destiny reminds me in more than one way of the destiny of Moses.

The road to general relativity theory had been local classical field theory. The same road, he hoped, would also lead to the implementation of objective reality.

D. Einstein's vision

In 1923 Einstein published an article (1923b) entitled "Does field theory offer possibilities for the solution of the quantum problem?" It begins with a reminder of the successes achieved in electrodynamics and general relativity theory in regard to a causal description: events are causally determined by differential equations combined with initial conditions on a spacelike surface. However, Einstein continued, this method cannot be applied to quantum problems without further ado. As he put it, the discreteness of the Bohr orbits indicates that initial conditions cannot be chosen freely. Then he asked: can one nevertheless implement these quantum constraints in a (causal) theory based on partial differential equations? His answer: "Quite certainly: we must only 'overdetermine' the field variables by [appropriate] equations." Next he states his program, based on three requirements. (1) General covariance. (2) The desired equations should at least be in accordance with the gravitational and the Maxwell theory. (3) The desired system of equations which overdetermines the fields should have static spherically symmetric solutions which describe the electron and the proton. If this overdetermination can be achieved then "we may hope that these equations co-determine the mechanical behavior of the singular points (electrons) in such a way that also the initial conditions of the field and the singular points are subject to restrictive conditions." He goes on to discuss a tentative example and concludes as follows: "To me the main point of this communication is the idea of overdetermination."

This paper contains all the essential ingredients of the vision on particles, fields, and the quantum theory which Einstein was to pursue for the rest of his life.

To Einstein the concept of a unified field theory meant something different from what it meant and means to anyone else. He demanded that the theory shall be strictly causal, that it shall unify gravitation and electromagnetism, that the particles of physics shall emerge as special solutions of the general field equations, and that the quantum postulates *shall be a consequence* of the general field equations. Einstein had all these criteria in mind when he wrote (1949a) in 1949: "Our problem is that of finding the field equations of the total field."

Already in 1923 he had been brooding on these ideas for a number of years. In 1920 he had written (1920c) to Born: "I do not seem able to give tangible form to my pet idea ["meine Lieblingsidee"], which is to understand the structure of the quanta by redundancy in determination, using differential equations." It is the earliest reference to Einstein's strategy that I am aware of. It would seem likely that ideas of this kind began to stir in Einstein soon after 1917, when he had not only completed the general theory of relativity but had also confronted the lack of causality in spontaneous emission (Einstein, 1917a). The early response of others to these attempts by Einstein has been recorded by Born (1971, p. 88): "In those days [early in 1925] we all thought that his objective ... was attainable and also very important." Einstein himself (1924j) felt that he had no choice: "The road may be quite wrong but it must be tried."

As I already intimated in the Introduction, it is essential for the understanding of Einstein's thinking to realize that there were two sides to his attitude concerning quantum physics. There was Einstein the critic, never yielding in his dissent from complementarity, and there was Einstein the visionary, forever trying to realize the program outlined above, which went well beyond a mere reinterpretation of quantum mechanics (Einstein, 1952). His vision predates quantum mechanics; it was certainly with him in 1920 and probably even a few years earlier.

A detailed description of his efforts in this direction belongs to a history of unified field theory, a topic which cannot be dealt with here. In concluding this paper I shall confine myself to a few brief observations concerning Einstein's own attitude toward his program.

Einstein believed that the field equations would generate particles with nonzero spin as particle-like solutions which are not spherically symmetrical (V. Bargmann, private communication). Presumably he hoped that his idea of overdetermination would lead to discrete spin values.[98] He also hoped that the future theory would contain solutions which are not absolutely localized and which would carry quantized electric charge (Einstein, 1933). [In 1925 Einstein noted (1925f) that if the combined gravitational/electromagnetic field equations have particle-like solutions with charge e and mass m, then there should also be solutions[99] with

[98] I note in passing that in 1925 Einstein gave a helping hand to Uhlenbeck and Goudsmit in the explanation of the origins of the spin-orbit coupling of electrons in atoms (Uhlenbeck, 1976).

[99] The proof involves the application of time reversal to the combined equations. In related context the existence of the $(\pm e, m)$ solutions was first noted by Pauli (1919) in 1919.

$(-e, m)$! This led him to doubt for some time that the unification of gravitation and electromagnetism was consistently possible.]

Einstein's correspondence shows that the unified field theory and the quantum problems were very often simultaneously on his mind. Here are but a few examples. In 1925, while he was at work on a theory with a nonsymmetric metric he wrote (1925g) to a friend: "Now the question is whether this field theory is compatible with the existence of atoms and quanta." He discussed the same generalized theory in a letter (1942c) written in 1942: "What I am doing now may seem a bit crazy to you. One must note, however, that the wave-particle duality demands something unheard of." In 1949 (1949e): "I am convinced that the ... statistical [quantum] theory ... is superficial and that one must be backed by the principle of general relativity." And in 1954 (1954b): "I must seem like an ostrich who forever buries his head in the relativistic sand in order not to face the evil quanta."

Forever and in vain Einstein kept looking for hints which would help him to realize his vision of a quantum theory derived from a unified field theory. This urge explains his reference to the quantum theory at unexpected places. In 1930 he gave a lecture on unified field theory, a report of which (cabled to the New York Times) contains the statement: "He emphasized that he is in no way taking notice of the results of quantum calculations because he believes that by dealing with microscopic phenomena these will come out by themselves" (Einstein, 1930b). A report in 1931 by Einstein (1931b) on a five-dimensional theory which should unify gravitation and electromagnetism ends as follows. "This theory does not yet contain the conclusions of the quantum theory." Two months after the Einstein–Podolsky–Rosen article, Einstein and Rosen (1935) completed another paper, this one dealing with singularity-free solutions of the gravitational-electromagnetic field equations. One phrase in this paper, "One does not see *a priori* whether the theory contains the quantum phenomena," illustrates once again the scope of the program which was on Einstein's mind.

Simplicity was the guide in Einstein's quest: "In my opinion there is *the* correct path and ... it is in our power to find it. Our experience up to date justifies us in feeling sure that in Nature is actualized the ideal of mathematical simplicity" (Einstein, 1933). Already in 1927 Heisenberg (1927a) stressed, in a letter to Einstein mentioned earlier, that Einstein's concept of simplicity and the simplicity inherent in quantum mechanics cannot both be upheld. "If I have understood correctly your point of view than you would gladly sacrifice the simplicity [of quantum mechanics] to the principle of [classical] causality. Perhaps we could comfort ourselves [with the idea that] the dear Lord could go beyond [quantum mechanics] and maintain causality. I do not really find it beautiful, however, to demand physically more than a description of the connection between experiments."

As Einstein's life drew to a close, doubts about his vision arose in his mind.

He wrote to Born, probably in 1949 (see Born, 1971, p. 180): "Our respective hobby horses have irretrievably run off in different directions ... even I cannot adhere to [mine] with absolute confidence." I have mentioned before the reservations which Einstein expressed to me in the early fifties.[100] To his dear friend Besso he wrote (1954c), in 1954: "I consider it quite possible that physics cannot be based on the field concept, i.e., on continuous structures. In that case *nothing* remains of my entire castle in the air, gravitation theory included, [and of] the rest of modern physics." It is to be doubted whether any physicist can be found who would not respectfully and gratefully submit that this judgment is unreasonably harsh.

Otto Stern has recalled a statement which Einstein once made to him: "I have thought a hundred times as much about the quantum problems as I have about general relativity theory" (Jost, 1977). He kept thinking about the quantum till the very end. Einstein (1956b) wrote his last autobiographical sketch in Princeton, in March 1955, about a month before his death. Its final sentences deal with the quantum theory. "It appears dubious whether a field theory can account for the atomistic structure of matter and radiation as well as of quantum phenomena. Most physicists will reply with a convinced 'No,' since they believe that the quantum problem has been solved in principle by other means. However that may be, Lessing's comforting word stays with us: the aspiration to truth is more precious than its assured possession.

During the last years of his life Einstein was not well. "We around him knew since about five years of the sword of Damocles hanging over us. He knew it too and waited for it calmly and smilingly" (Dukas, 1955). His final illness was not long. On April 15 he entered Princeton hospital. He refused to be operated on. "I want to go when *I* want—I have done my share, it is time to go—I will do it elegantly" (Dukas, 1955). Einstein died in the early morning hours of April 18.

XI. EPILOG

I saw Einstein for the last time in December 1954. As he had not been well he had for some weeks been absent from the Institute where he normally spent a few hours each morning. Since I was about to take a term's leave from Princeton, I called Helen Dukas and asked her to be kind enough to give my best wishes to Professor Einstein. She suggested I might come to the house for a brief visit and a cup of tea. I was of course glad to accept. After I arrived, I went upstairs and knocked at the door of Einstein's study. There was his gentle "come." As I entered he was seated in his arm chair, a blanket over his knees, a pad on the blanket. He was working. He put his pad aside at once and greeted me. We spent a pleasant half hour or so; I do not recall what was discussed. Then I told him I should not stay any longer. We shook hands, and I said goodbye. I walked to the door of the study, not more than four or five steps away. I turned around as I opened

[100] V. Bargmann informs me that Einstein made similar remarks to him in the late thirties (see also Einstein, 1941).

the door. I saw him in his chair, his pad back on his lap, a pencil in his hand, oblivious to his surroundings. He was back at work.

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Allison, S. K., 1965, Biogr. Mem. Nat. Acad. Sci. **38**, 81.
Arrhenius, S., 1965 in *Nobel Lectures in Physics* (Elsevier, New York), Vol. 1, pp. 478–481.
Avogadro, A., 1833, Ann. Chim. Phys. **55**, 80.
Balmer, J. J., 1885, Ann. Phys. (Leipz.) **25**, 80.
Bateman, H., 1923, Philos, Mag. **46**, 977.
Bay, Z., V. P. Henri, and F. McLennon, 1955, Phys. Rev. **97**, 1710.
Behn, U., 1893, Ann. Phys. (Leipz.) **48**, 708.
Benz, U., 1975, *Arnold Sommerfeld* (Wissenschaftliche Verlagsgesellschaft, Stuttgart), p. 74.
Blanpied, W. A., 1972, Am. J. Phys. **40**, 1212.
Bohr, N., 1913a, Philos. Mag. **26**, 1.
Bohr, N., 1913b, Nature (Lond.) **92**, 231.
Bohr, N., 1920, letter to A. Einstein, June 24 (unpublished), housed in the Albert Einstein Archives at the Institute for Advanced Study, Princeton, N.J.).
Bohr, N., 1923, Z. Phys. **13**, 117, especially Section 4.
Bohr, N., 1925a, Z. Phys. **34**, 142.
Bohr, N., 1925b, letter to R. H. Fowler, April 21, quoted in Stuewer (1975), p. 301.
Bohr, N., 1925c, Z. Phys. **34**, 142, Appendix.
Bohr, N., 1928, Nature (Lond.) **121**, 580.
Bohr, N., 1935, Phys. Rev. **48**, 696.
Bohr, N., 1948, Dialectica **2**, 312.
Bohr, N., 1949, in P. A. Schilpp, editor, *Albert Einstein, Philosopher-Scientist* (Library of Living Philosophers, Evanston, Ill.), p. 199.
Bohr, N., H. A. Kramers, and J. C. Slater, 1924, Philos, Mag. **47**, 785.
Boltzmann, L., 1871, Wien. Ber. **63**, 679.
Boltzmann, L., 1876, Wien. Ber. **74**, 553.
Boltzmann, L., 1884, Ann. Phys. (Leipz.) **22**, 31, 291.
Boltzmann, L., 1897, *Vorlesungen über die Principe der Mechanik* (Barth, Leipzig), Vol. 1, p. 9; reprinted, 1974, by Wiss. Buchges. Darmstadt.
Boltzmann, L., 1912, *Vorlesungen über Gastheorie* (Barth, Leipzig), Vol. 2, p. 131.
Born, M., 1926, Z. Phys. **37**, 863.
Born, M., 1944, letter to A. Einstein, October 10, in Born (1971), p. 155.
Born, M., 1971, editor, *The Born–Einstein Letters* (Walker, New York), p. 91.
Born, M., W. Heisenberg, and P. Jordan, 1925, Z. Phys. 35, 557.
Born, M., and Th. von Kármán, 1912, Phys. Z. **13**, 297.
Born, M., and Th. von Kármán, 1913, Phys. Z. **14**, 15.
Bose, S. N., 1924a, letter to A. Einstein, June 4 (unpublished, Einstein Archives).
Bose, S. N., 1924b, Z. Phys. **26**, 178.
Bothe, W., 1923, Z. Phys. **20**, 145.
Bothe, W., and H. Geiger, 1924, Z. Phys. **26**, 44.
Bothe, W., and H. Geiger, 1925a, Naturwissenschaften **13**, 440.
Bothe, W., and H. Geiger, 1925b, Z. Phys. **32**, 639.
Brillouin, L., 1921, J. Phys. (Paris) **2**, 142.
Casimir, H. B. G., 1977, letter to A. Pais, December 31.
Chadwick, J., 1962, *Proceedings of the Tenth International Congress on the History of Science*, Ithaca (Hermann, Paris), Vol. 1, p. 159.
Compton, A. H., 1923, Phys. Rev. **21**, 483.
Compton, A. H., and A. W. Simon, 1925, Phys. Rev. **26**, 889.
Curie, P., and G. Sagnac, 1900, C. R. Acad. Sci. (Paris) **130**, 1013.
Davis, L., A. S. Goldhaber, and M. M. Nieto, 1975, Phys. Rev. Lett. **35**, 1402.
de Broglie, L., 1923a, C. R. Acad. Sci. (Paris) **177**, 630.
de Broglie, L., 1923b, C. R. Acad. Sci. (Paris) **177**, 507.
de Broglie, L., 1923c, C. R. Acad. Sci. (Paris) **177**, 548.
de Broglie, L., 1962, *New Perspectives in Physics* (Basic, New York), p. 150.
de Broglie, L., 1963, Preface to his reedited 1924 Ph.D. Thesis, *Recherches sur la Théorie des Quanta* (Masson et Cie., Paris), p. 4.
de Broglie, 1978a, letter to A. Pais, August 9.
de Broglie, 1978b, letter to A. Pais, September 26.
Debye, P., 1910, Ann. Phys. (Leipz.) **33**, 1427.
Debye, P., 1912, Ann. Phys. (Leipz.) **39**, 789.
Debye, P., 1923, Phys. Z. **24**, 161.
de Hevesy, 1913, letter to E. Rutherford, October 14, quoted in A. S. Eve, 1939, *Rutherford* (Cambridge University Press, Cambridge, England), p. 226.
de la Rive, A., and F. Marcet, 1840, Ann. Chim. Phys. **75**, 113.
Dewar, J., 1872, Philos. Mag. **44**, 461.
Dewar, J., 1905, Proc. R. Soc. Lond. **76**, 325.
Dirac, P. A. M., 1926, Proc. R. Soc. A **112**, 661.
Dirac, P. A. M., 1927, Proc. R. Soc. A **114**, 243.
Dirac, P. A. M., 1931, Proc. R. Soc. A **133**, 60.
Dirac, P. A. M., 1977, in *History of Twentieth Century Physics*, Varenna Summer School (Academic, New York), pp. 133–134.
Duane, W., and F. L. Hunt, 1915, Phys. Rev. **6**, 166.
Dukas, H., 1955, letter to A. Pais, April 30.
Ehrenfest, P., 1922, letter to A. Einstein, January 17 (unpublished, Einstein Archives).
Ehrenfest, P., 1925, Z. Phys. **34**, 362.
Ehrenfest, P., 1926, letter to A. Einstein, April 7 (unpublished, Einstein Archives).
Einstein, A., 1902, Ann. Phys. (Leipz.) **9**, 417.
Einstein, A., 1903, Ann. Phys. (Leipz.) **11**, 170.
Einstein, A., 1904, Ann. Phys. (Leipz.) **14**, 354.
Einstein, A., 1905a, Ann. Phys. (Leipz.) **17**, 132.
Einstein, A., 1905b, Ann. Phys. (Leipz.) **17**, 549.
Einstein, A., 1905c, Ann. Phys. (Leipz.) **17**, 891.
Einstein, A., 1905d, Ann. Phys. (Leipz.) **18**, 639.
Einstein, A., 1905e, letter to Conrad Habicht, undated but probably written in March; reproduced in Seelig (1954), pp. 88, 89.
Einstein, A., 1906a, Ann. Phys. (Leipz.) **19**, 289.
Einstein, A., 1906b, Ann. Phys. (Leipz.) **20**, 199.
Einstein, A., 1907a, Ann. Phys. (Leipz.) **22**, 180.
Einstein, A., 1907b, Ann. Phys. (Leipz.) **22**, 800.
Einstein, A., 1908, letter to J. J. Laub, quoted in Seelig (1954), p. 103.
Einstein, A., 1909a, Phys. Z. **10**, 817.
Einstein, A., 1909b, Phys. Z. **10**, 185.
Einstein, A., 1911a, Ann. Phys. (Leipz.) **34**, 175.
Einstein, A., 1911b, Ann. Phys. (Leipz.) **34**, 170.

Einstein, A., 1911c, Ann. Phys. (Leipz.) **35**, 679.
Einstein, A., 1912a, Ann. Phys. (Leipz.) **37**, 832.
Einstein, A., 1912b, Ann. Phys. (Leipz.) **38**, 881.
Einstein, A., 1912c, Ann. Phys. (Leipz.) **38**, 888.
Einstein, A., 1912d, in *La théorie du rayonnement et les quanta,* Proceedings of the First Solvay Conference, edited by P. Langevin and M. de Broglie (Gauthier-Villars, Paris).
Einstein, A., 1913, Naturwissenschaften **1**, 1077.
Einstein, A., 1914, Verh. Dtsch. Phys. Ges. **16**, 820.
Einstein, A., 1916a, letter to M. Besso, November 18, in Speziali (1972), p. 78.
Einstein, A., 1916b, Verh. Dtsch. Phys. Ges. **18**, 318.
Einstein, A., 1916c, Mitt. Ph. Ges. Zürich **16**, 47.
Einstein, A., 1916d, letter to M. Besso, Sept. 6, in Speziali (1972), p. 82.
Einstein, A., 1917a, Phys. Z. **18**, 121.
Einstein, A., 1917b, Verh. Dtsch. Phys. Ges. **19**, 82.
Einstein, A., 1917c, letter to M. Besso, March 9, in Speziali (1972), p. 103.
Einstein, A., 1918, letter to M. Besso, July 29, in Speziali (1972), p. 130.
Einstein, A., 1919, postcard to M. Planck, October 23, in Seelig (1954), p. 193.
Einstein, A., 1920a, letter to M. Born, Jan. 27, in Born (1971), p. 23.
Einstein, A., 1920b, letter to N. Bohr, May 2 (unpublished, Einstein Archives).
Einstein, A., 1920c, letter to M. Born, March 3, in Born (1971), p. 26.
Einstein, A., 1921, Sitzungsber. Preuss. Akad. Wiss. Phys.-Math. Kl., p. 882.
Einstein, A., 1922, Sitzungsber. Preuss. Akad. Wiss. Phys.-Math. Kl., p. 18.
Einstein, A., 1923a, letter to N. Bohr, January 11.
Einstein, A., 1923b, Sitzungsber. Preuss. Akad. Wiss. Phys.-Math. Kl., p. 359.
Einstein, A., 1924a, letter to M. Besso, May 24, in Speziali (1972), p. 201.
Einstein, A., 1924b, "Das Komptonsche Experiment," Berliner Tageblatt, April 20.
Einstein, A., 1924c, letter to K. Joel, November 3 (unpublished, Einstein Archives).
Einstein, A., 1924d, letter to P. Ehrenfest, May 31 (unpublished, Einstein Archives).
Einstein, A., 1924e, undated document in the Einstein Archives, obviously written in 1924.
Einstein, A., 1924f, letter to M. Born, April 29, in Born (1971), p. 82.
Einstein, A., 1924g, letter to P. Ehrenfest, July 12 (unpublished, Einstein Archives).
Einstein, A., 1924h, Sitzungsber. Preuss. Akad. Wiss. Phys.-Math. Kl., p. 261.
Einstein, A., 1924i, letter to H. A. Lorentz, December 16 (unpublished, Einstein Archives).
Einstein, A., 1924j, letter to M. Besso, Jan. 5, in Speziali (1972), p. 197.
Einstein, A., 1925a, Sitzungsber. Preuss. Akad. Wiss. Phys.-Math. Kl., p. 3.
Einstein, A., 1925b, Sitzungsber. Preuss. Akad. Wiss. Phys.-Math. Kl., p. 18.
Einstein, A., 1925c, Z. Phys. **31**, 784.
Einstein, A., 1925d, letter to R. A. Millikan, Sept. 1 (unpublished, Einstein Archives).
Einstein, A., 1925e, letter to M. Besso, Dec. 25, 1925, in Speziali (1972), p. 215.
Einstein, A., 1925f, Physica **5**, 330.
Einstein, A., 1925g, letter to M. Besso, July 28, in Speziali (1972), p. 209.
Einstein, A., 1926a, letter to P. Ehrenfest, April 12 (unpublished, Einstein Archives).
Einstein, A., 1926b, letter to Mrs. M. Born, March 7, in Born (1971), p. 88.
Einstein, A., 1926c, letter to E. Schroedinger, April 26, reproduced in M. Klein, 1967, translator, *Letters on Wave Mechanics* (Philosophical Library, New York).
Einstein, A., 1926d, letter to M. Besso, May 1, in Speziali (1972), p. 224.
Einstein, A., 1926e, letter to M. Born, Dec. 4, in Born (1971), p. 90.
Einstein, A., 1926f, in Born (1971), p. 91.
Einstein, A., 1928, in *Electrons et Photons,* the Proceedings of the Fifth Solvay Conference (Gauthier-Villars, Paris), p. 253.
Einstein, A., 1930a, Sitzungsber. Preuss. Akad. Wiss. Phys.-Math. Kl., p. 543.
Einstein, A., 1930b, Science **71**, 608.
Einstein, A., 1931a, letter to the Nobel Committee in Stockholm, Sept. 30 (unpublished, Einstein Archives).
Einstein, A., 1931b, Science **74**, 438.
Einstein, A., 1932, Z. Angew. Chem. **45**, 23.
Einstein, 1933, *On the method of theoretical physics* (Oxford U.P., New York), reprinted in Philos. Sci. **1**, 162 (1934).
Einstein, A., 1934, letter to M. Born, March 22, in Born (1971), p. 121.
Einstein, A., 1936, J. Franklin Inst. **221**, 313.
Einstein, A., 1939, letter to E. Schroedinger, August 9, in M. Klein, translator, 1967, *Letters on Wave Mechanics* (Philosophical Library, New York), p. 32.
Einstein, A., 1941, letter to L. Infeld, March 6 (unpublished, Einstein Archives).
Einstein, A., 1942a, Sci. Monthly **54**, 195.
Einstein, A., 1942b, letter to C. Lanczos, March 21 (unpublished, Einstein Archives).
Einstein, A., 1942c, letter to M. Besso, August, in Speziali (1972), p. 366.
Einstein, A., 1946, letter to A. Sommerfeld, December 14, in A. Hermann, editor, *Albert Einstein/Arnold Sommerfeld Briefwechsel* (Schwabe, Basel), p. 121.
Einstein, A., 1948a, Dialectica **2**, 320.
Einstein, A., 1948b, letter to M. Born, March 18, in Born (1971), p. 163.
Einstein, A., 1948c, letter to M. Born, March 18, in Born (1971), p. 162.
Einstein, A., 1949a, "Autobiographical Notes," in P. A. Schilpp, editor, 1949, *Albert Einstein: Philosopher-Scientist* (Library of Living Philosophers, Evanston, Illinois).
Einstein, A., 1949b, letter to N. Bohr, April 4 (unpublished, Einstein Archives).
Einstein, A., 1949c, letter to M. Besso, August 8, in Speziali (1972), p. 407.
Einstein, A., 1949d, letter to M. Besso, July 24, in Speziali (1972), p. 402.
Einstein, A., 1949e, letter to M. Besso, August 16, in Speziali (1972), p. 409.
Einstein, A., 1950, letter to E. Schroedinger, Dec. 22, in M. Klein, 1967, translator, *Letters on Wave Mechanics* (Philosophical Library, New York), p. 36.
Einstein, A., 1951a, letter to M. Besso, Dec. 12, in Speziali (1972), p. 453.
Einstein, A., 1951b, in *Scientific Papers Presented to Max Born* (Hafner, New York), p. 33.
Einstein, A., 1952, letter to M. Born, May 12, in Born (1971), p. 192.
Einstein, A., 1953, in *Louis de Broglie, Physician et Penseur* (Michel, Paris), p. 5.
Einstein, A., 1954a, letter to B. Becker, March 20 (unpublished, Einstein Archives).
Einstein, A., 1954b, letter to L. de Broglie, Feb. 8 (unpublished, Einstein Archives).
Einstein, A., 1954c, letter to M. Besso, August 10, in Speziali (1972), p. 525.
Einstein, A., 1956a, *Lettres a Maurice Solovine* (Gauthier-Villars, Paris), p. 130.
Einstein, A., 1956b, in *Helle Zeit, Dunkle Zeit,* edited by

C. Seelig (Europa, Zürich).
Einstein, A., and P. Ehrenfest, 1922, Z. Phys. **11**, 31.
Einstein, A., and P. Ehrenfest, 1923, Z. Phys. **19**, 301.
Einstein, A., and L. Hopf, 1910, Ann. Phys. (Leipz.) **33**, 1105.
Einstein, A., B. Podolsky, and N. Rosen, 1935, Phys. Rev. **47**, 777.
Einstein, A., and N. Rosen, 1935, Phys. Rev. **48**, 73.
Einstein, A., and O. Stern, 1913, Ann. Phys. (Leipz.) **40**, 551.
Einstein, A., R. Tolman, and B. Podolsky, 1931, Phys. Rev. **37**, 780.
Elster, J., and H. Geitel, 1890, Ann. Phys. (Leipz.) **41**, 166.
Eucken, A., 1912, Sitzungsber, Preuss. Akad. Wiss. Phys.-Math. Kl., p. 141.
Fermi, E., 1926, Z. Phys. **36**, 902.
Fox, R., 1968, Brit. J. Hist. Sci. **4**, 1.
Gibbs, J. W., 1902, *Elementary Principles of Statistical Mechanics* (Yale University, New Haven, Conn.), Chap. 7.
Gonzalez, J. J., and H. Wergeland, 1973, K. Nor. Vidensk. Selsk. Skr., No. 4.
Goudsmit, S., 1978, letter to A. Pais, Jan. 16.
Hallwachs, W., 1888, Ann. Phys. (Leipz.) **33**, 310.
Heisenberg, W., 1924, letter to W. Pauli, June 8 (unpublished; housed at Salle Pauli, CERN, Geneva).
Heisenberg, W., 1925, letter to A. Einstein, Nov. 30 (unpublished, Einstein Archives).
Heisenberg, W., 1926a, Z. Phys. **38**, 411.
Heisenberg, W., 1926b, Z. Phys. **33**, 879.
Heisenberg, W., 1927a, letter to A. Einstein, June 10 (unpublished, Einstein Archives).
Heisenberg, W., 1927b, letter to A. Einstein, May 19 (unpublished, Einstein Archives).
Heisenberg, W., 1927c, Z. Phys. **43**, 172.
Heisenberg, W., 1929, Naturwissenschaften **17**, 490.
Heisenberg, W., 1955, in *Niels Bohr and the Development of Physics*, edited by W. Pauli (McGraw-Hill, New York), p. 12.
Hermann, A., 1969, *Frühgeschichte der Quantentheorie, 1899–1913* (Mosbach, Baden).
Hertz, H., 1887, Ann. Phys. (Leipz.) **31**, 983.
Hettner, G., 1922, Naturwissenschaften **10**, 1033.
Huang, K., 1963, *Statistical Mechanics* (Wiley, New York), p. 26.
Hughes, A. L., 1912, Trans. R. Soc. **212**, 205.
Jammer, M., 1974, *The Philosophy of Quantum Mechanics* (Wiley, New York).
Jauch, J. M., and F. Rohrlich, 1955, *The Theory of Photons and Electrons* (Addison-Wesley, Reading, Mass.), p. 40.
Jeans, J. H., 1905a, Philos. Mag. **10**, 91.
Jeans, J. H., 1905b, Nature (Lond.) **72**, 293.
Jeans, J. H., 1914, *Report on Radiation and the Quantum Theory* (The Electrician, London), p. 59.
Joel, K., 1924, letter to A. Einstein, Oct. 28 (unpublished, Einstein Archives).
Jost, R., 1977, letter to A. Pais, August 17.
Jost, R., 1979, Vierteljahresschr. Naturforsch. Ges. Zürich **124**, 5.
Kahn, B., and G. E. Uhlenbeck, 1938, Physica **4**, 399.
Kangro, H., 1976, *History of Planck's Radiation Law* (Taylor and Francis, London).
Keesom, W. H., 1942, *Helium* (Elsevier, New York).
Kelvin, W. T., 1901, Philos. Mag. **2**, 1.
Kinoshita, T., 1978, in *New Frontiers in High Energy Physics* (Plenum, New York), p. 127.
Kirchhoff, G., 1859, Monatsber. Berlin, p. 662.
Kirchhoff, G., 1860, Ann. Phys. und Chemie **109**, 275.
Kirchhoff, G., and R. Bunsen, 1860, Ann. Phys. und Chem. **110**, 160.
Kirsten, G., and H. Körber, 1975, *Physiker über Physiker* (Akademisches Verlag, Berlin), p. 201.
Klein, M., 1958, Proc. Ned. Akad. Wet. Amsterdam **62**, 41, 51.
Klein, M. J., 1964, Nat. Philos. **3**, 1.
Klein, M., 1965, Science **148**, 173.
Klein, M., 1967, Science **157**, 509.
Klein, M., 1970, Hist. Stud. Phys. Sci. **2**, 1.
Klein, M., 1977, "The Beginnings of the Quantum Theory," in *History of Twentieth Century Physics* (Academic, New York).
Klein, M., 1979, in the *Proceedings of the Einstein Centennial Symposium*, Princeton, N.J., H. Woolf, ed. March (Addison-Wesley, Reading, Mass.).
Kundt, A., and E. Warburg, 1876, Ann. Phys. (Leipz.) **157**, 353.
Ladenburg, R., 1909, Jahrb. Radioakt. Elektron. **17**, 93, 273.
Landolt, H., and R. Bornstein, 1905, *Physikalisch Chemische Tabellen*, 3rd edition (Springer, Berlin), p. 384.
Langley, S. P., 1886, Philos. Mag. **21**, 394.
Lenard, P., 1902, Ann. Phys. (Leipz.) **8**, 149.
Lenard, P., and M. Wolf, 1889, Ann. Phys. (Leipz.) **37**, 443.
Le Verrier, U. J. J., 1859, C. R. Acad. Sci. (Paris) **49**, 379.
Lewis, G. N., 1907, J. Am. Chem. Soc. **29**, 1165, 1516.
Lewis, G. N., 1926, Nature (Lond.) **118**, 874.
London, F., 1938a, Nature (Lond.) **141**, 643.
London, F., 1938b, Phys. Rev. **54**, 947.
Lorentz, H. A., 1903, in *Collected Works*, 1936 (Nyhoff, the Hague), Vol. 3, p. 155.
Lorentz, H. A., 1909, letter to W. Wien, April 12, quoted by Hermann (1969), p. 68.
Lorentz, H. A., 1916, *Les Théories Statistiques en Thermodynamique* (Teubner, Leipzig), p. 59.
Lorentz, H. A., 1927, *Lectures in Theoretical Physics* (McMillan, London), Vol. 2, p. 171.
Lummer, O., and E. Pringsheim, 1900, Verh. Dtsch. Phys. Ges. **2**, 163.
Maxwell, J. C., 1965, "The Scientific Papers of James Clerk Maxwell," edited by W. P. Niven (Dover, New York), Vol. 2, p. 418.
Mehra, J., 1975, "Satyendra Nath Bose," Biogr. Mem. Fellows R. Soc. **21**, 117.
Millikan, R., 1913, Science **37**, 119.
Millikan, R. A., 1914, Phys. Rev. **4**, 73.
Millikan, R. A., 1915, Phys. Rev. **6**, 55.
Millikan, R. A., 1916a, Phys. Rev. **7**, 18.
Millikan, R. A., 1916b, Phys. Rev. **7**, 355.
Millikan, R. A., 1949, Rev. Mod. Phys. **21**, 343.
Nernst, W., 1906a, Sitzungsber. Preuss. Akad. Wiss. Phys.-Math. Kl., p. 933.
Nernst, W., 1906b, Gött. Nachr., p. 1.
Nernst, W., 1910, Sitzungsber. Preuss. Akad. Wiss. Phys.-Math. Kl., p. 262.
Nernst, W., 1911a, Sitzungsber. Preuss. Akad. Wiss. Phys.-Math. Kl., p. 306.
Nernst, W., 1911b, Sitzungsber. Preuss. Akad. Wiss. Phys.-Math. Kl., p. 65.
Nernst, W., 1914, Z. Elektrochem. **20**, 397.
Nernst, W., 1916, Verh. Dtsch. Phys. Ges. **18**, 83.
Nernst, W., and F. Lindemann, 1911, Sitzungsber. Preuss. Akad. Wiss. Phys.-Math. Kl., p. 494.
Neumann, F. E., 1831, Ann. Phys. (Leipz.) **23**, 32.
Nichols, E. F., 1893, Phys. Rev. **1**, 1.
Ornstein, L. S., and F. Zernike, 1919, Proc. K. Ned. Akad. Wet. Amsterdam **28**, 280.
Pais, A., 1948, letter dated Dec. 9.
Pais, A., 1967, in *Niels Bohr* (North-Holland, Amsterdam), p. 215.
Pais, A., 1977, Rev. Mod. Phys. **49**, 925.
Paschen, W., 1897, Ann. Phys. (Leipz.) **60**, 662.
Pauli, W., 1919, Phys. Z. **20**, 457.
Pauli, W., 1923, Z. Phys. **18**, 272.
Pauli, W., 1924, letter to N. Bohr, Oct. 2 (unpublished, housed at Salle Pauli, CERN, Geneva).
Pauli, W., 1925a, letter to H. A. Kramers, July 27 (unpublished, housed at Salle Pauli, CERN, Geneva).
Pauli, W., 1925b, Z. Phys. **31**, 625.
Pauli, W., 1927, Z. Phys. **41**, 81.
Pauli, W., 1949, in P. A. Schilpp, editor, 1949, *Albert Einstein: Philosopher-Scientist* (Library of Living Philosophers,

Evanston, Illinois), p. 156.
Pauli, W., 1958, Neue Züricher Zeitung, Jan. 12, reprinted in Pauli (1964), Vol. 2, p. 1362.
Pauli, W., 1964, Collected Sci. Papers (Interscience Publishers, N. Y.), Vol. 1.
Petit, A. T., and P. L. Dulong, 1819, Ann. Chim. Phys. **10**, 395.
Pipkin, F. M., 1978, Adv. At. Mol. Phys. (Academic, New York), Vol. 14.
Planck, M., 1900a, Verh. Dtsch. Phys. Ges. **2**, 202.
Planck, M., 1900b, Verh. Dtsch. Phys. Ges. **2**, 237.
Planck, M., 1900c, Ann. Phys. (Leipz.) **1**, 69.
Planck, M., 1901a, Ann. Phys. (Leipz.) **4**, 553.
Planck, M., 1901b, Ann. Phys. (Leipz.) **4**, 564.
Planck, M., 1907, letter to A. Einstein (unpublished, Einstein Archives).
Planck, M., 1909, Phys. Z. **10**, 825.
Planck, M., 1911, "*Vorlesungen über Thermodynamik*" (Von Veit, Leipzig), 3rd ed., Introduction and Section 292.
Planck, M., 1958, "Wissenschaftliche Selbstbiographie," in Max Planck, Physikalische Abhandlungen and Vorträge, edited by M. von Laue (Vieweg, Braunschweig), Vol. 3, p. 374.
Pohl, R., and L. Pringsheim, 1913, Philos. Mag. **26**, 1017.
Rayleigh, J. W. S., 1900a, Philos. Mag. **49**, 539.
Rayleigh, J. W. S., 1900b, Philos. Mag. **49**, 98.
Rayleigh, J. W. S., 1905a, Nature (Lond.) **72**, 54.
Rayleigh, J. W. S., 1905b, Nature (Lond.) **72**, 243.
Regnault, H. V., 1841, Ann. Chim. Phys. **1**, 129.
Richarz, F., 1893, Ann. Phys. (Leipz.) **48**, 708.
Rosenfeld, L., 1968, in "Fundamental Problems in Elementary Particle Physics," *Proceedings of the Fourteenth Solvay Conference* (Interscience, New York, 1968), p. 232.
Rubens, H., and F. Kurlbaum, 1900, Sitzungsber. Preuss. Akad. Wiss. Phys.-Math. Kl., p. 929.
Rubens, H., and E. F. Nichols, 1897, Ann. Phys. (Leipz.) **60**, 418.
Rutherford, E., 1900, Philos. Mag. **49**, 1.
Rutherford, E., 1918, J. Röntgen Soc. **14**, 81.
Rutherford, E., 1920, Proc. R. Soc. A **97**, 374.
Rutherford, E., and H. Geiger, 1908, Proc. R. Soc. A **81**, 162.
Rutherford, E., and F. Soddy, 1902, Philos. Mag. **4**, 370, 569.
Salaman, E., 1979, Encounter, April, p. 19.
Schroedinger, E., 1924, Naturwissenschaften **36**, 720.
Schroedinger, E., 1926a, Ann. Phys. (Leipz.) **79**, 361.
Schroedinger, E., 1926b, Phys. Z. **27**, 95.
Schroedinger, E., 1926c, Ann. Phys. (Leipz.) **79**, 361.
Schroedinger, E., 1926d, Ann. Phys. (Leipz.) **79**, 734, footnote on p. 735.
Schroedinger, E., 1955, letter to E. Broda, reproduced in E. Broda, *Ludwig Boltzmann* (Deuticke, Vienna), p. 65.
Scott, W. T., 1967, *Erwin Schroedinger* (University of Massachusetts, Amherst, Mass.).
Seelig, C., 1954, *Albert Einstein* (Europa, Zürich).
Shankland, R. S., 1963, Am. J. Phys. **31**, 47.
Simon, F., 1956, Yearbook Phys. Soc. London, p. 1.
Slater, J. C., 1925, Phys. Rev. **25**, 395.
Slater, J. C., 1967, Int. J. Quantum Chem. **1s**, 1.
Sommerfeld, A., 1912, Verh. Ges. Dtsch. Naturforsch. Aerzte **83**, 31.
Sommerfeld, A., 1922, *Atombau und Spektrallinien* (Vieweg, Braunschweig), 3rd edition, p. 311.
Sommerfeld, A., 1924, *Atombau und Spektrallinien* (Vieweg, Braunschweig), 4th edition, p. VIII.
Sommerfeld, A., 1968, *Gesammelte Schriften* (Vieweg, Braunschweig), Vol. 3, p. 10.
Speziali, P., 1972, editor, *Albert Einstein Michele Besso Correspondance 1903–1955* (Hermann, Paris).
Stark, J., 1909, Phys. Z. **10**, 902. (Stark wrote n instead of ν.)
Stefan, J., 1879, Sitzungsber. Akad. Wiss. Wien, Math-Naturwiss. Kl. 2. Abt. **79**, 391.
Stuewer, R. H., 1975, *The Compton Effect* (Science History, New York).
Thirring, W., 1977, letter to A. Pais, November 29.
Thomson, J. J., 1897, Philos. Mag. **44**, 269.
Thomson, J. J., 1899, Philos. Mag. **48**, 547.
Thomson, J. J., 1936, *Recollections and Reflections* (Bell, London), p. 341.
Uhlenbeck, G. E., 1927, *Over statistische methoden in de theorie der quanta*, Ph.D. Thesis (Nyhoff, The Hague).
Uhlenbeck, G. E., 1976, Phys. Today **29**, June, p. 43.
Uhlenbeck, G. E., 1979, in *Proceedings of the Einstein Centennial Symposium*, Princeton, N.J., edited by H. Woolf (Addison-Wesley, Reading, Mass.).
von Schweidler, E., 1904, Jahrb. Radioakt. Elektron. **1**, 358.
Weber, H. F., 1872, Ann. Phys. (Leipz.) **147**, 311.
Weber, H. F., 1875, Ann. Phys. (Leipz.) **154**, 367, 553.
Weyl, H., 1930, *The theory of groups and quantum mechanics* (Dover, New York), 2nd edition, pp. 263–264 and preface; original edition published 1938 as *Gruppentheorie und Quantenmechanik*.
Wien, W., 1893, Sitzungsber. Dtsch. Akad. Wiss. Berl. Kl. Math. Phys. Tech., p. 55.
Wien, W., 1896, Ann. Phys. (Leipz.) **58**, 662.
Wigand, A., 1907, Ann. Phys. (Leipz.) **22**, 99.
Wigner, E. P., 1939, Ann. Math. **40**, 149.
Williams, B., 1977, editor, *Compton Scattering* (McGraw-Hill, New York).
Wolfke, M., 1921, Phys. Z. **22**, 315.
Wüllner, A., 1896, *Lehrbuch der Experimentalphysik* (Teubner, Leipzig), Vol. 2, p. 507.