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THE QUANTUM THEORY OF RADIATION

Max Planck (1858-1947)

BORN

Max Karl Ernst Ludwig Planck [1]

ALMOST HALF A CENTURY HAS elapsed since Max Planck's discovery of the quantum of action, a time sufficiently long to estimate its importance for science and, more generally, for the development of human thought. There is no doubt that it was an event of the first order, comparable with the scientific revolutions brought about by Galileo and Newton, Faraday and Maxwell. Like these it has changed the whole aspect of physics and deeply influenced all neighbouring sciences, from chemistry to biology. Its philosophical implications reach far beyond the epistemology of science itself into the deepest roots of metaphysics.

What kind of man was he who initiated this great movement? . . . my best help must be the memory of years of personal contact and friendship, which have left an unforgettable impression.

Planck came from an old family of lawyers, public servants and scholars. One of his ancestors was a minister in Suabia who later became a professor of Divinity at Göttingen. One of the grandchildren of this man was

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[1] Max Born, *Obituary Notices of Fellows of the Royal Society*, 6, 1948, 161–180.

to become a celebrated jurist, professor of law at Göttingen, distinguished as the founder of the German Civil Code (*Deutsches Bürgerliches Gesetzbuch*). The Planck-Strasse, where I later lived for several years, was called after him. He lost his sight at an early age, and I remember the venerable figure of the blind old "Excellency" from my own student days. He and Max Planck's father were cousins; the latter also a distinguished jurist and professor of law at Kiel University. In 1867 he was called to Munich, and is said to have enjoyed the confidence of his colleagues and played an important part in the administration of the university.

This ancestry of excellent, reliable, incorruptible, idealistic and generous men, devoted to the service of the Church and State, must be remembered if one wishes to understand the character of Max Planck and the roots of his success. For his work was directed by just the same traits combined with a sincere belief in the simplicity of nature and an absolute confidence in logical reasoning from facts.

Max Karl Ernst Ludwig Planck was born at Kiel on 23 April 1858. Here he spent his early childhood. When he was nine years old the family moved to Munich. He became a pupil of the Maximilian Gymnasium (Grammar School), where he received his first scientific inspiration from his mathematics teacher, Herman Müller, an ingenious and sharp-witted man who knew how to demonstrate the laws of physics from simple, forceful examples.

In his autobiography Planck says about these beginnings: "What led me to my science and from my youth filled me with enthusiasm, is the fact—not at all self-evident—that our laws of thinking conform with the lawfulness in the passage of impressions which we receive from the outer world, thus making it possible for man to gain information about that lawfulness by mere thinking. In this it is of the highest significance that the outer world represents something independent of us and absolute with which we are confronted, and the search for the laws which govern this absolute has appeared to me as the most fascinating work of a lifetime."

These ideas are characteristic of Planck's whole attitude to science, and it was Müller who encouraged and assisted him in developing them.

The principle of conservation of energy was welcomed by Planck "like a gospel" as the first of those "absolute" laws.

When it came to the choice of a profession there were, however, other competing interests. He considered for a time the study of classical philology. He tried his musical gifts in composition, but came to the conviction that they did not suffice for original production. In the end physics prevailed. Music, however, remained an essential part of his life. He became an excellent pianist and found in playing deep enjoyment and recreation.

Planck studied for three years at the University of Munich. There were no chairs of theoretical physics at that time, so he attended the lectures on mathematics by Gustav Bauer and Ludwig Seidel, and on physics by Ph. von Jolly. They gave him a solid foundation of knowledge, but the wide horizon of science was opened to him only when he went to Berlin, where he studied one year under Helmholtz and Kirchhoff. However, so he reports, it was not from their lectures that he benefited. Helmholtz was never properly prepared, he improvised with the help of a little notebook and made mistakes in the calculations on the blackboard, so that his students felt that he was just as bored as they themselves. Kirchhoff's lectures on the other hand, were carefully worked out, each sentence well considered, but the whole was dry and monotonous. It was when Planck turned to their writings that he was fascinated. His main interest was still the principle of conservation of energy. Soon he discovered a new source of enlightenment in the publications of Clausius, which made a deep impression on him through their clear language and lucid explanations. Here he learned for the first time to distinguish between the two fundamental theorems, as formulated by Clausius, and from this time on his whole scientific thinking was rooted in the conceptions of thermodynamics.

His doctor's thesis, Munich 1879, is the first of his papers dealing with the second theorem. He was not satisfied with Clausius' definition of irreversibility, namely that a process is irreversible if it cannot be made to go in the opposite direction, like the conduction of heat; Planck regards this as insufficient, as it does not exclude the possibility of reversing the result of the process in an indirect way, which is just what should be excluded. So he suggests that a process should be called irreversible, or as he prefers to say "natural," if it cannot be completely undone without compensation. Entropy is a measure of the "predilection" of nature for the final state and it increases in all "natural" processes.

Planck's expectation of a favourable reception of his paper was not fulfilled. Helmholtz was indifferent, Kirchhoff objected that entropy was only measurable by reversible processes and should therefore not be applied to irreversible ones. An attempt to get in personal contact with Clausius in Bonn failed, and a correspondence with Carl Neumann in Leipzig remained futile.

But Planck was not discouraged. He continued his work on thermodynamics in a series of papers from 1880 to 1892, the first of which was used as thesis for the admission as *Privatdozent* (habilitation) at Munich University, which he obtained in 1880.

If one reads these articles now, one has the feeling of meeting old acquaintances. Everything seems to be familiar, the definitions, demonstrations, even the symbols. The reason is that we have all been nursed with Planck's book on thermodynamics, in which he has condensed the result of his previous work. So systematic was his mind, so well considered every word, every formula he wrote, that hardly anything had to be changed in the final compilation. This book *Vorlesungen über Thermodynamik* appeared first in 1897 and has since had numerous new editions.

Planck's interest in physics had always a philosophical background, namely, his fundamental belief formulated above, that the human mind can penetrate into the mysteries of nature by pure thinking because of a harmony between the laws of mind and the laws of nature. Therefore, he always preferred deductive, sometimes even axiomatic methods. This is quite obvious in his thermodynamical work. Once he was convinced of the general and universal law of the increase of entropy he tried to deduce from it as much as possible. Equilibria are then characterized by maxima of entropy, or by equivalent extrema of other thermodynamical potentials. These extremal principles of thermodynamics formed therefore the basis of his work, in contrast to the usual methods of the physico-chemists who preferred special cyclic processes which appealed to their intuition. Planck did not know at the time that his extremal principles had already been discovered and applied by Willard Gibbs, and it is only natural that he felt a certain disappointment when he found this out. He was still *Privatdozent* at the University of Munich and waited with some impatience for the offer of a professorial chair. Yet the chances were slight since theoretical physics was not an acknowledged academic subject.

In order to make himself better known in the scientific world Planck decided to compete for the prize for 1887 of the Philosophical Faculty of Göttingen, demanding a thesis on the conception of energy. Before this paper was finished Planck was offered a chair as "extraordinary" (i.e. not full, or "ordinary") professor of theoretical physics at the University of Kiel, the place of his birth. He has told us in his autobiography that the day when he received this call was one of the happiest of his life; for although he was living quite comfortably in the house of his parents, he yearned for independence and a house of his own. Now it became his ambition to justify the confidence shown to him by his colleagues in Kiel. He quickly finished his work for Göttingen and was successful against two competitors, though he was awarded only the second prize. The report of the Faculty contains a paragraph criticizing his attitude to Weber's law of electro-dynamic interaction. Wilhelm Weber, then Professor of Physics at Göttingen, was involved in a sharp controversy with Helmholtz. If Planck's siding with the latter cost him the first prize at Göttingen, he was soon compensated by the interest which the Berlin physicists took in the young scholar.

Planck now returned to his favourite subject and wrote four big papers with the common title "On the Principle of the Increase of Entropy" (1887, 1891). The first of these, where again instead of "irreversible" the term "natural" processes is used, contains the introduction of thermodynamical potentials and the derivation of their extremal properties, already mentioned above. Planck calls the expression $w = u + pv - Ts$ "Massieu's function" which to-day goes under the name of Gibbs' Potential. The equilibrium of different phases is discussed and a general outline of the theory of chemical equilibrium given. The following papers fill this frame with detail. For obtaining concrete results explicit expressions for the thermodynamical potentials must be known. Planck made simple and natural assumptions, for instance that in a dilute solution the thermodynamic potential is a linear function of the concentration of the dissolved particles. In this way he developed a formal apparatus which he applied to deducing observable facts.

This was a period when the newly established science of physical chemistry produced discoveries and theories in abundance. The law of mass action was established by Guldberg and Waage; the properties of dilute solutions were studied by Van't Hoff and those of electrolytes by Arrhenius. Planck deduced many of these results from his principles, sometimes independently of and even prior to the chemists. He gave a thermodynamical derivation of the dissociation of gases, of the osmotic pressure and of the lowering of the freezing point in solutions. Discussing the observed values of the freezing point in many solutions of salts, he arrived at the conclusion that the salts in solution must be dissociated. He saw in this result a thermodynamical foundation of the theory of electrolytic dissociation which Svante Arrhenius had developed about the same time from a large amount of experimental material. Arrhenius however rejected Planck's thermodynamical reasoning because he believed that the ionic state was essential for his hypothesis. Planck insisted that the thermodynamical laws are equally applicable to charged and neutral particles—which is certainly right; yet the actual form of the laws may well depend on the charge, as modern investigations have shown (Debye and Hückel). To-day we can therefore say that none of the adversaries was quite right. If Planck resented the misunderstanding of his work there is no trace of it in his publications. He welcomes the agreement of conclusions reached by so widely different methods and sees in it a confirmation of his belief in the fundamental character of the second law of thermodynamics.

After Kirchhoff's death the Philosophical Faculty of Berlin, apparently under Helmholtz's influence, offered in 1899 the Chair of Theoretical Physics to Planck. It was an "extraordinary" Professorship which however in 1892 was converted into an "ordinary" one. Planck describes the subsequent years as a most important period which widened his scientific horizon by bringing him, for the first time in his life, in personal contact with leading men in his field. Helmholtz, whose works he had admired, won also his personal veneration by his simplicity, dignity and kindness. A word of appreciation from him made him happy. Planck was also on excellent terms with Helmholtz' successor, A. Kundt, and a close friendship developed later with H. Rubens.

In the first period of Planck's life in Berlin he suspended his thermodynamical work for another task, which attracted his musical interest.

A big harmonium with numerous keys, built on Helmholtz' suggestion in pure tuning, was at that time delivered to the Department of Physics. Planck learned to play this complicated instrument and studied the effect of the pure tuning as compared with the tempered one introduced by Bach; he found the unexpected result, published in a special paper (1893), that our ear prefers decidedly the tempered scales.

At that time there had grown, under the leadership of Wilhelm Ostwald, a school of "energetics" which proclaimed that the law of energy was a sufficient basis for the derivation of the whole of physics and chemistry. Boltzmann took up this challenge and was soon involved in a sharp controversy with this group. Planck came to his assistance in an article (1896) which revealed, for the first time, his polemic gifts. Ostwald distinguished three different types of energy corresponding to the three dimensions of space: energy of distance, of surface and of volume. Planck replied that there are cases where no volume energy in Ostwald's sense exists, as for instance in the case of an ideal gas where the energy depends only on temperature and not at all on the volume. Another point of controversy was the failure of the energetics school to understand Clausius' Second Theorem. They compared the flow of energy from a higher level of temperature to a lower with the falling of a weight without taking into account the irreversibility of the process. This superficial analogy was violently opposed by Planck. Although the principle of conservation of energy was, right from the beginning, foremost in his mind, he was perfectly clear that it alone was an insufficient foundation on which to build up mechanics and that a much more powerful principle, such as that of least action, was needed. With regard to thermodynamics he defended Clausius' distinction between reversible and irreversible processes.

Planck complains in his autobiography that in this case, as in many others, he did not succeed in convincing his colleagues by arguments which seemed to him, though theoretical, perfectly valid. In fact, the defeat of the energetics' school was eventually due to Boltzmann's atomistic theory which Planck, at that time, did not fully appreciate.

Boltzmann's investigations on the kinetic theory of gases had led him to the construction of a certain quantity H, depending on the velocity distribution of the molecules, which he could prove was continuously decreasing in time. By identifying $-H$ with the entropy he obtained a kinetic interpretation of Clausius' second law. This atomistic conception of irreversibility made a profound impression and was generally accepted.

Planck confesses himself that to begin with he was not only indifferent but somewhat doubtful about Boltzmann's statistical views. The reason is that he believed the law of the increase of entropy to be just as general and free from exception as the law of conservation of energy while in Boltzmann's theory it appeared only as a probable law: the quantity H might occasionally increase, the entropy decrease.

E. Zermelo, a young and temperamental pupil of Planck, attacked Boltzmann's statistical ideas, using a theorem of Poincaré according to which any mechanical system is quasi-periodic; how then could a quantity defined in terms of mechanical variables, like Boltzmann's H, permanently decrease? It was not difficult for Boltzmann to refute this argument by showing that the definition of his function H involved probability and that therefore the theorem of its decrease had to be understood statistically. The controversy was carried on from both sides with considerable heat; Boltzmann brought into play his sarcastic wit, hitting also Planck himself who had backed his pupil. From that time on the relation between the two men was not too friendly, until the atomistic derivation of the radiation law by Planck mellowed Boltzmann's mind. In fact, nobody has done more to foster and spread Boltzmann's ideas than Planck; his theory of radiation is completely built on them and modelled on their analogy, and one of its main results was the determination from radiation data, of the constant k in Boltzmann's fundamental relation $S = k \log P$ between entropy S and probability P.

Discussing this question in his autobiography Planck expresses a slight resentment at the usual nomenclature "Boltzmann's constant" for this factor k, pointing out that Boltzmann had neither introduced it nor ever thought of determining it numerically, leaving this task to his colleague Loschmidt. This indicates that the quarrel with Boltzmann had left traces in his mind which still were discernible in his old age when he wrote those lines.

Planck's interest in heat radiation was roused by the experimental work done at the Physikalisch-Technische Reichsanstalt (National Physical Laboratory) in Berlin-Charlottenburg on the spectral distribution of the radiation emitted by a "black body." There were two prominent teams at work, Lummer and Pringsheim, Rubens and Kurlbaum. Their measurements directed Planck's attention to Kirchhoff's theoretical investigations of the properties of the radiation of a "black body," that is to say, the radiation in a cavity bounded by perfectly reflecting walls and containing arbitrary emitting and absorbing substances. He had shown that an equilibrium is established in the course of time where all substances have the same temperature and the radiation in all its properties, including the spectral distribution (energy per unit wave-length), is independent of the bodies and only a function of temperature. This so-called "normal spectrum" is therefore something "absolute," a great attraction for Planck, whose philosophical mind was always directed towards the search for the "absolute." Henceforward the explanation of this law was his aim which he pursued from 1896 on with amazing persistency, always in contact with the parallel experimental investigations of the Reichsanstalt. The series of papers (1897–1901) exclusively dealing with this problem and ending with complete success are a testimony not only to Planck's skill and ingenuity, but also to his character, his unbending will and untiring industry, his cautious patience combined with greatest audacity. His was, by nature, a conservative mind; he had nothing of the revolutionary and was thoroughly sceptical about speculations. Yet his belief in the compelling force of logical reasoning from facts was so strong that he did not flinch from announcing the most revolutionary idea which ever has shaken physics.

Maxwell's electromagnetic theory of light was at that time beginning to conquer the continent. Planck accepted and used it for his purpose. As according to Kirchhoff the nature of the emitting and absorbing substances was irrelevant for black body radiation, Planck chose a simple model, namely linear oscillators with different proper frequencies and small damping. He expected to find that the exchange of energy by emission and absorption of radiation would lead automatically to a final equilibrium state in agreement with Kirchhoff's results. The first step in this direction consisted in the calculation of the averaged emission and absorption of an oscillator situated in a given radiation field. To represent the latter, the electromagnetic field components were expanded in Fourier series with arbitrary amplitudes and phases. This is now a standard method of theoretical physics and so well known that few physicists will realize the effort needed to invent it. This effort is still discernible in Planck's book on the theory of radiation which appeared much later and contains a condensed form of the calculations. The main result was a relation between the mean energy u of an oscillator of a given frequency ν and the mean energy density ρ of the surrounding radiation in stationary (statistical) equilibrium,

$$\rho = \frac{8\pi\nu^2}{c^3}\, u.$$

This relation is independent of the damping of the oscillator, a fact which meant a considerable simplification of the problem; for it was thus reduced to the study of the system of oscillators, each of which had only one degree of freedom. On the other hand, Planck's original hope, that the oscillators would produce an exchange of energy between different frequencies and thus lead directly to the establishment of the normal spectrum, was disappointed, as each oscillator was found to be sensitive only to the radiation of its own frequency.

Here again a controversy with Boltzmann developed. The latter denied that the interaction of the oscillators with the radiation was irreversible and pointed out that every single process considered by Planck could just as well go in the opposite direction, even the emission of a spherical wave by an oscillator; for in a stationary state to each expanding wave there corresponds a contracting wave which transfers energy to the oscillator. This is formally correct, but nevertheless Planck was perfectly right and showed a deeper insight in a matter of statistical physics even than Boltzmann. Just as in a gas the mechanical reversibility can only be transformed into thermodynamical irreversibility through the introduction of the hypothesis of molecular disorder (i.e. by replacing rigorous expressions by averaged ones) one has in the case of radiation to introduce a corresponding assumption which Planck called the hypothesis of "natural radiation." It consists in averaging the phases and amplitudes of the simple harmonic waves into which the radiation can be decomposed. Thus this dispute was not futile but led Planck to greater clarity about his own procedure.

After the failure of his first attempt Planck looked for another way of attack and found it in the use of thermodynamical conceptions and finally in the application of the statistical methods due to his adversary Boltzmann.

Planck had the idea that he would obtain simple results by investigating the relation of energy U to entropy S, not to temperature T. For a system in a fixed volume one has the thermodynamical formula $TdS = dU$, from which one easily obtains

$$\frac{d^2S}{dU^2} = \frac{-1}{T^2\dfrac{dU}{dT}}.$$

If the energy U is a known function of the temperature, the right-hand side can be regarded as a given function of U; hence one has a differential equation to determine $S(U)$.

Now at that time W. Wien had published a law for the spectral distribution of radiation of the form $U(T) = Ae^{-B/T}$ (B proportional to the frequency), which was attractive by reason of its similarity to Boltzmann's statistical distribution law and also well confirmed by experiment in a wide spectral region. If it is introduced into the previous formula one finds

$$\frac{d^2S}{dU^2} = -\frac{1}{BU}.$$

a result so surprisingly simple that Planck first believed it would be generally correct. At this point Planck's close connexion with the experimentalists of the Reichsanstalt was decisive. Through the measurements of Lummer and Pringsheim and still more those of Rubens and Kurlbaum, it became more and more clear, that Wien's radiation law, though very satisfactory for short waves and low temperatures, was not in agreement with the facts for long waves and higher temperatures, where it had to be replaced by another law, namely that the energy per frequency interval is proportional to the temperature, $U(T) = CT$. This law is now known under the name of Rayleigh-Jeans; in fact Lord Rayleigh showed about the same time, in 1900, that it is a necessary consequence of ordinary statistical mechanics applied to radiation, and this point of view was stressed by Jeans again in 1909. One finds in this case

$$\frac{d^2S}{dU^2} = -\frac{C}{U^2},$$

again a surprisingly simple result.

Thus Planck had two limiting cases to ponder about. In describing this period Planck says that fate was kind to him. He had often been pained by the lack of interest of his colleagues in his work; but now this turned to his advantage; nobody else had the idea to consider the entropy as the crucial quantity, and he was allowed to follow his plan to its end without interference or competition. The next problem was to combine the two expressions into one, that they appear as the limiting cases for large and for small U. Planck noticed at once that this is achieved by taking the reciprocal of d^2S/dU^2 and adding the two expressions $-BU$ and $-U^2/C$; taking again the reciprocal he found the differential equation

$$\frac{d^2S}{dU^2} = \frac{-C}{U(U+BC)}.$$

This adding up was one of the most fateful and significant interpolations ever made in the history of physics; it reveals an almost uncanny physical intuition. Five years later it became much more comprehensible and natural by an interpretation due to Einstein (made in the same paper where he correlated Planck's quanta with the photoelectric effect); he remarked that the reciprocal of d^2S/dU^2 has a simple physical meaning: it represents the mean square fluctuation of energy $\overline{\Delta U^2}$; and it is well known that mean square fluctuations are additive, if due to independent causes. This argument was then used by Einstein as an indication of the independent existence of light quanta; but that is beyond the scope of this article.

The combined formula, which contains two constants, can now be integrated and leads directly to the new radiation formula, which Planck submitted to the Berliner Physikalische Gesellschaft on 19 October 1900.

He tells us that the next morning his colleague Rubens appeared to inform him that in the same night, after the meeting, he had compared Planck's formula with his own measurements and found everywhere satisfactory agreement. Lummer and Pringsheim believed first that there were deviations but discovered soon that these were due to an error in computation. Many later experiments have been made to check Planck's formula, with the result that the agreement has been found to become more and more perfect with the improvement of methods of measurement.

Yet it was only an interpolation, a real physical meaning had to be found. At this point Planck's attention was directed to Boltzmann's fundamental relation between entropy and probability, $S = k \log P$. Hence he investigated the question whether the expression of P obtained by substituting for S the value corresponding to the new radiation law could be interpreted as a probability. In a lecture given to the German Physical Society on 14 December 1900, he announced the result that this interpretation is possible indeed. Apart from the constant k which was recognized to be the absolute gas constant per gram-molecule, there appeared a new constant of the dimensions (energy × time) which he called the "elementary quantum of action" and denoted by h, now always quoted as Planck's constant. Planck, right from the beginning, saw the essential feature of his discovery in this "quantum of action." His contemporaries were more stirred by the "quantum of energy" $\epsilon_0 = h\nu$, and by Planck's contention that the energy of the emitting and absorbing oscillators was "atomistic," always a multiple of ϵ_0. It was this assumption which led Planck to the expression for the mean energy of a system of oscillators; using Boltzmann's distribution law one has

$$u = \frac{\sum_{n=1}^{\infty} n\,\epsilon_0 e^{-n\epsilon_0/kT}}{\sum_{n=0}^{\infty} e^{-n\epsilon_0/kT}} = \frac{\epsilon_0}{e^{\epsilon_0/kT} - 1}, \quad \epsilon_0 = h\nu,$$

and, with the help of Planck's previous result concerning the relation of radiation and oscillators, one finds the expression for the radiation density

$$\rho = \frac{8\pi\nu^2}{c^3}\, u = \frac{\dfrac{8\pi h}{c^3}\,\nu^3}{e^{h\nu/kT} - 1}.$$

This formula contains all previously known radiation laws, the law of Stephan and Boltzmann for the total radiation, Wein's displacement law and, of course, the two limiting laws of Rayleigh-Jeans for large T and of Wien for small T. From the known constants of these Planck derived numerical values for his two constants k and h. From k he calculated the number N of atoms per gramme molecule (Avogadro's or Loschmidt's number), and, with the help of Faraday's law, the elementary electric charge e; his values were much more reliable than all known before and were later confirmed by many other methods.

Planck was perfectly clear about the importance of his discovery. We have not only the testimony of his wife but also an account of his son Erwin, given to and reported by Professor Bavink. It was in 1900 when his father, on a walk in the Grunewald, near Berlin, said to him: "To-day I have made a discovery as important as that of Newton." Planck has, of course, never said anything like that in public. His modest and reluctant way of speaking about his work has caused the impression that he did himself not quite believe in his result. Therefore, the opinion spread, especially outside Germany, that Planck "did not seem to know what he had done when he did it," that he did not realize the range of his discovery. That this is wrong can clearly be seen from his autobiography; though it was written in his old age, we have no reason to doubt that it correctly reflects his thoughts in the years following his discovery. Planck reports that he tried hard to fit the quantum of action into the frame of classical theory, but with no success. Then he continues: "But this quantity (the constant h) proved to be unwieldy and resistive against all attempts of this kind. As long as it could be regarded as infinitely small, i.e. for larger energies and longer periods, everything was in good order. But in the general case there appeared somewhere a cleavage which became the more conspicuous, the faster the vibrations considered. The failure of all attempts to bridge this gulf, soon removed all doubt that the quantum of action plays a fundamental part in atomic physics, and that with its appearance a new epoch of physical science has begun. For it forebodes something unheard-of destined to reform thoroughly our physical thinking which since the invention of the infinitesimal calculus through Leibniz and Newton was based on the assumption of continuity of all causal relations."

It has been generally acknowledged that the year 1900 of Planck's discovery marks indeed the beginning of a new epoch in physics. Yet during the first years of the new century very little happened. It was the time of my own student days, and I remember that Planck's idea was hardly mentioned in our lectures, and if so as a kind of preliminary "working hypothesis" which ought of course to be eliminated. Planck himself turned to other fields of work. But that he never forgot his quanta is shown by the publication, in 1906, of his book *Vorlesungen über die Theorie der Wärmestrahlung* which made a profound impression by the masterly presentation of the successive steps which led to the quantum hypothesis.

A year earlier Einstein's paper, already quoted, had appeared in that famous volume (1905) of *Annalen der Physik,* which contains also two other fundamental articles of Einstein, one on relativity and one on the Brownian movement. Einstein showed that the quanta were not a feature of radiating heat, but of radiation in general, and he produced experimental and theoretical evidence for a corpuscular interpretation of light. A series of phenomena like the photo-electric effect, the excitation of X-rays by the impact of electrons on the target, Stokes' rule of fluorence could be simply explained in terms of 'light quanta' $h\nu$. Now the interest of the experimentalists was roused and progress became quicker. From the standpoint of the theory a decisive step was made again by Einstein in 1907, when he applied Planck's formula for the mean energy u of a system of oscillators (given above) to the vibrations of atoms, molecules and solids, explaining in particular the deviations of the specific heat of solids from the classical law of Dulong and Petit. This initiated a great amount of experimental research, for instance the investigations of Nernst and his school on the specific heat at very low temperatures. But I cannot follow up the history of quantum theory in general as this would mean a description of the greatest part of modern physics; I must confine myself to Planck's own contribution.

There is a publication of his in 1910 where he summarizes the situation. He discusses a number of papers by J. J. Thomson, Larmor, Stark and Einstein which use the quantum hypothesis for explaining divers phenomena; but he is very cautious in regard to Einstein's revival of the corpuscular theory of light. Its main argument is the existence of electrostatic fields which from Einstein's standpoint would be something completely different from radiation fields. Can one abandon the unification due to Maxwell of all electromagnetic fields in view of the existing evidence? His conclusion is that electrodynamics is very probably right, but physical statistics possibly wrong.

In his lecture to the Solvay Congress 1911, he made a decided attempt to develop a modified statistical mechanics by assuming the phase space of Gibbs divided up in finite cells of the size h for each pair of conjugate variables p, q. At the same time he changed his assumption about absorption and emission; absorption was supposed to be continuous, emission discontinuous (1911). This strange hypothesis seemed to him the only way out of the dilemma between quantum effects and electromagnetic theory. Many physicists, particularly those of the younger generation, regarded Planck's "second quantum theory" as a weak compromise. It is to-day hardly worthwhile to discuss its pro and contra. But one must not forget that it led to a most important result, the zero point energy $\frac{1}{2}h\nu$ per oscillator. It appears formally when one expands Planck's formula for the resonator energy for large temperatures

$$u = \frac{\epsilon_0}{e^{\epsilon_0/kT} - 1} = kT - \frac{\epsilon_0}{2} + \ldots ,$$

where the terms indicated by dots vanish for $T \to \infty$. This expression shows that Planck's formula does not precisely tend to the equipartition value kT (which corresponds to Rayleigh-Jeans' formula for ρ) but differs from it by $\frac{1}{2}\epsilon_0 = \frac{1}{2}h\nu$. Planck's new statistics leads to a value for u which is larger than that given above by $\frac{1}{2}\epsilon_0$ and tends therefore exactly towards kT for $T \to \infty$. In this way he found a new approach to Nernst's theorem and the zero point entropy of gases (1916). Later research has demonstrated the reality of the zero point energy, for instance by its influence on the scattering of X-rays in crystals. Planck himself regarded his second quantum hypothesis as so important that he made it the basis of the second edition of his book *Wärmestrahlung,* which appeared in 1913. Another modification of the theory is contained in a series of papers (1915, 1917) where he replaced the oscillators by rotators. Here he used a method first introduced by Einstein in his theory of Brownian motion and later improved and applied to radiation by Fokker; it describes the changes in time and space of a distribution of particles subject to small irregular impulses with the help of a partial differential equation containing as coefficients the mean displacement and the mean square displacement for a given small time. This formula is now generally quoted as the Fokker-Planck equation, and its full range of application seems to be not at all exhausted yet.

The year 1913 marks a turning point in quantum theory as there appeared Niels Bohr's first papers on the quantum theory of the electronic structure of atoms. A straight development in which Planck took an active part, led from here to modern quantum mechanics. But in the intermediate period he turned his mind to many other subjects of which a short account must be given.

His investigations on radiation convinced him that the electromagnetic field showed statistical features similar to those of a gas; the amplitudes and phases of the elementary waves are arbitrary and may be distributed at random. In this way he came to his theory of "natural" or "white" light (1902), which later was taken up by his pupil Max von Laue. Then he became interested in ordinary optics, in particular in Drude's theory of dispersion in which he introduced radiation damping of the oscillators (1902, 1903, 1904, 1905). He calculated the extinction of light in an optically homogeneous medium of normal dispersion and compared his results with an older theory of Lord Rayleigh concerning the propagation of light in a vacuum in which numerous non-conducting particles are dispersed; he found the same law for the extinction coefficient as had Rayleigh, although the dispersion law is completely different for the two models. The experiments made by Hagen and Rubens on the optical properties of metals induced Planck to a theoretical study of this subject (1905).

He returned to the theory of gases (1908) and generalized Boltzmann's method in such a way that it could take into account van der Waal's corrections due to the finite volume of the molecules.

But the subject that caught Planck's imagination more than anything else was Einstein's theory of relativity, published in 1905. In Planck's scientific autobiography is a remarkable page where he explains how his search for "the absolute," the main spring of his scientific activity, is compatible with his interest in the principle of relativity. "One might regard this as a contradiction. . . . This presumption is based on a fundamental error. For everything 'relative' presupposes something 'absolute,' it has only significance if it is opposed to some absolute. The often quoted sentence 'Everything is relative' is just as misleading as thoughtless. So at the bottom of the so-called theory of relativity there is something absolute, namely the metric of the space-time continuum, and it is just a particularly attractive problem, to discover the Absolute which lends a meaning to a given Relative. . . ." He found the attraction of relativity in the search for those invariants which represent the "absolute." The velocity of light which in classical physics has only a relative meaning becomes in relativity an absolute invariant. The next important invariant is the action integral of mechanics; the laws of motion can be obtained by the principle of least action in relativity.

Planck applied this idea first to a mass point (1906) and found the relativistic form of the mechanical equations, a little earlier than Minkowski. He discussed Kaufmann's measurements of the deflexion of β-rays in regard to their bearing on the principle of relativity (1906, 1907). In 1908 he published a long paper on the general dynamics of moving systems, in which he expands the thesis of his pupil K. v. Mosengeil (published by Planck in 1907 after the young author's untimely death). As relativity teaches that mass is proportional to energy, and as the energy of a body depends on its heat content, a separation of mechanics and thermodynamics is impossible. Planck develops a combined theory based on the relativistic invariance of the principle of least action and obtains the transformation laws for energy, momentum, entropy and temperature; with the help of these the expression of these quantities in terms of the velocity can be obtained from their values in the rest system. If one supposes that these expressions derived for steady motion also hold for acceleration, one can write down the equations of motion. This paper contains a most remarkable section (§ 18) in which he predicts the possibility of utilization of "atomic energy." He is perfectly clear that every body contains, in [its] rest-mass, a colossal amount of "latent" energy, and says: "Though the actual production of such a 'radical' process might have appeared extremely small only a decade ago, it is now in the range of the possible, through the discovery of radioactive elements and their transmutation, and in fact the observation of continuous production of heat of radioactive substances is direct evidence for the assumption that the source of this heat is just nothing else than the latent energy of the atoms."

The Prussian Academy, mainly on the instigation of Planck, Nernst and Haber, created a special chair for Einstein which allowed him to pursue his ideas unhampered by teaching and routine work. Now for many years Planck and Einstein met at regular intervals at the Berlin Academy, and a friendship developed which went far beyond the exchange of scientific ideas. Yet it is difficult to imagine two men of more different attitudes to life: Einstein a citizen of the whole world, little attached to the people around him, independent of the emotional background of the society in which he lived—Planck deeply rooted in the traditions of his family and nation, an ardent patriot, proud of the greatness of German history and consciously Prussian in his attitude to the state. Yet what did all these differences matter in view of what they had in common—the fascinating interest in the secrets of nature, similar philosophical convictions, and a deep love of music. They often played chamber music together, Planck at the piano and Einstein fiddling, both perfectly absorbed and happy. Planck was an excellent pianist and could play on demand almost any piece of classical music, a great many by heart. He also liked to improvise either on a theme given to him, or on old German folk-songs which he dearly loved.

The collaboration of Planck and Einstein made Berlin, in the years preceding the first World War, the greatest centre of theoretical physics in the world. I was also fortunate to be called to Berlin. Planck wished to be freed from a part of his duties in routine teaching and persuaded the Prussian Minister of Education to found a new (extraordinary) Chair at the University of Berlin. This was offered to me, but alas, on the day of mobilization, 2 August 1914. There was not much of teaching and peaceful research for me during the following four years of war, yet I was in Berlin for long periods and saw Einstein and Planck frequently. A short walk brought me from my own house to Planck's, a villa in the suburb of Grunewald. I remember his studio, the walls covered with books, simple furniture, among which a high desk (like those found in old-fashioned offices) where he used to work standing upright. I had never been his pupil, not even attended one of his courses; I knew his papers and books, I had seen him from a distance at scientific meetings and perhaps exchanged a few words with him. He was at that time already a great and famous man, and I approached him with some shyness. But his kindness, his lovable expression, the hospitality of his house removed very quickly the barrier of age and experience. We had many fascinating discussions on physics and the topics of the day. He had very definite views and expressed them frankly, even if he did not expect agreement, but never in an offending way. The same systematic order, tidiness and clarity which distinguish his writings, were also characteristic of his attitude to the small and big questions of ordinary life. During the years of the war a great change came over him; sorrow darkened his friendly expression. It was not only the general suffering, the catastrophic end of the struggle which hurt his patriotic feeling deeply, but terrible personal loss. Planck's first wife, Marie Merck, had died in 1909. He had married again, Marga von Hoesslin. Three of the four children of his first marriage died during the war period. His eldest son Karl was killed in action near Thiaumont, France, in 1916. The two daughters, Emma and Margarete, were twins. One of them married Professor Ferdinand Fehling; she died in 1917 in childbirth; her sister took charge of the orphan baby and later married the widower. A year later exactly the same thing happened to her; she died after her first confinement, while the child lived. Both children were partly educated in the grandfather's house. Only one son, Erwin, of his first wife was left, and a young son, Hermann, of his second marriage. More tragedy was to come.

In spite of all this worry and sorrow Planck continued his scientific work, returning to his long neglected quantum theory, which through Bohr's papers of 1913, had suddenly become the focus of interest in the world of physics. Bohr's method of quantization was extremely successful for the one-electron problem; how could it be generalized for a system of many electrons? This problem was almost simultaneously solved by Sommerfeld, Epstein and Planck (1915, 1916). The methods differ in form but lead in all practical cases to essentially the same results. While Sommerfeld considers multi-periodic systems for which a separation of the Hamiltonian in independent pairs of co-ordinates and momenta is possible, Planck's method consists in a division of the total "phase space" of all co-ordinates and momenta into cells, with the help of pairs of surfaces nh apart ($n = 1, 2, \ldots .$) which are invariant integrals of the equations of motion. In this way he obtained for instance the energy in terms of quantum numbers for the rotator, the symmetric top, the ordinary and relativistic Kepler motion, etc. He even tackled the asymmetric top (1918). Then he applied the results to the optical problem of the rotational spectra of molecules (1917). There he had to overcome a particular difficulty connected with his method of quantization; this allowed for each set of quantum numbers small but finite domains in the phase space, while the observations showed rather sharp lines. The way in which he removed this apparent contradiction has to-day only historical interest like all the work done in this period. Therefore it suffices to mention some other papers which show that he always tried to attack the most interesting problems of the day. He calculated the heat of dissociation of the hydrogen molecule according to the "ring model" suggested by Bohr and Debye (1919). He tried to solve Gibb's paradox of statistical mechanics by a careful determination of the free energy of gas molecules with arbitrary velocity distribution (1922). Several papers under different titles deal with the fluctuations of energy in the black body radiation (1923, 1924). He discussed a difficulty concerning the free energy of atomic hydrogen gas; as Bohr had already noticed the partition function taken over the discontinuous states diverges in this case as the energy values approach zero like $-n^{-1}$. Planck's solution consists essentially in cutting off the discrete spectrum where the radius of the orbit reaches the linear dimensions of the vessel; he does not however neglect the remainder but shows that in these states the electron can be treated as a free particle.

One paper entitled "A New Statistical Definition of Entropy" (1925) contains a general formulation of Boltzmann's and Gibb's statistical expression of the entropy $S = k \log P$ for quantum systems; Planck defines P as the number of stationary states for which the energy does not exceed a given value E (instead of taking the sum over all states in a given narrow energy interval) and he shows that this leads to the correct expression for a system of oscillators and for a monatomic gas.

When the war ended I left Berlin. Max von Laue, Planck's celebrated pupil, wished to return to Berlin and to be near his master; so he offered me an exchange of my Berlin position (Extraordinariat) with his full professorship in Frankfurt-on-Main, and as Planck agreed, I accepted. From 1919 on Berlin enjoyed this constellation of three most-brilliant theoretical physicists, Planck, Einstein, v. Laue, which was soon to be enhanced by a fourth, Schrödinger. He had published in 1926 his paper on wave mechanics which made an immediate impression everywhere, even more than Planck's discovery in 1900; for the world of physics was prepared for this step by the work done during the preceding twenty-five years and in particular by the publications of de Broglie and the Göttingen school.

So it was only natural, that in 1928 when Planck reached his seventieth year and had to resign his Chair, Schrödinger became his successor. Planck, however, did not retire into inactivity. He remained permanent secretary to the mathematical physical class of the Berlin Academy and continued his scientific work and publications, free from the burden of lecturing to students.

Planck had never had a research school, like Sommerfeld in Munich, and the number of pupils who wrote a thesis under his direction is small. I have already mentioned K. von Mosengeil, E. Zermelo and M. von Laue; then there are Max Abraham, known through his book on Maxwell's theory of electricity, F. Reiche who wrote one of the first books on quantum theory, E. Lamla, H. Kallmann and a few others. Lise Meitner was Planck's assistant for a considerable time.

But large numbers of students have attended his lectures and studied his books. His normal course of lectures was published in 1930 in five volumes, corresponding to five semesters (2½ years) lecturing. The first four contain mechanics of points and rigid bodies, mechanics of continuous substances, electricity and magnetism, optics; the last volume gives a condensed account of thermodynamics, the theory of radiation and quantum theory. They are the prototype of similar lectures given at all German universities. An English translation has spread their influence over a wider area. Planck has edited books and lectures by Clausius and Kirchhoff. In 1910 he published a series of eight lectures given by him the previous year at Columbia University, New York; in 1922 a book entitled *Physikalische Rundblicke,* and more recently, 1943, a collection of his speeches and addresses in two volumes under the title *Wege zur physikalischen Erkenntnis.*

The last period of his scientific life is that of quantum mechanics. What he expected from the work of his successor is revealed in the address, with which he, as Secretary of the Academy, replied to Schrödinger's inaugural lecture (1929). Planck welcomed wave mechanics as the solution of a crisis threatening physics, namely the sceptical attitude towards the universal validity of the law of causality.

I quote the last words of this address: "You were the first to show how the spatio-temporal process in an atomic system can in fact be completely determined, though only under the supposition that one regards as their elements not the motions of particles but of material waves; and how the mysterious discontinuous proper values of the energy of the system can be calculated with absolute accuracy from your differential equation together with natural boundary conditions, while the question about the physical significance of the waves can be left undecided."

This crisis of causality occupied his mind very much, as is seen from his numerous popular writings and addresses. Before speaking of these it must be mentioned that up to his very old age he continued to publish papers on special subjects, mainly those on which he had worked in earlier periods. There is a series (1930, 1931, 1933) on the boundary layers of dilute electrolytes, one on the principle of le Chatelier and Braun (1934), one on the production of electricity in electrolytes. Most remarkable are three papers with the title "Attempt at a Synthesis between Undulatory and Corpuscular Mechanics" written in 1940, when he was above eighty years of age. They contain a careful consideration of the transition from wave mechanics to particle mechanics through the limiting process $h \rightarrow o$. He shows that to obtain this transition an additional condition must be fulfilled, and he postulates this condition to hold rigorously, instead of the usual boundary conditions of the Schrödinger equation. Translated into the language of optics it means the exclusion of all solutions which correspond to diffraction phenomena, as Wessel has pointed out. I do not share Planck's hope that his "modified wave mechanics" will bridge the gap between quantum and classical physics; but it shows clearly how deeply Planck was worried by the logical hardships which his own work has imposed on the physicists. This brings me to a short account of his philosophical writings which became more and more numerous with increasing age and predominate in his last period.

It is hardly possible to attach to Planck's work a label with one of the traditional philosophical systems; it has strains of rationalism, idealism, empiricism. But there is one school which he emphatically and repeatedly rejected: positivism. His spirited controversy with Ernst Mach is still worth reading. Planck started it in 1909 with an article on "The Unit of the Physical Picture of the World," published in the *Physikalische Zeitschrift*. The next volume (1910) of this periodical contains Mach's strongly ironical reply and a final article of Planck which is not less pointed and peppered. Mach defends his idea that all science is due to the principle of economy of thinking which itself can only be understood in the frame of Darwin's biological theory, and he claims to have thus found a basis for science free from all metaphysics. Planck's main answer is that this principle of economy itself is certainly metaphysical. There are many other points of disagreement. Mach was sceptical about the existence of atoms, he declared Boltzmann's kinetic theory, even the absolute zero of temperature, to be unproved hypothesis, and he attacked the Newtonian concept of absolute rotation, anticipating in some vague way Einstein's theory of general relativity. But this is the only point where he was right, in all other questions at issue Planck's physical intuition was confirmed by the later development of physics.

In 1930 Planck renewed his attack against the anti-metaphysical school in a lecture, "Positivism and the Real External World," in which he presents his arguments in a less caustic but most convincing way. I quote a paragraph containing the essence of this paper:

"The basis given to physics by positivism, though well founded, is too narrow, it has to be widened by an additional statement, whose importance is this: it frees science as far as possible from the incidences produced by the relation to human individuals. And this is achieved through a fundamental step into metaphysics, not imposed by formal logics but by common sense; namely through the hypothesis, that our personal experiences do not form the physical world, but that they only bring us messages from another world which lies beyond them and which is independent of them; in other words, that there exists a real external world."

The same idea appears in many of his philosophical lectures and articles. Their general tendency is to show that science is nothing but developed and refined common sense.

Meanwhile Planck's own child, quantum theory, had grown beyond all expectation and now dominated the whole of physics; but it had taken a direction which led straight away from Planck's fundamental convictions. Causality and strict determinism, even the assumption of an external objective world independent of ourselves became problematic. Planck discussed these questions in numerous publications, always maintaining the essence of his principles and trying to reconcile them with the facts of physics. Some of these articles culminate in a consideration of the paradoxes connected with the conception of free will in a deterministic world. Planck's solution is this: Determinism holds without exception, and we can use it for predicting not only events in inorganic nature, but even the behavior of other human beings—though never our own behavior. For by thinking about our possible decisions we influence them and can therefore not predict them. Hence there is no contradiction between the belief in free will and rigorous causality. An English version of Planck's ideas can be found in the Guthrie Lecture of the Physical Society, London, which he gave in 1932 under the title "The Concept of Causality," published in the *Proceedings of the Physical Society* and discussed in *Nature* (1932, p. 45). Planck was a religious man and several of his articles deal with the relation of science and faith (1930, 1947). He believed that science could contribute not only to material progress but also to the moral and spiritual development of mankind. There was no gap in his mind between his scientific and religious convictions.

Planck enjoyed good health up to his old age. This was certainly due to the simplicity and regularity of his life and to his custom of having real holidays. He spent the vacations mostly in the Alps, staying some weeks in lonely mountain villages near the high peaks, and then at his little property near Tegernsee. He loved the mountains and was a trained and hardened mountaineer. I visited him once in Trafoi when he was well over sixty; he had just returned from climbing the Ortler, a summit of 12,000 feet.

I was soon to meet him again in South Tyrol under different circumstances. After having been dismissed by Hitler in April 1933, my family and I left Germany at once for a little house in the Dolomites which we had rented for the summer. Planck spent the summer in a neighbouring valley, where I visited him. He told me then that in his capacity as President of the Kaiser Wilhelm Gesellschaft he had to pay a visit to Hitler and tried on this occasion to intervene in favour of his colleague Fritz Haber without whose method of fixing nitrogen from the air the First World War would have been lost by Germany from the beginning. Hitler's reaction was a violent outburst against the Jews in general. He finally brought himself into such a rage that Planck could do nothing but listen silently and take his leave. He later, in 1947, described the scene in *Physikalische Blätter.* After the failure of this attempt to plead for reason and restraint Planck seems to have given up all hope of changing the course of events, and he kept an outward peace with the powers in being. Yet there is no doubt about his true feelings, and the Nazis knew it. Goebbels wrote in his *Diary* (English edition by L. P. Lochner, p. 295): "It was a great mistake that we failed to win science over to support the new state. That men such as Planck are reserved, to put it mildly, in their attitude towards us, is the fault of Rust (the Minister of Education) and is irremediable." Planck continued to serve at the Academy, the Kaiser Wilhelm Gesellschaft and other public institutions, with the hope of saving German science and learning from total destruction. The Prussian tradition of service to the state and allegiance to the Government was deeply rooted in him. I think he trusted that violence and oppression would subside in time and everything return to normal. He did not see that an irreversible process was going on.

Planck has been in this country on several occasions and has had many friends here. In 1937 he came to Scotland to receive an Honorary degree at Glasgow and the honorary membership of the Royal Society of Edinburgh. He and his wife stayed in my house; it was the last time that we discussed matters scientific, political and personal. When I met him after the war at the Newton celebrations of the Royal Society in 1946 he was only a shadow of his former self, tired and frail, yet with his kindly smile unchanged. His house in Grunewald was destroyed in one of the big air raids on Berlin, and he lost everything, including his library. His son Erwin, the only surviving one of the four children of his first marriage, who held a high post in the Government, was involved in the July plot of 1944 against Hitler and was killed by the Nazis.

I know little about Planck's life during the war. He and his wife had found a refuge on the estate of a friend in Rogätz, on the river Elbe, near Magdeburg. There they came between the lines of the retiring Germans and of the advancing Allied armies, the battle raged around them for days. When Pohl, the physicist in Göttingen, heard of their plight he induced the Americans to send a military car and take them to the safety of Göttingen.

Planck bore his Job-like fate with quiet fortitude, resigning himself to the will of God. In this his deterministic philosophy may have helped him as well as his faith. He made his last home in Göttingen, but undertook long and tedious journeys when he was invited to lecture. On one of these occasions he fell seriously ill at Bonn but miraculously recovered from double pneumonia in spite of his eighty-eight years. So it could be hoped that he would reach his ninetieth birthday for which a great celebration was being prepared. Yet a few months before this date he began to fail and died on 4 October 1947 in Göttingen. The planned birthday celebration was changed into a memorial service which took place on 23 April 1948. It was attended by representatives of numerous scientific institutions in Germany and in many other countries.

The list of those institutions which have honoured Planck by awarding him a degree or by electing him a member is too long to be reproduced here. A few only may be mentioned. He had the German degrees of Dr. rer. nat. h.c., Dr. ing. h.c., Dr. med. h.c., also honorary degrees of several British universities, including Cambridge. He was a member of all the German and Austrian Academies (Berlin, Munich, Dresden, Göttingen, Vienna) and of many others (Britain, Denmark, Eire, Finland, Greece, Holland, Hungary, Italy, Russia, Sweden, Ukraine, United States). The Royal Society of London elected him a Foreign Member in 1926. He received the Nobel Prize in 1919. One of the small planets was "given" to him as a present on his eightieth birthday by the astronomers and called Planckiana. In 1930 he became President and in 1946 honorary President of the Kaiser Wilhelm Gesellschaft which has now been renamed "Max Planck Gesellschaft."

A Planck Medal has been founded by the German Physical Society, which he was the first to receive. He was awarded the Goethe-Preis of the city of Frankfurt-on-Main in 1946 and was appointed honorary member and "knight" of an American Mark Twain Society.

Thus his greatness has been acknowledged by his contemporaries. Will posterity confirm this judgment? We who have witnessed the incredible transformation of science which his work has brought about in less than half a century, have no doubt it will.

* * *

Max Born's excellent biographical sketch of Max Planck, originally published as an obituary by the Royal Society after Planck's death in 1947, is included in this volume because of its importance as a historical document. Born indicates the catholicity of Planck's interests, discussing his revolutionary contributions to atomism and physics as a whole. In our own commentary, we restrict our discussion to Planck's quantum theory of radiation, the most significant of his discoveries. Planck's Nobel Prize address of 1919, on the quantum of action, is presented after this commentary. The award, given in 1919, honored Planck for work done at the turn of the century.

As Born suggests in the previous essay, near the close of the nineteenth century, physicists were almost unanimous in the conviction that all the fundamental laws had been discovered and that there remained only mopping-up actions in the form of more precise experiments. It was genuinely felt that no matter how refined experimental apparatus became, there would be no serious departures from the theories known at the time. Thus it was taken for granted that ultimately all astronomical observations would take their proper place in the Newtonian gravitational system, even though at that moment the motion of the planet Mercury could not be accurately accounted for. Likewise, it was assumed that all the problems concerning radiation would one day yield to the laws of Maxwell. Although no one had any notion about the way radiation and matter are related or by what process matter emits and absorbs radiation, no one had any doubts that these questions would be answered in time by the proper application of Newton's laws of motion and Maxwell's theory of the electromagnetic field.

At this same time, physicists were encountering special difficulties in trying to understand the nature of the energy emitted in hollow enclosures by the walls surrounding such enclosures. This problem, which at first sight may have seemed uninteresting, became the rock upon which the ship of classical physics foundered.

In solving this problem, Planck was led to the discovery of the quantum of action and the concept of the photon. To understand the difficulty of the problem, we first note that when a body is heated it radiates energy; as its temperature rises, not only does the amount of energy emitted per second increase rapidly, but the quality of the emitted radiation changes visibly. For example, when the temperature is low, the emitted energy is concentrated mostly in the long wavelengths and the body glows with a cherry-red color; as the temperature of the body increases, the cherry-red color gives way to a yellow and finally to a blue-white color.

Now the late nineteenth- and early twentieth-century physicists, such as Gustav Kirchhoff, Lord Rayleigh, James H. Jeans, and Willy Wien, had explained most of the observed data relating to this type of radiation in terms of the classical electromagnetic and thermodynamic principles. But one observation stubbornly refused to yield to classical physics. This was the spectral distribution of the total amount of emitted energy. It was found that if one separated the emitted radiation into its various component colors by means of a spectroscope, the results obtained (the amount of radiant energy in each color) did not agree with the predictions of the theory. It was in dealing with this flaw in classical theory that Planck presented his revolutionary idea of the quantum of energy. In his Nobel address, which is reproduced here, he outlines the way in which he was led to the conception of the quantum of energy and ultimately to that of the quantum of action.

To study the properties of the radiant energy in an enclosure, he started out by picturing the material in the walls as being composed of simple harmonic oscillators, because Heinrich Hertz had already shown how such bodies emit and absorb energy. As he notes in his address, he was justified in doing this, since Kirchhoff had proved that the nature of the radiation in an enclosure does not in any way depend on the material of the walls, but only on their temperature. Using this simple model, Planck analyzed the way in which such oscillators would emit into and absorb energy from the enclosure. This energy—referred to as "black-body radiation" because it is the same as the energy emitted by a perfectly black body—is distributed in a definite way among all possible wavelengths. A certain fraction of the total radiant energy is concentrated in each color. The exact amount depends only on the temperature of the enclosure and the wavelength being considered.

The problem that Planck faced was to determine the mathematical form of the relationship between the concentration of energy in a particular color, the temperature of the walls, and the wavelength of the particular color being considered. As Planck notes in his address, he expected to discover this law for the distribution of black-body radiation within the framework of classical electrodynamics. This was his anticipation, in spite of his knowledge that previous similar attempts to achieve the same results by other physicists had failed. Planck proceeded by developing the most general laws of the emission and absorption of a linear harmonic oscillator. He saw at once, however, that there was nothing about the classical electrical properties of an oscillator that would cause it to absorb and emit radiation in such a way as to give a result that agreed with experiment.

We may understand the nature of the difficulty if we consider a harmonic oscillator vibrating with a definite frequency. It emits and absorbs electromagnetic radiation of this same frequency, according to classical electromagnetic theory. As a result, Planck could not account for the distribution of the total energy among the various frequencies, since he could see no way in which two oscillators vibrating at different frequencies could influence one another and establish a condition of equilibrium that depended only on the temperature. However much he tried to invent some method by which this might be achieved, he found himself up against the fact that each oscillator must interact in a reversible manner with the radiation field. Thus, a distribution of the radiation over the entire spectrum could not occur since this would mean that the oscillator would have to re-emit radiation of all frequencies even though it could absorb only one frequency.

In other words, Planck had first tried to find some asymmetrical relationship between the rate at which an oscillator emits energy and the rate at which it absorbs energy. He felt that if he then expressed this relationship in terms of the temperature, he would obtain the condition for equilibrium between the oscillators and the radiation—and that then the energy-distribution formula would drop into his lap. This, however, proved to be a vain hope, since, as Ludwig Boltzmann pointed out, all the effects considered by Planck could, according to the laws of classical mechanics, work in exactly the reverse direction, so that emission and absorption are completely symmetrical. This led Planck finally to discard the electromagnetic approach to the problem (that is, the approach through the laws of radiation) and to consider the laws of thermodynamics, with which, as he says ". . . I felt more at home. . . ." Since Planck had done a good deal of research into the second law of thermodynamics, he decided to attack the problem from that direction, that is, with the aid of the concept of *entropy.*

The second law of thermodynamics is essentially a qualification of the first law of thermodynamics, which itself is an extension of the principle of the conservation of energy to include heat as well as mechanical energy. The second law does not deny that there must always be an energy balance under all conditions, but it severely restricts the conditions under which heat can be turned into mechanical energy, that is, work. It is, of course, always possible to change work completely into heat; the reverse, however, it is not true, and heat can never be changed completely into work without leaving some kind of compensating change elsewhere in the universe. This means that there are irreversible processes in nature—so that a system left to itself evolves only along certain directions and not along others; certain processes are completely excluded.

The second law defines the directions in which a system may move or the states that it will reach if left to itself; these states are calculated by introducing a function of the state of the system that is called *entropy.* This important quantity, as Boltzmann first pointed out, is a measure of the probability that a system will evolve in a certain way; the higher the entropy of a particular state is, the greater is the likelihood of ultimately finding the system in that state. To say of a system composed of two bodies in contact that the entropy increases when heat flows spontaneously from the hotter to the cooler body, is the same as saying that heat must always flow spontaneously to the cooler body in such a system.

The entropy of a system is similar to the energy of a system in that one cannot give it an absolute value (at least this was the case when the entropy was first introduced). One can only determine the difference between the entropy of a system in some standard or reference state and the entropy in another state. In fact, when the concept of entropy was introduced, it was defined mathematically as the change in the energy that takes place in a system, divided by the absolute temperature at which the change occurs. Since the entropy is thus related to the energy change within a system, Planck felt that he could deal with the energy distribution of the radiation emitted by harmonic oscillators by starting with the entropy of a collection of such oscillators. His belief was reinforced by the work of W. Wien, who at just about the same time had discovered a law for the spectral distribution of the radiation emitted by a black body, which agrees very well with the observations for the high frequencies, or short-wavelength end of the spectrum. Planck was convinced that he could find a universal and simple relationship that would express the most general distribution law, if he could relate the entropy of the radiation to the energy by means of Wien's law.

Planck found, by combining Wien's law with the mathematical expression for entropy, that a certain simple quantity (the R in the text of his talk, which follows) varies directly with the energy, that is, it is a linear function of the energy. Planck at first thought that this was the universal law that he was seeking. But it turned out that Wien's law disagreed with the data for black-body radiation for the long wavelengths. Thus, Planck realized that he would have to look further. He was aided in his search by the experimental work of Rubens and Kurlbaum, which showed that the quantity R for long wavelengths varies as the square of the energy, instead of directly as the energy. A classical formula giving this result for long wavelengths had already been derived by Lord Rayleigh. Planck therefore decided to set up an algebraic formula for R, consisting of a sum of two terms; one term was to depend on the first power of the energy, and the other on the second power with two coefficients that were to be determined. He determined these coefficients by choosing them so that the new formula went smoothly into (that is, became) the Wien formula for the short wavelengths, and the Rayleigh formula for long wavelengths. If the wavelength in Planck's formula is allowed to become very small, the formula approaches the Wien formula. As the wavelength becomes large the Planck formula approaches the Rayleigh formula.

Although the formula he thus obtained agreed with the distribution of the energy in black-body radiation over the entire spectrum, he was not entirely happy with it since, as he said, ". . . even if this radiation formula should prove to be absolutely accurate, it would after all be only an interpolation formula found by happy guesswork and would thus leave one rather unsatisfied." He therefore was concerned from the day of its discovery with the problem of giving it physical meaning.

To do this, Planck started again from the concept of entropy. But this time he related it to the probability that a certain state would occur in a system. In the case of radiation, this was the probability for the distribution of the energy emitted by a black body among the various frequencies of the spectrum. This relationship between probability and entropy had already been treated by Boltzmann, who had formulated the principle that entropy is a measure of the physical probability of finding a system in a given state.

To use this relationship between entropy and probability to obtain the general law of black-body radiation, it was necessary to start with a formula for the absolute entropy. But up to that time, as already noted, only *differences* of entropy were considered to have any meaning. This absolute definition of entropy was now introduced by Planck in such a way that the constant in the usual formula for the entropy of a system goes to zero as the absolute temperature of the system approaches zero. The application of this absolute formula to the determination of the spectral distribution of black-body radiation led to the formula that Planck had previously obtained by his empirical *ad hoc* methods. But there were two constants in the final radiation formula that had to be properly interpreted before the entire procedure could be given physical meaning.

One constant was fairly easy to interpret, since it turned out to be just twice the average kinetic energy of a harmonic oscillator divided by the absolute temperature of the ensemble of oscillators that were in equilibrium with the black-body radiation. This constant, k, which is called the Boltzmann constant (we have seen that Planck felt this to be ironic), plays a very important role in the kinetic theory of gases and is equal to two-thirds the average kinetic energy of molecules in a gas divided by the absolute temperature of the gas. This constant also plays a very important role in the relationship between probability and entropy; it is just the ratio of the entropy of a system in a given state to the logarithm of the probability of the system's being in that state.

The second universal constant that appears in Planck's formula required a good deal more thought on Planck's part before he properly interpreted it. The initial difficulty arose because he attempted to fit this constant into the framework of classical physics and the wave properties of radiation. He soon realized, however, that he could not do this without completely destroying the results of his theory and therefore the agreement that he had found with the observational data. He finally, and quite reluctantly, concluded that this new constant represented a drastic departure from classical physics and would have to be explained in terms of an atomism in radiation that was quite revolutionary.

This universal constant h, known as Planck's constant, has the very small numerical value 6.6×10^{-27}; it has the dimensional properties of an energy multiplied by a time so that it must be related to what is called "action" in classical physics. The importance of this concept for a given system in classical physics lies in the assumption that all systems tend to move along paths for which the total action, as with the planets, is a minimum. Moreover, in classical physics the action is always assumed to vary continuously, representing a continuous change in the system as it moves along its orbit. But the existence of the constant *h* indicated to Planck that in the future one would have to revise one's entire thinking concerning the way in which events occur in nature. Instead of a classical construction based upon the assumption that there exists a "continuity of all causal chains of events," one would have to introduce a quantum description of nature based upon the concept that action itself is atomistic and therefore quantized or discontinuous.

When Planck obtained his quantum formula of radiation, few physicists were prepared to accept its full implications; namely, that the wave picture of electromagnetic radiation as it had been developed by Maxwell was not complete and that it would have to be replaced by a wave-corpuscular picture.

The full significance of the quantum of action *h* is only now apparent when we see that it appears in all atomic, nuclear, and high energy processes. The presence of *h* in Planck's formula distinguishes it from the classical radiation formula and we find in general that all quantum formulae are characterized by the presence of this constant. Just as Planck's formula changes into Rayleigh's classical law as *h* goes to zero, so all atomic formulas become classical formulas as *h* goes to zero. Thus, in a universe in which *h* decreased steadily, ultimately becoming zero, quantum phenomena (the discontinuous structure of radiation, of action, and of energy in general) would disappear and classical science would be valid. From all that we know today, it is clear that the variegated structure of the atom and such stable structures as the electron and the photon are possible because *h* is finite. Thus, if *h* were zero, atoms as we know them could not exist and such things as organic chemistry and life itself would disappear. The importance of the quantum of action *h* for life processes is indicated by the continuity and remarkable stability of the gene. If action were not quantized, even small environmental changes would change the genetic structure, but the existence of a quantum of action means that genes retain their structures unless enough energy, let us say in the form of a high-energy photon, is absorbed to disrupt this structure. Thus genes can only change their structure discontinuously and not gradually.

That *h* is a very small number indicates that the discontinuities in nature are very minute. To the unaided eye, therefore, action (defined by the physicist as the momentum of a body multiplied by the distance it moves with the same momentum), energy, radiation, all appear continuous. Only to the physicist who peers deeply into matter and observes the behavior of the tiny individual components of matter (electrons, atoms, protons, neutrons) are the discontinuities apparent, and the need for a finite *h* and a quantum theory obvious.

The atomicity of action means that the emission and absorption of radiation by matter is discontinuous. Thus radiation of frequency ν can be absorbed or emitted only in bundles (or quanta) $h\nu$. The larger the frequency is (the bluer the light), that is, the shorter the wavelength, the more energy there is concentrated in a bundle. Thus, in the emission and absorption of black-body radiation by oscillators at a given temperature, the very high-frequency oscillators play a very small role because a great deal of energy per quantum is required to excite such an oscillator. Precisely because the roles of the high-energy oscillators are practically eliminated in black-body radiation by the quantum of action does Planck's theory give the correct spectral distribution of black-body radiation.

The last paragraph in Planck's Nobel address indicates his hesitancy in extending his quantum concept beyond what was required to give his radiation formula. He was sure that energy is emitted and absorbed in little packets but was not convinced that the packets are permanent features of radiation.

We shall see, in our commentary on Einstein's theory of the photon, that Einstein answered this and other questions fully. Energy is not only emitted and absorbed in bundles, but these "photons," as Einstein called them, exist as unchanging entities after they have been emitted.

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PLANCK

The Origin and Development of the Quantum Theory [2]

. . . WHEN I RECALL THE DAYS of twenty years ago, when the conception of the physical quantum of "action" was first beginning to disentangle itself from the surrounding mass of available experimental facts, and when I look back upon the long and tortuous road which finally led to its disclosure, this development strikes me at times as a new illustration of Goethe's saying, that "man errs, so long as he is striving." And all the mental effort of an assiduous investigator must in-

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[2] Max Planck, Nobel Prize in Physics Award Address, 1919. By permission of the Nobel Foundation.

deed appear vain and hopeless, if he does not occasionally run across striking facts which form incontrovertible proof of the truth he seeks, and show him that after all he has moved at least one step nearer to his objective. The pursuit of a goal, the brightness of which is undimmed by initial failure, is an indispensable condition, though by no means a guarantee, of final success.

In my own case such a goal has been for many years the solution of the question of the distribution of energy in the normal spectrum of radiant heat. The discovery by Gustav Kirchhoff that the quality of the heat radiation produced in an enclosure surrounded by any emitting or absorbing bodies whatsoever, all at the same temperature, is entirely independent of the nature of such bodies, established the existence of a universal function, which depends only upon the temperature and the wavelength, and is entirely independent of the particular properties of the substance. And the discovery of this remarkable function promised a deeper insight into the relation between energy and temperature, which is the principal problem of thermodynamics and therefore also of the entire field of molecular physics. The only road to this function was to search among all the different bodies occurring in nature, to select one of which the emissive and absorptive powers were known, and to calculate the energy distribution in the heat radiation in equilibrium with that body. This distribution should then, according to Kirchhoff's law, be independent of the nature of the body.

A most suitable body for this purpose seemed H. Hertz's rectilinear oscillator (dipole) whose laws of emission for a given frequency he had just then fully developed. If a number of such oscillators be distributed in an enclosure surrounded by reflecting walls, there would take place, in analogy with sources and resonators in the case of sound, an exchange of energy by means of the emission and reception of electro-magnetic waves, and finally what is known as black-body radiation corresponding to Kirchhoff's law should establish itself in the vacuum-enclosure. I expected, in a way which certainly seems at the present day somewhat naïve, that the laws of classical electrodynamics would suffice, if one adhered sufficiently to generalities and avoided too special hypotheses, to account in the main for the expected phenomena and thus lead to the desired goal. I thus first developed in as general terms as possible the laws of the emission and absorption of a linear resonator, as a matter of fact by a rather circuitous route which might have been avoided had I used the electron theory which had just been put forward by H. A. Lorentz. But as I had not yet complete confidence in that theory I preferred to consider the energy radiating from and into a spherical surface of a suitably large radius drawn around the resonator. In this connexion we need to consider only processes in an absolute vacuum, the knowledge of which, however, is all that is required to draw the necessary conclusions concerning the energy changes of the resonator.

The outcome of this long series of investigations of which some could be tested and were verified by comparison with existing observations, e.g. the measurements of V. Bjerknes on damping, was the establishment of a general relation between the energy of a resonator of a definite free frequency and the energy radiation of the corresponding spectral region in the surrounding field in equilibrium with it. The remarkable result was obtained that this relation is independent of the nature of the resonator, and in particular of its coefficient of damping—a result which was particularly welcome since it introduced the simplification that the energy of the radiation could be replaced by the energy of the resonator so that a simple system of one degree of freedom could be substituted for a complicated system having many degrees of freedom.

But this result constituted only a preparatory advance towards the attack on the main problem, which now towered up in all its imposing height. The first attempt to master it failed: for my original hope that the radiation emitted by the resonator would differ in some characteristic way from the absorbed radiation, and thus afford the possibility of applying a differential equation, by the integration of which a particular condition for the composition of the stationary radiation could be reached, was not realized. The resonator reacted only to those rays which were emitted by itself, and exhibited no trace of resonance to neighboring spectral regions.

Moreover, my suggestion that the resonator might be able to exert a one-sided, i.e. irreversible, action on the energy of the surrounding radiation field called forth the emphatic protest of Ludwig Boltzmann, who with his more mature experience in these questions succeeded in showing that according to the laws of the classical dynamics every one of the processes I was considering could take place in exactly the opposite sense. Thus a spherical wave emitted from a resonator when reversed shrinks in concentric spherical surfaces of continually decreasing size on to the resonator, is absorbed by it, and so permits the resonator to send out again into space the energy formerly absorbed in the direction from which it came. And although I was able to exclude such singular processes as inwardly directed spherical waves by the introduction of a special restriction, to wit the hypothesis of "natural radiation," yet in the course of these investigations it became more and more evident that in the chain of argument an essential link was missing which should lead to the comprehension of the nature of the entire question.

The only way out of the difficulty was to attack the problem from the opposite side, from the standpoint of thermodynamics, a domain in which I felt more at home. And as a matter of fact my previous studies on the second law of thermodynamics served me here in good stead, in that my first impulse was to bring not the temperature but the entropy of the resonator into relation with its energy, more accurately not the entropy itself but its second derivative with respect to the energy, for it is this differential coefficient that has a direct physical significance for the irreversibility of the exchange of energy between the resonator and the radiation. But as I was at that time too much devoted to pure phenomenology to inquire more closely into the relation between entropy and probability, I felt compelled to limit myself to the available experimental results. Now, at that time, in 1899, interest was centred on the law of the distribution of energy, which had not long before been proposed by W. Wien, the experimental verification of which had been undertaken by F. Paschen in Hanover and by O. Lummer and E. Pringsheim of the Reichsanstalt, Charlottenburg. This law expresses the intensity of radiation in terms of the temperature by means of an exponential function. On calculating the relation following from this law between the entropy and energy of a resonator the remarkable result is obtained that the reciprocal value of the above differential coefficient, which I shall here denote by R, is proportional to the energy. This extremely simple relation can be regarded as an adequate expression of Wien's law of the distribution of energy; for with the dependence on the energy that of the wave-length is always directly given by the well-established displacement law of Wien.

Since this whole problem deals with a universal law of nature, and since I was then, as to-day, pervaded with a view that the more general and natural a law is the simpler it is (although the question as to which formulation is to be regarded as the simpler cannot always be definitely and unambiguously decided), I believed for the time that the basis of the law of the distribution of energy could be expressed by the theorem that the value of R is proportional to the energy. But in view of the results of new measurements this conception soon proved untenable. For while Wien's law was completely satisfactory for small values of energy and for short waves, on the one hand it was shown by O. Lummer and E. Pringsheim that considerable deviations were obtained with longer waves, and on the other hand the measurements carried out by H. Rubens and F. Kurlbaum with the infra-red residual rays (*Reststrahlen*) of fluorspar and rock salt disclosed a totally different, but, under certain circumstances, a very simple relation characterized by the proportionality of the value of R not to the energy but to the square of the energy. The longer the waves and the greater the energy the more accurately did this relation hold.

Thus two simple limits were established by direct observation for the function R: for small energies proportionality to the energy, for large energies proportionality to the square of the energy. Nothing therefore seemed simpler than to put in the general case R equal to the sum of a term proportional to the first power and another proportional to the square of the energy, so that the first term is relevant for small energies and the second for large energies; and thus was found a new radiation formula which up to the present has withstood experimental examination fairly satisfactorily. Nevertheless it cannot be regarded as having been experimentally confirmed with final accuracy, and a renewed test would be most desirable.

But even if this radiation formula should prove to be absolutely accurate it would after all be only an interpolation formula found by happy guesswork, and would thus leave one rather unsatisfied. I was, therefore, from the day of its origination, occupied with the task of giving it a real physical meaning, and this question led me, along Boltzmann's line of thought, to the consideration of the relation between entropy and probability; until after some weeks of the most intense work of my life clearness began to dawn upon me, and an unexpected view revealed itself in the distance.

Let me here make a small digression. Entropy, according to Boltzmann, is a measure of a physical probability, and the meaning of the second law of thermodynamics is that the more probable a state is, the more frequently will it occur in nature. Now what one measures are only the differences of entropy, and never entropy itself, and consequently one cannot speak, in a definite way, of the absolute entropy of a state. But nevertheless the introduction of an appropriately defined absolute magnitude of entropy is to be recommended, for the reason that by its help certain general laws can be formulated with great simplicity. As far as I can see the case is here the same as with energy. Energy, too, cannot itself be measured; only its differences can. In fact, the concept used by our predecessors was not energy but work, and even Ernst Mach, who devoted much attention to the law of conservation of energy but at the same time strictly avoided all speculations exceeding the limits of observation, always abstained from speaking of energy itself. Similarly in the early days of thermochemistry one was content to deal with heats of reaction, that is to say again with differences of energy, until Wilhelm Ostwald emphasized that many complicated calculations could be materially shortened if energies instead of calorimetric numbers were used. The additive constant which thus remained undetermined for energy was later finally fixed by the relativistic law of the proportionality between energy and inertia.

As in the case of energy, it is now possible to define an absolute value of entropy, and thus of physical probability, by fixing the additive constant so that together with the energy (or better still, the temperature) the entropy also should vanish. Such considerations led to a comparatively simple method of calculating the physical probability of a given distribution of energy in a system of resonators, which yielded precisely the same expression for entropy as that corresponding to the radiation law; and it gave me particular satisfaction, in compensation for the many disappointments I had encountered, to learn from Ludwig Boltzmann of his interest and entire acquiescence in my new line of reasoning.

To work out these probability considerations the knowledge of two universal constants is required, each of which has an independent meaning, so that the evaluation of these constants from the radiation law could serve as an a posteriori test whether the whole process is merely a mathematical artifice or has a true physical meaning. The first constant is of a somewhat formal nature; it is connected with the definition of temperature. If temperature were defined as the mean kinetic energy of a molecule in a perfect gas, which is a minute energy indeed, this constant would have the value $\frac{2}{3}$. But in the conventional scale of temperature the constant assumes (instead of $\frac{2}{3}$) an extremely small value, which naturally is intimately connected with the energy of a single molecule, so that its accurate determination would lead to the calculation of the mass of a molecule and of associated magnitudes. This constant is frequently termed Boltzmann's constant, although to the best of my knowledge Boltzmann himself never introduced it (an odd circumstance, which no doubt can be explained by the fact that he, as appears from certain of his statements, never believed it would be possible to determine this constant accurately). Nothing can better illustrate the rapid progress of experimental physics within the last twenty years than the fact that during this period not only one, but a host of methods have been discovered by means of which the mass of a single molecule can be measured with almost the same accuracy as that of a planet.

While at the time when I carried out this calculation on the basis of the radiation law an exact test of the value thus obtained was quite impossible, and one could scarcely hope to do more than test the admissibility of its order of magnitude, it was not long before E. Rutherford and H. Geiger succeeded, by means of a direct count of the α-particles, in determining the value of the electrical elementary charge as $4 \cdot 65.10^{-10}$, the agreement of which with my value $4 \cdot 69.10^{-10}$ could be regarded as a decisive confirmation of my theory. Since then further methods have been developed by E. Regener, R. A. Millikan, and others, which have led to a but slightly higher value.

Much less simple than that of the first was the interpretation of the second universal constant of the radiation law, which, as the product of energy and time (amounting on a first calculation to $6 \cdot 55.10^{-27}$ erg. sec.) I called the elementary quantum of action. While this constant was absolutely indispensable to the attainment of a correct expression for entropy —for only with its aid could be determined the magnitude of the "elementary region" or "range" of probability, necessary for the statistical treatment of the problem—it obstinately withstood all attempts at fitting it, in any suitable form, into the frame of the classical theory. So long as it could be regarded as infinitely small, that is to say for large values of energy or long periods of time, all went well; but in the general case a difficulty arose at some point or other, which became the more pronounced the weaker and the more rapid the oscillations. The failure of all attempts to bridge this gap soon placed one before the dilemma: either the quantum of action was only a fictitious magnitude, and, therefore, the entire deduction from the radiation law was illusory and a mere juggling with formulae, or there is at the bottom of this method of deriving the radiation law some true physical concept. If the latter were the case, the quantum would have to play a fundamental rôle in physics, heralding the advent of a new state of things, destined, perhaps, to transform completely our physical concepts which *since* the introduction of the infinitesimal calculus by Leibniz and Newton have been founded upon the assumption of the *continuity* of all *causal* chains of *events.**

Experience has decided for the second alternative. But that the decision should come so soon and so unhesitatingly was due not to the examination of the law of distribution of the energy of heat radiation, still less to my special deduction of this law, but to the steady progress of the work of those investigators who have applied the concept of the quantum of action to their researches.

The first advance in this field was made by A. Einstein, who on the one hand pointed out that the introduction of the quanta of energy associated with the quantum of action seemed capable of explaining readily a series of remarkable properties of light action discovered experimentally, such as Stokes's rule, the emission of electrons, and the ionization of gases, and on the other hand, by the identification of the expression for the energy of a system of resonators with the energy of a solid body, derived a formula for the specific heat of solid bodies which on the whole represented it correctly as a function of temperature, more especially exhibiting its decrease with falling temperature. A number of questions were thus thrown out in different directions, of which the accurate and many-sided investigations yielded in the course of time much valuable material. It is not my task to-day to give an even approximately complete report of the successful work achieved in this field; suffice it to give the most important and characteristic phase of the progress of the new doctrine.

First, as to thermal and chemical processes. With regard to specific heat of solid bodies, Einstein's view, which rests on the assumption of a single free period of the atoms, was extended by M. Born and Th. von Karman to the case which corresponds better to reality, viz. that of several free periods; while P. Debye, by a bold simplification of the assumptions

* Editors' italics.

as to the nature of the free periods, succeeded in developing a comparatively simple formula for the specific heat of solid bodies which excellently represents its values, especially those for low temperatures obtained by W. Nernst and his pupils, and which, moreover, is compatible with the elastic and optical properties of such bodies. But the influence of the quanta asserts itself also in the case of the specific heat of gases. At the very outset it was pointed out by W. Nernst that to the energy quantum of vibration must correspond an energy quantum of rotation, and it was therefore to be expected that the rotational energy of gas molecules would also vanish at low temperatures. This conclusion was confirmed by measurements, due to A. Eucken, of the specific heat of hydrogen; and if the calculations of A. Einstein and O. Stern, P. Ehrenfest, and others have not as yet yielded completely satisfactory agreement, this no doubt is due to our imperfect knowledge of the structure of the hydrogen atom. That "quantized" rotations of gas molecules (i.e. satisfying the quantum condition) do actually occur in nature can no longer be doubted, thanks to the work on absorption bands in the infra-red of N. Bjerrum, E. v. Bahr, H. Rubens and G. Hettner, and others, although a completely exhaustive explanation of their remarkable rotation spectra is still outstanding.

Since all affinity properties of a substance are ultimately determined by its entropy, the quantic calculation of entropy also gives access to all problems of chemical affinity. The absolute value of the entropy of a gas is characterized by Nernst's chemical constant, which was calculated by O. Sackur by a straightforward combinatorial process similar to that applied to the case of the oscillators, while H. Tetrode, holding more closely to experimental data, determined by a consideration of the process of vaporization, the difference of entropy between a substance and its vapour.

While the cases thus far considered have dealt with the states of thermodynamical equilibrium, for which the measurements could yield only statistical averages for large numbers of particles and for comparatively long periods of time, the observation of the collisions of electrons leads directly to the dynamic details of the processes in question. Therefore the determination, carried out by J. Franck and G. Hertz, of the so-called resonance potential or the critical velocity which an electron impinging upon a neutral atom must have in order to cause it to emit a quantum of light, provides a most direct method for the measurement of the quantum of action. Similar methods leading to perfectly consistent results can also be developed for the excitation of the characteristic X-ray radiation discovered by C. G. Barkla, as can be judged from the experiments of D. L. Webster, E. Wagner, and others.

The inverse of the process of producing light quanta by the impact of electrons is the emission of electrons on exposure to light-rays, or X-rays, and here, too, the energy quanta following from the action quantum and the vibration period play a characteristic rôle, as was early recognized from the striking fact that the velocity of the emitted electrons depends not upon the intensity but only on the colour of the impinging light. But quantitatively also the relations to the light quantum, pointed out by Einstein, have proved successful in every direction, as was shown especially by R. A. Millikan, by measurements of the velocities of emission of electrons, while the importance of the light quantum in inducing photochemical reactions was disclosed by E. Warburg.

Although the results I have hitherto quoted from the most diverse chapters of physics, taken in their totality, form an overwhelming proof of the existence of the quantum of action, the quantum hypothesis received its strongest support from the theory of the structure of atoms (Quantum Theory of Spectra) proposed and developed by Niels Bohr. For it was the lot of this theory to find the long-sought key to the gates of the wonderland of spectroscopy which since the discovery of spectrum analysis up to our days had stubbornly refused to yield. And the way once clear, a stream of new knowledge poured in a sudden flood, not only over this entire field but into the adjacent territories of physics and chemistry. Its first brilliant success was the derivation of Balmer's formula for the spectrum series of hydrogen and helium, together with the reduction of the universal constant of Rydberg to known magnitudes; and even the small differences of the Rydberg constant for these two gases appeared as a necessary consequence of the slight wobbling of the massive atomic nucleus (accompanying the motion of electrons around it). As a sequel came the investigation of other series in the visual and especially the X-ray spectrum aided by Ritz's resourceful combination principle, which only now was recognized in its fundamental significance.

But whoever may have still felt inclined, even in the face of this almost overwhelming agreement—all the more convincing, in view of the extreme accuracy of spectroscopic measurements—to believe it to be a coincidence, must have been compelled to give up his last doubt when A. Sommerfeld deduced, by a logical extension of the laws of the distribution of quanta in systems with several degrees of freedom, and by a consideration of the variability of inert mass required by the principle of relativity, that magic formula before which the spectra of both hydrogen and helium revealed the mystery of their "fine structure" as far as this could be disclosed by the most delicate measurements possible up to the present, those of F. Paschen—a success equal to the famous discovery of the planet Neptune, the presence and orbit of which were calculated by Leverrier [and Adams] before man ever set eyes upon it. Progressing along the same road, P. Epstein achieved a complete explanation of the Stark effect of the electrical splitting of spectral lines, P. Debye obtained a simple interpretation of the K-series of the X-ray spectrum investigated by

Manne Siegbahn, and then followed a long series of further researches which illuminated with greater or less success the dark secret of atomic structure.

After all these results, for the complete exposition of which many famous names would here have to be mentioned, there must remain for an observer, who does not choose to pass over the facts, no other conclusion than that the quantum of action, which in every one of the many and most diverse processes has always the same value, namely $6 \cdot 52.10^{-27}$ erg. sec., deserves to be definitely incorporated into the system of the universal physical constants. It must certainly appear a strange coincidence that at just the same time as the idea of general relativity arose and scored its first great successes, nature revealed, precisely in a place where it was the least to be expected, an absolute and strictly unalterable unit, by means of which the amount of action contained in a space-time element can be expressed by a perfectly definite number, and thus is deprived of its former relative character.

Of course the mere introduction of the quantum of action does not yet mean that a true Quantum Theory has been established. Nay, the path which research has yet to cover to reach that goal is perhaps not less long than that from the discovery of the velocity of light by Olaf Römer to the foundation of Maxwell's theory of light. The difficulties which the introduction of the quantum of action into the well-established classical theory has encountered from the outset have already been indicated. They have gradually increased rather than diminished; and although research in its forward march has in the meantime passed over some of them, the remaining gaps in the theory are the more distressing to the conscientious theoretical physicist. In fact, what in Bohr's theory served as the basis of the laws of action consists of certain hypotheses which a generation ago would doubtless have been flatly rejected by every physicist. That with the atom certain quantized orbits [i.e. picked out on the quantum principle] should play a special rôle could well be granted; somewhat less easy to accept is the further assumption that the electrons moving on these curvilinear orbits, and therefore accelerated, radiate no energy. But that the sharply defined frequency of an emitted light quantum should be different from the frequency of the emitting electron would be regarded by a theoretician who had grown up in the classical school as monstrous and almost inconceivable.

But numbers decide, and in consequence the tables have been turned. While originally it was a question of fitting in with as little strain as possible a new and strange element into an existing system which was generally regarded as settled, the intruder, after having won an assured position, now has assumed the offensive; and it now appears certain that it is about to blow up the old system at some point. The only question now is, at what point and to what extent this will happen. If I may express at the present time a conjecture as to the probable outcome of this desperate struggle, everything appears to indicate that out of the classical theory the great principles of thermodynamics will not only maintain intact their central position in the quantum theory, but will perhaps even extend their influence. The significant part played in the origin of the classical thermodynamics by mental experiments is now taken over in the quantum theory by P. Ehrenfest's hypothesis of the adiabatic invariance; and just as the principle introduced by R. Clausius, that any two states of a material system are mutually interconvertible on suitable treatment by reversible processes, formed the basis for the measurement of entropy, just so do the new ideas of Bohr show a way into the midst of the wonderland he has discovered.

There is one particular question the answer to which will, in my opinion, lead to an extensive elucidation of the entire problem. What happens to the energy of a light-quantum after its emission? Does it pass outwards in all directions, according to Huygens's wave theory, continually increasing in volume and tending towards infinite dilution? Or does it, as in Newton's emanation theory, fly like a projectile in one direction only? In the former case the quantum would never again be in a position to concentrate its energy at a spot strongly enough to detach an electron from its atom; while in the latter case it would be necessary to sacrifice the chief triumph of Maxwell's theory—the continuity between the static and the dynamic fields—and with it the classical theory of the interference phenomena which accounted for all their details, both alternatives leading to consequences very disagreeable to the modern theoretical physicist.