5 Einstein and the development of quantum physics

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1

Towards the end of his life Albert Einstein wrote to his oldest friend that fifty long years of 'conscious brooding' over the question, 'What are light quanta?', had brought him no closer to its answer. As usual Einstein was not exaggerating: the problem of understanding discreteness as well as continuity in the natural world occupied him throughout his career. That Einstein spent so much time and energy wrestling with the quantum theory may well surprise many, and even many physicists. His creation of the special and general theories of relativity and his long series of attempts to construct a still more general theory, a unified field theory, have overshadowed his other achievements. Yet anyone who knows Einstein's work is likely to agree with Max Born, one of the major figures in the development of quantum mechanics, who wrote: 'In my opinion he would be one of the greatest theoretical physicists of all times even if he had not written a single line on relativity.' That opinion is based mainly on the papers in which Einstein reported the remarkable results of his 'conscious brooding' over the problems of quanta.

The great discoverers can readily be classed under two types of mentality: those who dig deep and those who range wide. Those who possess the gift of combining depth with breadth are rare indeed. Albert Einstein was one of them'.

(François le Lionnais, 'From Plurality to Unity', in *Science and Synthesis*)

Einstein was the one who, in 1905, first proposed the idea of light quanta. It was simply heretical at that time to suggest that light sometimes behaved as though it consisted of localized particles of energy, and years went by before this suggestion won any acceptance. As Einstein probed further, and worked out the consequences of Max Planck's radiation law, he saw that a new theory of light was needed, one in which the dual nature of light—wave and particle—would be accounted for. By 1908 he was already convinced that these problems were 'so incredibly important and difficult' that every physicist should devote his efforts to trying to solve them. Einstein was also the first to realize that a quantum theory of matter was needed, as well as a new theory of radiation. His early effort in this direction—a quantum theory of the specific heats of solids—led to new and unexpected connections among the thermal, optical, and elastic properties of solids, helping to convince other physicists that the quantum theory must be taken seriously. Einstein's papers in this field over a period of twenty years influenced and inspired Niels Bohr, Louis de Broglie, and Erwin Schrödinger, among others, in their own contributions to the great synthesis that created a quantum physics in the 1920s.

In this article I will sketch Einstein's role in this development, describing the works just mentioned, emphasizing the questions Einstein was trying to answer, and the deep concern with the foundations of physics underlying all his efforts. But the story does not end there. When the new quantum physics was developed, Einstein greeted it sceptically even though he had done as much as anyone to bring it into being. He recognized its great successes, but he never accepted it as the new fundamental theory it claimed to be. Einstein wrote relatively little on this subject during the second half of his career, concentrating on his search for a unified field theory. His critical comments during this period cannot, however, be ignored; they were important to his opponents, especially to Bohr, in helping to clarify just what the new quantum physics did mean. They are also important in understanding Einstein's own goals as a physicist for, as Born remarked, 'Einstein's conception of the physical world cannot be divided into watertight compartments'.

'I have greatly admired the papers published by Mr Einstein on questions dealing with modern theoretical physics. Moreover, I believe that the mathematical physicists all agree that these works are of the highest order. . . . If one considers that Mr Einstein is still very young, one has every right to justify the greatest expectations from him, and to see in him one of the leading theoreticians of the future. . . .'
(Marie Curie)

II

In June 1905, *Annalen der Physik* published an article by Einstein entitled 'On a Heuristic Viewpoint Concerning the Production and Transformation of Light'. Physicists usually refer to this as 'Einstein's paper on the photoelectric effect', but that description does not do it justice. Einstein himself characterized it at the time as 'very revolutionary', and he was right. This is the paper in which he proposed that light can, and in some situations must, be treated as a collection of independent particles of energy—light quanta—that behave like the particles of a gas. Einstein was well aware that a great weight of evidence had been amassed in the course of the previous century showing light to be a wave phenomenon. He knew, in particular, that Heinrich Hertz's experiments, carried out less than twenty years earlier, had confirmed Maxwell's theoretical conclusion that light waves were electromagnetic in character. Despite all this evidence Einstein argued that the wave theory of light had its limits, and that many phenomena involving the emission and absorption of light 'seemed to be more intelligible' if his idea of quanta were adopted. The photoelectric effect was one of several such phenomena which he analysed to show the power of his new hypothesis. But even granting the success of that hypothesis, what prompted Einstein to make this extraordinary suggestion?

Einstein devoted the greater part of his paper to answering just this question, presenting the arguments that led him to his new 'heuristic viewpoint' of quanta. These arguments, at once simple and daring, embody some of the essential features of his whole approach to physics. His deepest concern, expressed in the opening sentences of his paper, was the very foundation of his science. Let us look briefly at the background for this concern.

When Einstein was a student at the Polytechnic in Zürich just before 1900, working eagerly in the laboratory but skipping many of the lectures to read the works of the great physicists on his own, he absorbed the spirit that had guided the development of physics through three centuries. I refer to 'the mechanical world view', the conviction that all natural phenomena are to be explained in terms of a single underlying theory—mechanics. The successes of this approach were evident to the young Einstein. 'What made the greatest impression upon the student,' he wrote many years later, 'was . . . the achievements of mechanics in areas which apparently had nothing to do with mechanics: the mechanical theory of light . . . and above all the kinetic theory of gases. . . . These results supported at the same time mechanics as the foundation of physics and of the atomic hypothesis. . . . It was also of profound interest that the statistical theory of classical mechanics was able to deduce the basic laws of thermodynamics, something which was in essence already accomplished by Boltzmann.' The vision of a single fundamental theory as the basis for all the diverse aspects of the world captured Einstein's imagination, as it had captured the imagination of theorists long before him.

. . . he never hesitated to change his opinion when he found that he had made a mistake and to say so. Indeed, there was an occasion when somebody accused him of saying something different from what he had said a few weeks previously, and Einstein replied, 'Of what concern is it to the dear Lord what I said three weeks ago?' It was just a way of saying that it did not matter. It was wrong, and now he knew better.
(Otto Frisch, in G. J. Whitrow, *Einstein: The Man and His Achievement*)

By 1900, however, it was no longer possible to accept the goal of explaining all phenomena in mechanical terms, and Einstein recognized this too in his early years. He read Ernst Mach, whose criticism of the mechanical programme, carried out with 'incorruptible scepticism and independence', shook Einstein's 'dogmatic faith'. He also studied Maxwell's theory of electromagnetism, finding it 'the most fascinating subject at the time that I was a student'. This theory made a shift in basic concepts, a shift that Einstein called nothing less than 'revolutionary', from the idea of forces acting at a distance to that of fields acting locally. Although Maxwell and his immediate successors thought of the electromagnetic field as acting through a mechanical medium whose structure could eventually be determined, all attempts to determine that structure proved fruitless. Electromagnetism was not successfully explained in mechanical terms and, as Einstein put it: 'One got used to operating with these fields as independent substances without finding it necessary to give one's self an account of their mechanical nature; thus mechanics as the basis of physics was being abandoned, almost unnoticeably, because its adaptability to the facts presented itself finally as hopeless.'

Einstein was very conscious of this disturbing dualism in the foundations of physics, with two kinds of basic theories of quite different character—mechanics, and the electromagnetic field theory. It was this dichotomy he pointed to at the beginning of his 1905 paper, 'On a Heuristic Viewpoint': 'There is a profound formal difference between the theoretical ideas which physicists have formed concerning gases and other ponderable bodies and the Maxwell theory of electromagnetic processes in so-called empty space.' He referred to the contrast between the discrete mechanics of matter which is atomic in structure and in which a finite number of mechanical quantities specify the state of a system, and the continuous field theory of electromagnetism in which a set of continuous functions are needed to specify the state of the field. This dualism between particle and field, between mechanics and electromagnetism, was the starting point of his considerations. It was a disturbing dualism because it could lead to serious problems when the two disparate fundamental theories had to be brought to bear together. Einstein immediately gave an example of one of these problems, so serious that his friend Paul Ehrenfest later gave it the dramatic name, 'the ultraviolet catastrophe'. Einstein's example involved the blackbody radiation recently studied in detail by Max Planck using quite another approach. Let us examine Einstein's treatment of this situation.

During one of the lectures, Paul Ehrenfest passed on a note to Einstein, saying 'Don't laugh! There is a special section in purgatory for professors of quantum theory, where they will be obliged to listen to lectures on classical physics for ten hours every day.' To which Einstein replied, 'I laugh only at their naiveté. Who knows who would have the laugh in a few years?'
(J. Mehra, *The Solvay Conferences on Physics*)

He considered a volume, enclosed by reflecting walls, that contained a gas and also a number of harmonically bound electrons. These electrons, acting as charged harmonic oscillators, would emit and absorb electromagnetic radiation and, when the system came to thermodynamic equilibrium, this would be identical with the blackbody radiation. The oscillating electrons would also exchange energy with the freely moving molecules of the gas through collisions. These oscillating electrons served, in effect, as the link between the material system—the gas, described by mechanics—and the electromagnetic system—the radiation, described by Maxwell's theory. Both theories could be used to determine the average energy u of an oscillator of frequency ν when the system is in equilibrium at absolute temperature T. The statistical mechanics of the gas required an oscillator in equilibrium with the gas molecules to have an average energy proportional to T,

$$u = kT \tag{1}$$

where k is a universal constant, the gas constant per molecule (or Boltzmann's constant as it is now called). The electromagnetic theory required the average energy of the oscillator to be proportional to the energy density of the surrounding radiation, if absorption and emission were to be equal on the average. If $\rho(\nu, T)d\nu$ is the energy of the radiation, per unit volume, having frequencies in the interval ν to $\nu + d\nu$, then the average energy u of the oscillator must be given by

$$u = (c^3/8\pi\nu^2)\rho(\nu, T) \tag{2}$$

where c is the velocity of electromagnetic waves, or light.

Since equations (1) and (2) give two alternative expressions for the same quantity u, these expressions can be equated, giving the result

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

This equation ought to have fixed the distribution of energy in the spectrum of blackbody radiation by determining the function $\rho(\nu, T)$. The result obtained, however, was not only in conflict with experiment, but it was intrinsically unacceptable. For if one tried to calculate the total energy of the radiation in a unit volume by integrating $\rho(\nu, T)$ over all frequencies, the result obtained from equation (3) was proportional to ${}_0\int^\infty \nu^2 d\nu$ which is infinite. The result of combining the mechanical and electromagnetic equations was really no result at all. Einstein saw this as a clear sign that physics could not rest on its present divided foundations, and that in some way or other the foundations must be unified.

Since he saw no way of accomplishing that step at the time, what could be done? Einstein proceeded to analyse the implications of the radiation spectrum $\rho(\nu, T)$ as it was then known. As long as the frequency of the radiation considered was not too low (or the temperature too high), the spectrum could be described by the distribution law suggested by Wilhelm Wien in 1896:

$$\rho(\nu, T) = \alpha\nu^3 \exp[-\beta\nu/T] \tag{4}$$

where α and β are constants. To see the consequences of this distribution, Einstein treated the radiation as a thermodynamic system at equilibrium, a system having definite values of entropy as well as energy. He showed that if one considers the radiation of frequency ν, and keeps the energy E of this radiation fixed while slowly changing the volume of the container from V_0 to V, the entropy of this radiation changes from S_0 to S according to the equation

$$S - S_0 = (E/\beta\nu)\log(V/V_0) \tag{5}$$

This result was strikingly similar to the entropy change of an ideal gas of N particles whose volume is changed from V_0 to V at constant energy (or temperature),

$$(S - S_0)_{\text{gas}} = Nk\log(V/V_0) \tag{6}$$

where k is the same universal constant that appeared in equation (1). Was this a mere coincidence, or did it suggest something essential about the nature of radiation? The answer to that question depended on the significance of that logarithmic form for entropy. To explore this, Einstein turned to Ludwig Boltzmann's statistical interpretation of the entropy, according to which the entropy difference $S - S_0$ between two states of a macroscopic system is proportional to the relative probability W of the occurrence of those two states

$$S - S_0 = k\log W \tag{7}$$

A physical theory, in Einstein's conception, springs from the free creative activity of a man who sets up axioms to start with and need only justify them by their results, which are sometimes rather distant, and by a conviction of internal coherence when the proposed theory unites very wide areas of physics.

(André Lichnerowicz, 'From Plurality to Unity', in *Science and Synthesis*) with the same constant k appearing. Now, regardless of the laws of motion that describe the motions of the gas particles and regardless of the nature of these particles, so long as they move independently of one another and show no preference for one part of the available volume compared to another, the probability of finding the N particles in a subvolume V of the total volume V_0 is clearly

$$W=(V/V_0)^N \tag{8}$$

In other words, the logarithmic dependence of the entropy of a gas on its volume comes only from the independence of the gas particles.

Einstein's next step was to turn the argument around and apply it to the radiation: since the entropy of the radiation has exactly the same form as that of the gas, one can legitimately infer that the probability of finding all the radiation (of frequency ν) in the subvolume V must be given by the equation

$$W_{\text{rad}}=(V/V_0)^{N'} \tag{9}$$

where the exponent N' is obtained by comparing equations (5) and (6),

$$N'=(E/k\beta\nu) \tag{10}$$

Einstein drew what was, for him, the inescapable conclusion:

> Monochromatic radiation of low density (within the region of validity of the Wien distribution law) behaves with respect to thermal phenomena as if it consisted of independent energy quanta of magnitude $k\beta\nu$.

'All these fifty years of conscious brooding have brought me no nearer to the answer to the question "What are light quanta?" Nowadays every Tom, Dick, and Harry thinks he knows it, but he is mistaken.'

(A.E. to Besso, 12 December 1951)

This was the chain of reasoning that led Einstein to suggest treating radiation as if it were composed of a collection of independent particles of energy. He took the suggestion very seriously himself, applying it immediately to several phenomena, one of which was the photoelectric effect. The experimental material on the emission of electrons from a metal surface when the surface is irradiated by ultraviolet light was very limited in 1905, but it was known that the energies of the electrons emitted were independent of the intensity of the incident light. This was quite unintelligible if the light were considered to be a wave, since the intensity of a wave is always a measure of the energy carried by it. If one accepted Einstein's proposal, however, the process of photoelectric emission could be thought of as a combination of independent events, the simplest of which is the absorption of one quantum of energy by an electron in the metal surface, and its conversion into the kinetic energy of the electron which is thereby set free. The maximum energy of such a photoelectron would then be determined by the energy of one light quantum, which is $k\beta\nu$ on Einstein's hypothesis. The maximum kinetic energy of the electron could not be equal to $k\beta\nu$ because it would take a certain amount of work, P, to remove the electron from the metal in which it was bound, and so the equation for the maximum energy of the photoelectrons, $(\text{K.E.})_{\text{max}}$, would be

$$(\text{K.E.})_{\max} = k\beta\nu - P \tag{11}$$

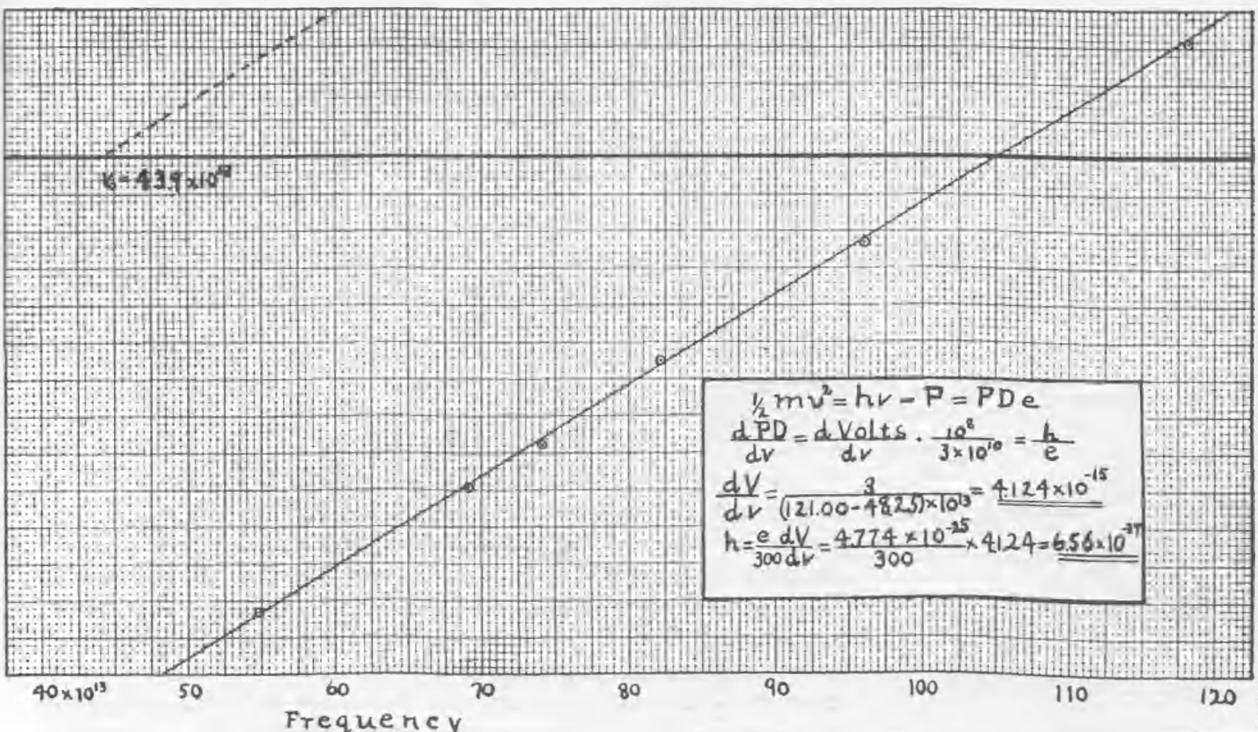
This argument immediately explains the independence of the electron energy from the intensity of the incident light, since increasing that intensity increases the number of incident quanta without affecting the energy $k\beta\nu$ of the individual light quantum. The energy of the emitted photoelectron would be less than the maximum predicted by equation (11) if the energy of a quantum were shared among several electrons, or if the electron emerged from the interior of the metal rather than its surface. The maximum energy can be measured by determining the electric potential Y_{stop} needed just to prevent any photoelectrons from reaching the collecting electrode. If e is the charge on an electron, equation (11) can then be rewritten in the form

$$Y_{\text{stop}} = (k\beta/e)\nu - (P/e) \tag{12}$$

The stopping potential should be a straight line when plotted against the frequency of the incident (monochromatic) light. The slope of that line $(k\beta/e)$ should be the same for all emitting surfaces, and this universal slope depends only on universal constants determinable from experiments on completely different phenomena. Only the work P is characteristic of the particular metal surface used in the experiment.

Figure 22

Millikan's verification of Einstein's photoelectric equation



These predictions made by Einstein on the basis of his light quantum hypothesis were almost as remarkable as the hypothesis itself, since practically nothing was known in 1905 about the frequency dependence of the stopping potential for photoelectrons. It took a decade of difficult experimentation before Einstein's equation (12) was fully confirmed, especially by the work of Robert A. Millikan. Even in 1916, although Millikan announced that Einstein had predicted 'exactly the observed results', he considered Einstein's idea of light quanta to be a 'bold, not to say reckless, hypothesis', which had 'now been pretty generally abandoned'.

III

The year 1905 was Einstein's *annus mirabilis*. Because of his contributions, volume 17 of the *Annalen der Physik* of that year is now regarded as one of the most remarkable volumes of scientific literature ever published.
(G. J. Whitrow, *Einstein: The Man and His Achievement*)

When Einstein proposed the usefulness, the 'heuristic' value, of light quanta on the basis of the arguments that have just been described, he had already read Max Planck's papers on the theory of blackbody radiation. Planck had been working on this problem since 1897 and in 1900 he announced a new form for the radiation distribution, one which generalized Wien's law (equation (4) above) and claimed validity for all frequencies and temperatures:

$$\rho(\nu, T) = \left(\frac{8\pi\nu^2}{c^3}\right)\frac{h\nu}{\exp(h\nu/kT) - 1} \tag{13}$$

The constant h (Planck's constant), which appears in this radiation law is related to the constants introduced earlier by the equation

$$h = \beta k \tag{14}$$

as one can easily see by looking at the limiting form of equation (13) for large values of $(h\nu/kT)$, when it reduces to the Wien form. At the other extreme, small values of $(h\nu/kT)$—low frequencies or high temperatures—Planck's result agrees with the inadequate result of mechanics and electromagnetic theory (equation (3) above) as Einstein pointed out in his 1905 paper.

Planck's derivation of his distribution law was not easily disentangled, however, and Einstein saw no direct connection between his own work and Planck's at that time. I say 'no direct connection' because Einstein had read Planck's work and thought about it; it had stimulated him to find his own way of dealing with radiation, a way quite different from Planck's. It was only in 1906 that Einstein realized that Planck, too, had introduced a new discreteness into physics. In Planck's case it was not the energy of radiation that was to be thought of as localized in particles or quanta, but rather the energy of those charged harmonic oscillators, the vibrating electrons that emitted and absorbed the radiation, that could only take on certain discrete values rather than varying continuously. Planck had not been very clear about this point; he introduced the discreteness as a device to make calculation possible, and did not insist on any physical significance of his 'elements of energy', as he called them, at the time he introduced them into physics in 1900.

Late in 1906, after Einstein had studied Planck's book on the theory of radiation and pursued his own ideas more deeply, he was ready to set forth some more startling consequences of his thinking. Planck's way of treating the charged oscillators in his theory was equivalent to saying that an oscillator of frequency ν could assume only the energies 0, $h\nu$, $2h\nu$, . . ., $nh\nu$, . . . and no others. The average energy u of such an oscillator in equilibrium at temperature T would no longer be given by equation (1), but instead by the equation

$$u=\frac{h\nu}{\exp(h\nu/kT)-1} \qquad (15)$$

which reduces to the earlier result when $(h\nu/kT)$ is very small. This meant a modification in the kinetic molecular theory of heat, or statistical mechanics as we would now call it, a modification with major implications, as Einstein pointed out:

> While up to now molecular motions have been supposed to be subject to the same laws that hold for the motions of the bodies we perceive directly . . ., we must now assume that, for ions which can vibrate at a definite frequency and which make possible the exchange of energy between radiation and matter, the manifold of possible states must be narrower than it is for the bodies in our direct experience.

But this was not all, for Einstein went on to write:

> I now believe that we should not be satisfied with this result. For the following question forces itself upon us: If the elementary oscillators that are used in the theory of the energy exchange between radiation and matter cannot be interpreted in the sense of the present kinetic molecular theory, must we not also modify the theory for the other oscillators that are used in the molecular theory of heat? There is no doubt about the answer in my opinion. If Planck's theory of radiation strikes to the heart of the matter, then we must also expect to find contradictions between the present kinetic molecular theory and experiment in other areas of the theory of heat, contradictions that can be resolved in a similar fashion.

Einstein saw that what Planck had found was only the beginning, and that this unexpected discreteness of the energy would have to prevail in a variety of other situations. In other words, Einstein saw the need for a quantum theory which, when it was achieved, would clarify the properties of matter as well as those of radiation. He could not construct such a theory in general, but he could and did point to one of those 'contradictions between the present kinetic molecular theory and experiment' that already existed, and show how it could be resolved with the help of the new discreteness in energy. The contradiction concerned the specific heats of solids.

The essence of Einstein's profundity lay in his simplicity; and the essence of his science lay in his artistry—his phenomenal sense of beauty. 'This was sometime a paradox, but now the time gives it proof,' as Hamlet said in a different connection.

(Banesh Hoffmann: *Albert Einstein: Creator and Rebel*)

The calorimetric measurements made by Dulong and Petit early in the nineteenth century had shown that the heat capacities of the elements in the solid state had a common value, if each of these heat capacities were taken for a gram atomic weight (or mole) of the substance in question. This Dulong–Petit rule had provided a useful method for estimating atomic weights, and it found a simple explanation in the kinetic molecular theory. If the thermal motions of the atoms in a solid were taken to be simple harmonic vibrations about positions of equilibrium, there would be three independent motions per atom, or $3N_0$ oscillations for one mole of the substance. (N_0 is Avogadro's number, the number of atoms in a gram atomic weight.) Each vibration in a solid at temperature T would have an average energy of kT, as required by equation (1), and so the total thermal energy of one mole of the solid would have to be $3N_0kT$, or $3RT$, where R is the usual gas constant per mole. The rate of change of this thermal energy with temperature is the specific heat per mole, and it has the value $3R$, or approximately 6 calories per degree, the Dulong–Petit value. So far there is no contradiction. But this explanation of the Dulong–Petit rule proved too much, since the rule is only a rule and a number of elements were known to have specific heats much smaller than the Dulong–Petit value. These exceptions occur particularly among the lightest elements such as boron and carbon, and it was also known before 1900 that their specific heats vary rapidly with temperature, approaching the Dulong–Petit value well above room temperature.

During the first of my talks with Einstein an amusing incident occurred. I was very nervous and still very shy and after we had been talking for about twenty minutes the maid came in with a huge bowl of soup. I wondered what was happening and I thought that this was probably a signal for me to leave. But when the girl left the room Einstein said to me in a conspiratorial whisper: 'That's a trick. If I am bored talking to somebody, when the maid comes in I don't push the bowl of soup away and the girl takes whomever I am with away and I am free.
(L. L. Whyte, in G. J. Whitrow, *Einstein: The Man and His Achievement*)

There was also another problem, perhaps even more disturbing than these exceptions to the Dulong–Petit rule. By 1906 it was clear that atoms had an internal structure and that they 'contained', in some way, electrons. The frequencies at which ultraviolet light was absorbed in solids had been associated with electronic motions, just as the infrared absorption frequencies were associated with ionic vibrations. Why did these electronic motions contribute nothing at all to the specific heat of the solid, instead of the amount k per vibration that the classical theory seemed to require?

Einstein resolved all these difficulties with one stroke. For if he was right in thinking that all oscillations on the atomic scale had to have quantized energies ('if Planck's theory strikes to the heart of the matter'), then each oscillator has an average energy given by equation (15) instead of the classical value kT. The electronic oscillations at ultraviolet frequencies can be seen at once to make negligible contributions at any reasonable temperature, since at such high frequencies $(h\nu/kT)$ is a large number and the average energy given by equation (15) is, in effect, practically zero, as is its temperature derivative. As for the atomic vibrations, Einstein made the simplest possible assumption (recognizing explicitly that he might be oversimplifying): he took all these vibrations to be independent and of the same frequency ν. The energy U of one mole of the solid would then be given by the equation

$$U = \frac{3N_0 h\nu}{\exp(h\nu/kT) - 1}. \qquad (16)$$

The specific heat is obtained by differentiating U with respect to temperature. If this specific heat is plotted as a function of temperature, or rather of $(kT/h\nu)$, one obtains a curve that rises smoothly and monotonically from zero at the origin and approaches the value $3R$, the Dulong–Petit value, asymptotically when $(kT/h\nu)$ becomes large. Roughly speaking, the specific heat is negligibly small when $(kT/h\nu)$ is less than about 0.1, and it has about the full value of $3R$ when $(kT/h\nu)$ is appreciably greater than one. Since light atoms would be expected to have higher vibration frequencies than heavier ones, other things being equal, this result already explained qualitatively why the light elements have anomalously low specific heats at room temperature.

This theory of specific heats suggested an important and previously unsuspected connection between the optical and thermal properties of solids. Einstein identified the vibration frequency of the atoms with the frequency of optical absorption, at least for those crystals in which such absorption occurred. The data available to him were consistent with this relationship and in several cases he was able to make reasonably accurate predictions of the absorption frequency from the measured specific heat and his equation for its temperature dependence.

Even more important than this relationship between optical and thermal properties was the general theorem implied by Einstein's theory: the specific heats of all solids must become vanishingly small at sufficiently low temperatures. The solids that had been labelled as exceptions because they did not obey the Dulong–Petit rule were not to be thought of as exceptional at all; they merely exhibited the universal decrease of specific heats with decreasing temperature at relatively high temperatures, because of their light atoms and correspondingly high vibrational frequencies. Carbon in the form of the diamond crystal, for example, did not acquire the full Dulong–Petit value of its specific heat unless it was heated above 1000 °C, and its specific heat was only about a tenth of that value when it was cooled to only −50 °C. Einstein used the data on diamond, whose specific heat had been measured as a function of temperature, for a test of his theoretical equation. He could not, however, test it on other materials, particularly those that did obey the Dulong–Petit rule at room temperature, because no data for the behaviour of specific heats at low temperatures were available.

Such experiments were made a few years later by Walther Nernst and his collaborators in Berlin, not in order to test Einstein's theory of specific heats but to confirm Nernst's own ideas on the thermodynamic properties of matter near the absolute zero of temperature. Nernst found in 1910 and 1911 that all the many specific heats he measured did fall off at low enough temperatures, and learned that this had been predicted by Einstein's quantum theory of specific heats. Nernst was properly impressed by this

It has been said that common sense is the prerogative of the good, and the bad are destroyed by their lack of it. We may wonder if something similar does not apply to truth—that truth is the prerogative of the simple, and only those who are in a certain sense without guile are able to recognize it. In the case of someone like Einstein we cannot but feel that there is indeed an inner and necessary connection between the extraordinary theoretical simplicity of his work and the personal simplicity of the man himself. We feel that only someone himself so simple could have conceived such ideas.

(Henry Le Roy Finch, in *Conversations with Einstein*) and became a staunch advocate of the importance of the new quantum theory of Planck and Einstein, even if he did refer to it as 'a very odd rule (for calculation), one might even say a grotesque one'.

The whole subject was discussed at the first of the famous Solvay Conferences on Physics, initiated and funded by the Belgian industrial chemist, Ernest Solvay. This first conference, held in 1911, had as its topic 'Radiation Theory and Quanta'. Lorentz, Planck, Nernst, and Einstein were among those who presented papers; the title of Einstein's paper was 'The Present State of the Problem of Specific Heats'.

IV

Einstein later summed up his feelings about the state of physics during this period in these words: 'It was as if the ground had been pulled out from under one's feet, with no firm foundation on which to build to be seen anywhere'. He devoted much of his effort to a continued probing of the consequences of Planck's distribution law for blackbody radiation, searching for what it implied about the structure of radiation and about the status of the electromagnetic field theory. In 1909 he reported some results of this probing at the annual meeting of German scientists, held that year at Salzburg. It was his first address to a major scientific gathering, and the first occasion for him to meet many of the physicists whose works he had studied.

The history of physics offers many classic cases where the non-scientific attitude of 'disciples' is quite unmistakable, and the study of such cases might very well give the physicist a 'feel' for recognizing similar patterns occurring in our days. It was reflecting upon one such case—the difference of attitude between Newton and his successors—that made Einstein remark: 'Newton himself was better aware of the weaknesses inherent in his intellectual edifice than the generation of learned scientists which followed him. This fact has always aroused my deep admiration.'

(Stanley L. Jaki, *The Relevance of Physics*)

In his address Einstein emphasized how much Planck had departed from classical ideas on radiation in his theory of the distribution law for black-body radiation. Planck's answer, the law expressed in equation (13), was well confirmed by experiments over the whole accessible spectrum, but one might still have some doubts. 'Would it not be conceivable,' Einstein asked, 'that Planck's radiation formula was indeed correct, but that it could be derived by some method that was not based on such an apparently monstrous assumption as Planck had used? Would it not be possible to replace the hypothesis of light quanta by some other hypothesis by means of which one could do equal justice to the familiar phenomena? If it is necessary to modify the principles of the theory could one not at least retain the equations for the propagation of radiation and interpret only the elementary events of emission and absorption in a way different from that used previously?'

To all these questions Einstein's answer was 'No'. It was not possible to have Planck's satisfactory distribution law without the new and disturbing discreteness in nature. Einstein justified this assertion by extending his earlier application of Boltzmann's relation between entropy and probability (equation (7)). Given that the radiation was a thermodynamic system whose equilibrium state was described by Planck's law, one could calculate the fluctuations in its energy. If one considers that part of the black-body radiation in a volume V, whose frequencies lie in a small interval between ν and $\nu + \mathrm{d}\nu$, the mean square fluctuation in its energy $(\Delta E)^2$ is found to have the form

$$(\Delta E)^2 = V d\nu[h\nu\rho + (c^2/8\pi\nu^2)\rho^2], \qquad (17)$$

where ρ is given by Planck's law (equation (13)). Einstein was able to identify these two terms individually. The first is just the fluctuation to be expected in a collection of independent energy quanta, each of which has energy $h\nu$. The second is the fluctuation that would result from interfering waves.

Einstein commented that it was as though there were two independent causes producing the fluctuations, with their separate contributions being simply additive. In the high frequency, low temperature region, where Planck's law goes over to Wien's, the first or particle term predominates. In the low frequency, high temperature region, where the classical distribution is found, the second or wave term predominates. Einstein concluded that the particle-like behaviour in the high frequency region is a necessary consequence of Planck's distribution law. One cannot hope to avoid it by a new derivation of the distribution from alternative assumptions; the particle-like behaviour follows from the law itself. While Planck had introduced quantization as a *sufficient* condition for deriving his distribution, Einstein argued that it was a *necessary* implication of that distribution.

The fluctuation result with its two independent terms, which Einstein confirmed by other arguments of quite another sort, suggested something further. Einstein's earlier heuristic proposal of light quanta never purported to be more than that; he had never claimed that he was offering it as a new theory to replace Maxwell's theory of the electromagnetic field. But now there was at least a hint as to the proper direction in which progress might be made, since the wave and particle aspects of radiation appeared together in a single equation. 'It is my opinion,' Einstein announced, 'that the next phase of the development of theoretical physics will bring us a theory of light that can be interpreted as a kind of fusion of the wave and emission [particle] theories.' The problem was to take the next step since, as he remarked, 'the fluctuation properties . . . present small foothold for setting up a theory'. After all, if one had known nothing of interference or diffraction phenomena and had had only the second (wave) term in the fluctuations to go on, 'Who would have enough imagination to construct the wave theory of light on this foundation?'

Difficult as the task was, Einstein certainly tried. During the years from 1908 to 1911 he wrestled with the problem, trying to construct some sort of nonlinear equation that would allow him to introduce both the radiation constant h and the electronic charge e into the theory. He expected the discreteness of charge and the discreteness of energy to enter the theory together since the combination (e^2/hc) is dimensionless. Although he published nothing but a few passing remarks on his work we know from his correspondence during those years how intensively he worked on the radiation problem. This is especially true of his correspondence with H. A.

. . . in spite of so many touches which show his friendliness there is every sign that he was extraordinarily self-sufficient. Only a man as self-sufficient as he, could have worked out his first epoch-making discoveries in obscurity. But despite his friendships he was essentially a lonely figure. It was perhaps a penalty he had to pay for an endowment of genius of a magnitude which appears but rarely in the whole of recorded history.

(Christopher Sykes, in G. J. Whitrow, *Einstein: The Man and His Achievement*)

Photo Couprie, Bruxelles

GOLDSCHMIDT PLANCK RUBENS LINDEMANN HASENOHRL
NERNST BRILLOUIN SOMMERFELD M DE BROGLIE HOSTELET
SOLVAY KNUDSEN HERZEN JEANS RUTHERFORD
LORENTZ WARBURG WIEN EINSTEIN LANGEVIN
PERRIN Madame CURIE POINCARÉ KAMERLINGH ONNES

Figure 23 The First Solvay Conference on Physics, Brussels, 1911

Lorentz, whose electron theory was then much in Einstein's mind. In May 1911 Einstein wrote to his closest friend, Michele Besso, that he was no longer trying to construct quanta, 'because I now know that my brain is incapable of accomplishing such a thing'. It was at about this time that Einstein turned his full attention to the problem of gravitation, with historic consequences.

V

By the time Einstein took up the problem of radiation again in 1916, there had been major changes in the quantum theory. Niels Bohr's papers had shown that quantum concepts offered the possibility of understanding the structure of the atoms and the characteristics of the spectra they emit. Although Einstein did not work on these problems he was clearly influenced by Bohr's ideas, as Bohr had been by his. Einstein's new work was, in the first instance, a fresh derivation of the Planck distribution law. Einstein referred to it in print as 'astonishingly simple and general', and in a letter to Besso as perhaps '*the* derivation' of this important law. This new derivation avoided an inconsistency that marred Planck's own treatment, namely, the use of the electrodynamic result expressed in equation (2) in a situation where the assumptions underlying this equation were violated. Einstein had been aware of this difficulty since 1906, and now he had found a way of avoiding it.

The new derivation was based on statistical assumptions about the processes of emission and absorption of radiation, assumptions chosen so as to follow the pattern of the classical theory without adopting it in detail. It also employed the basic assumption of Bohr's theory, that atomic systems have a discrete set of possible stationary states. The proof then used the condition that the absorption and emission of radiation suffice to keep a gas of atoms in thermodynamic equilibrium. (This paper introduced the concept of stimulated emission into quantum physics and so is often referred to as having provided the basis for the laser.)

Einstein's new approach to the radiation problem also included arguments for the directional character of the radiation emitted by an atom. He showed that in each individual emission process in which a quantum of frequency ν is emitted, that quantum must carry away momentum $h\nu/c$ in a definite direction; spherical waves were ruled out. Einstein considered his theoretical proof that all radiation must be sharply directional to be the most significant aspect of this paper. There was no real experimental support for this result at the time, but it came a few years later in the form of the Compton effect, the increase in wavelength of X-rays scattered by effectively free electrons. In 1923 Arthur Compton and Peter Debye showed independently that the Compton effect could be explained if the scattering were treated as a collision, obeying the conservation laws, between a free electron at rest and a light quantum of energy $h\nu$ and momentum $h\nu/c$ in the direction of the incident beam. This successful treatment of the Compton effect made the light quantum acceptable to many physicists who had previously refused to take it very seriously.

During the 1920s the problems of applying the quantum theory to atomic structure and atomic spectra were at the centre of interest in physics. Einstein took no part in this development which was occupying so many of his colleagues, from Niels Bohr, Arnold Sommerfeld, and Max Born to their younger colleagues such as H. A. Kramers, Werner Heisenberg, and Wolfgang Pauli. Although his major concern in those years was the generalization of the theory of relativity, Einstein continued to think about the problems of quanta.

Einstein never liked his photon as tenderly as his beloved relativity. The photon was a natural child, a bastard born out of wedlock; Einstein remained a strong believer in differential equations in a continuous medium. Discontinuities and quanta seemed to him unnatural.

(Léon Brillouin, *Relativity Reexamined*)

In 1924 a new occasion for doing so arose when he received a paper in English from a young Indian physicist, S. N. Bose, setting forth a theory in which radiation was treated as a gas of light quanta. This approach had been tried before, but if the gas of quanta were treated by the usual statistical methods one ended up with Wien's distribution law rather than Planck's. By changing the statistical procedure for counting the states of the gas Bose had been able to obtain the proper Planck distribution. Einstein was much taken with this paper. He translated it into German and saw that it was published, and then applied Bose's idea to a gas of material particles. This Bose–Einstein gas, as it came to be called, showed a variety of novel and interesting properties.

While he was working out the behaviour of this gas Einstein received a copy of a doctoral dissertation written in Paris. The author, Louis de Broglie, inspired by Einstein's earlier studies of the wave-particle duality for radiation, had become convinced that this duality must hold for matter as well. His thesis developed the idea that every material particle has a wave associated with it, the frequency ν and wavelength λ of the wave being related to energy E and momentum p of the particle through the equations

$$E = h\nu \qquad p = h/\lambda \tag{18}$$

Since de Broglie had no experimental evidence for his matter waves, his work did not impress most physicists. Einstein, however, was quite taken with it, and realized that de Broglie had 'lifted a corner of the great veil'. He found that de Broglie's ideas fitted in very well with his current work on the new theory of the gas. Both were concerned with the parallels between the gas of quanta and the gas of material particles. The fluctuations in density of the Bose–Einstein gas, which Einstein calculated early in 1925, showed exactly the same two-term structure as the fluctuations in blackbody radiation. Einstein saw this as important evidence supporting de Broglie's matter waves, and went on to suggest a number of experimental possibilities for detecting the de Broglie waves.

VI

In that same year, 1925, Heisenberg proposed a new approach to the

Figure 24

Einstein and Niels Bohr deep in thought (taken by Ehrenfest in about 1927)

quantum theory, an approach quickly developed by him in collaboration with Born and Pascual Jordan into a quantum mechanics based on matrix algebra. Einstein was interested and impressed, but he was not convinced. 'The most interesting theoretical work produced recently is the Heisenberg–Born–Jordan theory of quantum states,' he wrote to Besso. 'It's a real witches' calculus, with infinite determinants (matrices) taking the place of Cartesian coordinates. Most ingenious, and adequately protected by its great complexity against being proved wrong.' The following year he expressed his negative opinion to Born: 'An inner voice tells me that it is still not the true Jacob,' a judgement that Born took as 'a hard blow'.

When Erwin Schrödinger introduced an alternative to the algebraic quantum mechanics with his wave equation, Einstein reacted much more favourably. 'I am convinced that you have made a decisive advance with your formulation of the quantum condition,' he wrote to Schrödinger, 'just as I am equally convinced that the Heisenberg–Born route is off the track.' This reaction of Einstein's is not too surprising since Schrödinger's work followed the direction pointed by de Broglie, and he had been much influenced by what he referred to as Einstein's 'short but infinitely far-seeing remarks' on the implications of de Broglie's thesis.

As it turned out, the two methods that seemed so different were mathematically equivalent, and both became part of the synthesis that constituted the new quantum mechanics. One of the key features of this synthesis was Born's statistical interpretation of Schrödinger's wave function. This meant that the new theory was intrinsically statistical and renounced as meaningless any attempt to go beyond the probabilities to obtain a deterministic theory. Bohr expressed the generally accepted opinion when he described quantum mechanics as a 'rational generalization of classical physics', a generalization that resulted from the 'singularly fruitful cooperation of a whole generation of physicists'.

There was one great dissenter from this general agreement—Albert Einstein. He never accepted the finality of the quantum mechanical renunciation of causality, or its claim to be the new fundamental theory. From the Solvay Conference of 1927, where the quantum mechanical synthesis had its first major discussion, to the end of his life, Einstein never stopped raising questions about this new approach to physics. At first he tried to propose conceptual experiments that would prove the logical inconsistency of quantum mechanics, but these attempts were all turned aside successfully by Bohr and his collaborators. In 1935 Einstein began to emphasize another basic limitation in quantum mechanics, as he saw it. He argued that its description of physical reality was essentially incomplete, that there were elements of physical reality that had no counterparts in the theory. Bohr's response to this was to reject Einstein's criterion of physical reality as ambiguous, and to claim that only through his own principle of complementarity could one arrive at an experimentally meaningful criterion of completeness.

Einstein recognized the power of quantum mechanics, calling it 'the most successful physical theory of our time', but he would not admit it as the basis for theoretical physics. He refused to give up the idea that there was such a thing as 'the real state of a physical system, something that objectively exists independently of observation and measurement, and which can, in principle, be described in physical terms'. Einstein was convinced that when a theory giving a complete physical description was developed, the position of quantum mechanics in the framework of this future physics would be analogous to that of statistical mechanics in the framework of classical physics. It would be the theory to use when only incomplete information was available or when only an incomplete description was wanted.

Einstein's colleagues could only regret that he had chosen to follow a path separate from the rest. As Born wrote: 'Many of us regard this as a tragedy—for him, as he gropes his way in loneliness, and for us, who miss our leader and standard-bearer.' To Einstein himself the choice was inevitable. He was prepared for the 'accusation' brought against him sometimes 'in the friendliest of fashions', but sometimes not: he was accused of 'rigid adherence to classical theory'. But, he wrote, it was not so easy to declare guilt or innocence of this charge 'because it is by no means immediately clear what is meant by "classical theory"'. Newtonian mechanics was a classical theory, but it had not been an acceptable claimant as the fundamental theory underlying physics since the introduction of field theory. Field theories were never completed—neither Maxwell's theory of electromagnetism nor his own theory of gravitation—since they were never extended to include the sources of the field in a non-singular way. Einstein did plead guilty to adherence to the programme of field theory; for it was his hope that a complete field theory would provide the basis for all of physics, giving that complete description he missed in the quantum mechanics he had helped so much to develop. He saw his whole career as striving to create a new unified foundation for physics. That was what he meant when he ended his scientific autobiography by writing that he had tried to show 'how the efforts of a life hang together and why they have led to expectations of a definite form'.

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