Niels Bohr and quantum physics

A. B. Migdal

L. D. Landau Institute of Theoretical Physics, Chernogolovka (Moscow Region)
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The way of thinking and scientific style of Niels Bohr are discussed in connection with developments of his emotional and spiritual life. Analysis of the papers of Bohr, his predecessors, and his contemporaries reveals that he was a philosopher of physics who had an incomparable influence upon the creation and development of quantum mechanics. His struggle against nuclear weapons is mentioned.

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...Talent reaches the goal that
no one else can. Genius reaches
the goal that no one else can see.
A. Schopenhauer

INTRODUCTION

In 1961, when Bohr was in Moscow for the last time, I went to his hotel in order to take him to the Institute of Atomic Energy but at the entrance saw that he was sitting in a vehicle sent from the Academy of Sciences. I dashed to his son Aage, with whom I had already long been acquainted, and started to explain how I had hoped Niels would travel with me. Aage said a few words to his father, and, to the great displeasure of the representatives of the Academy, obediently transferred to my vehicle. Is it necessary to explain why I did this? Is it necessary to explain why, accompanying Bohr to the airport, I took with me my son, for whom at that time the idea of becoming a physicist was just a dream, and gave him Bohr's briefcase to carry to the aircraft? In 1929, traveling to Copenhagen, Paul Ehrenfest said to his Leyden student, the 20-year old Casimir: "You are about to become acquainted with Niels Bohr, and that is the most important thing that can happen in the life of a young physicist."

With Niels Bohr, there passed away an entire epoch that historians of science, following Planck, have come to call the *Sturm und Drang* epoch of quantum theory.

If we speak of an artist, we usually attempt to understand the manner in which he paints, the movement to which he belongs. The same approach can be applied to a scientist. Some theoretical physicists work in the style of Einstein, in the manner of Planck; others, in the style of Landau, Feynman, or, quite differently, Gell-Mann. But of no one can it be said he works in the style of Bohr. Nevertheless, Bohr had a formative influence on several generations of physicists. In 1922, in a letter to Arnold Sommerfeld, he wrote some strange words: "...As a scientist, I have recently frequently felt myself to be very isolated... ." Strange words because they were uttered by a man surrounded by a cluster of talented physicists who all revered him. They all traveled to work at the Institute at Blagdamsvej: Pauli, Heisenberg, Schrödinger, Kramers, Landau, Peierls, Klein, Dirac... . But the cause of Bohr's scientific isolation was that his style was inimitable.

The task of this paper is to attempt to understand the way of thinking and scientific style of Bohr. Werner Heisenberg wrote[1]: "Bohr was a philosopher rather than a physicist, but he knew that in our time natural philosophy acquires strength only after it has been subjected to pitiless testing by experiment." More accurately, one could say that Bohr was a philosopher of physics. It is this consideration that has dictated my selection of works to be discussed. I have left on one side the work of Bohr and his predecessors that, as it seems to me, does not help us to bring out the "musicality in the sphere of thought." The selection is necessarily subjective, but is it possible to do anything if one worries all the time about remaining objective? Most likely Bohr would have said that agitation and objectivity are complementary concepts. The encounter with the great creations of the past makes it possible to get a feeling for the pristine meaning of words that have become devalued in our time—scientific revolution, genius, illumination, spiritual triumph..... .

In biographical papers and books, the main attention is usually concentrated on scientific achievements and not the

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cast of mind of the scientist. A happy exception is Abraham Pais's book on Einstein,[2] though unfortunately it has not yet been translated into Russian. This book follows not only the thoughts of Einstein but carefully analyzes all the preceding events in physics on the background of which the discoveries arose, and shows how the new ideas influenced the subsequent development of science. This paper attempts to go in the same direction. I have used Pais's book not only as an example to follow, but also a source of facts, in particular, details about the meetings and interrelationships between Bohr and Einstein. Finally, I have used the papers and books of Bohr himself, papers and books about him, including the deep and attractive book "Nil's Bor" (Niels Bohr)[3] by Danin, who describes Bohr's life from his birth to his death. Quotations from letters and archive material are given without references; they can be found either in Pais[2] or Danin.[3]

I. "...THE HIGHEST FORM OF MUSICALITY IN THE SPHERE OF THOUGHT..."

These words were said by Einstein of Bohr's paper "On the constitution of atoms and molecules," which was published in 1913 and became a turning point in the scientific biography of Bohr, the point at which he was transformed from a talented young man with promise into a great physicist. It was for this paper that Bohr received the Nobel Prize in 1922—"for investigations into the structure of atoms and their radiation." This work makes it possible to trace the sources and aspects of that physical thought of Bohr which finds its complete reflection in the discussions with Einstein on the physical meaning of quantum theory and the methods of its description.

But first, for the young readers at least, let us recall the events in the spiritual and emotional life of Bohr that preceded the appearance of this paper. In other words, let us discuss what the English would call the "background" of Niels Bohr.

1. Bohr up to 1913

Bohr was exceptionally lucky. He was born into a family that combined in equal measure broad interest in the natural and humanistic sciences, in philosophy. Today, in the century of the specialization of knowledge, it is almost impossible to encounter this combination.

Bohr's mother—Ellen Bohr (Adler)—was, in the words of all who knew her, the embodiment of "tenderness, disinterestedness, and a rare charm." Maybe that from her Bohr inherited his gentleness, which was combined in him with inflexibility. It could be that the example of the mother helped him to find the ideal spouse of the scientist—Margrethe Nørland; this was to be another great success for Bohr.

His father—Christian Bohr—was a well-known physiologist, the author of classical studies on physicochemical processes of breathing. Despite his interest in the physics and chemistry of life, he held teleological views, believing that biological laws are to be understood from the point of view of expediency, and not as the result of the action of physicochemical laws. His papers stimulated lively discussions on one of the great philosophical themes of the time—vitalism and mechanism. Of course, the interest of the father influenced the future interest of Niels Bohr in biology and led him to the thought that the correct understanding of life is possible only on the basis of the idea of the complementarity of physicochemical causality and biological purposefulness. In the opinion of Holton, Bohr, in pondering these things, fulfilled, as it were, a filial duty.

Bohr's spiritual life began with a passion for philosophy. At their home, a frequent guest was the philosopher Harald Höffding, Professor at the Copenhagen University and author of the book *The Psychological Basis of Logical Conclusions*. Here is one of his pronouncements: "The solutions may die, but the problems are still living; otherwise philosophy would not have had so long a life as she has had." Also constantly at the house was the physicist C. Christiansen—to whom Bohr dedicated his paper in 1913—and an eminent philologist, the linguist Vilhelm Thomsen. The quartet of scientists, members of the Danish Academy—Christian Bohr and his guests—regularly met and discussed the most varied subjects, sometimes in the presence of Niels and his brother Harald, who later became a well-known mathematician. Niels Bohr later discussed the influence of these discussions on him and his brother and recounted how they were delighted when they could listen to the discussions of the elders. This helped them to feel a unity in scientific understanding manifested so diversely in the biologist, the philospher, the physicist, and the linguist.

Niels and Harald Bohr were not only brothers but also friends exceptionally close in spirit. When they took part in school, and later, university discussions, all were struck by their "team-work." Eye-witnesses said that the brothers Bohr spoke alternately, continuing each other—the development of their thought seemed to be synchronous. They remained inseparable throughout life.

A book that Niels Bohr read already as a schoolboy—*Tale of a Danish Student* by Poul Møller—made on him such a strong impression that many years later he would offer it as reading to all physicists coming to work with him. The little book explains how a young man begins to think about how he thinks and arrives at the conclusion that any thought must be preceded by another thought and, therefore, the thought must exist already before it appears... . Léon Rosenfeld recalled that Bohr especially marked the places at which the student could no longer speak on behalf of his innumerable personas and gave talks on the impossibility of forming a thought. From these playful and jesting discussions, Bohr led his conversation companions to the thought of the impossibility of unambiguous pronouncements. We find echoes of these ideas in Bohr's interpretation of the interaction between the instrument and the object.

It is possible that this book also forced him, student at the Copenhagen University, to think about the problem of free will and deterministic behavior, a topic that was to occupy him in the future. Such questions also attracted other second year students who attended Höffding's seminars. Twelve of them, including Niels, Harald, and their closest friend, the mathematician Niels Nørland, brother of Margrethe the future wife of Bohr, formed a philosophical circle, in which papers were read in turn. Here there already appeared one of the features of the future method of Bohr's work—he preferred to put forward and develop his ideas in discussion.

In his youthful years he was stimulated by the poetic prose of the Danish philosopher Sören Kierkegaard (1813–1855), during his life hardly known outside Denmark but a figure who became famous in the twenties of this century, when he became the posthumous ideologist of existentialism. According to Kierkegaard, a philosopher must not construct philosophy but live it and become its embodiment in actions. There later came to the surface in Bohr's consciousness the thoughts that he took from the philosophy of Kierkegaard, having rejected its irrationalism.

Here are some pronouncements of Kierkegaard[4]: "...The speculative [philosophers] in our time are stupidly objective. They completely forget that the thinker himself is simultaneously the musical instrument, the flute, on which he plays" (again in accord with the idea of the interaction between the instrument and the object). Rejecting Hegel's idea of the transition of quantity into quality, Kierkegaard says: "A higher quantitative definiteness explains a jump as little as a lower. The new occurs abruptly." He denies the element of continuity preserved in the transition to the new. A new quality, according to Kierkegaard, appears with the abruptness of the mysterious. The jump is alogical, not accessible to rational understanding, does not follow with logical necessity from the preceding state.... .

The youthful impressions, accumulating in the subconscious, prepared the ground on which there could grow that remarkable type of thought that distinguished Niels Bohr.

Bohr's first scientific paper "Determination of the surface-tension of water by the method of jet vibration" was published in 1909.[5] Such a method of determining surface tension was proposed in 1879 by Rayleigh, who constructed the theory of oscillations of a jet for small amplitudes of the oscillations. According to Rayleigh's theory, the surface tension of a liquid can be determined if one knows the velocity, the cross section of the jet, and the length of the waves formed on the surface of the jet. This method makes it possible to study the surface tension of a newly formed jet with a perfectly clean surface.

Already here Bohr showed himself as the future theoretician. He undertook a refinement of Rayleigh's theory, generalizing it to the case of finite amplitudes of the oscillations with allowance for the viscosity of the liquid and the density of air. The paper contains scrupulous calculations—the solution to the equations of the hydrodynamics of a viscous liquid in the form of a series in powers of the oscillation amplitude.

According to the conditions of the competition of the Royal Danish Academy of Sciences and Letters to which it was submitted, the study must be experimental.

For the complete experimental program Bohr did not have sufficient time, and he measured only the surface tension of water. Also submitted to the competition was a study by the Danish physicist P. O. Pedersen, who had measured the surface tension of many liquids. And although Bohr's paper did not strictly satisfy the conditions of the competition, he obtained a gold medal for developing Rayleigh's theory. A gold medal was also awarded to Pedersen.

Bohr's first paper contains apparently more mathematical calculations than all his subsequent papers taken together. We shall encounter this aspect of his papers again.

In 1909, Bohr completed his master's dissertation, which we call a "diploma thesis," a review of the literature on the application of the theory of electrons to metals. This work served as a preparation for the doctoral dissertation (our "candidate's dissertation"), which Bohr defended in 1911.[6] In it, he analyzed and improved the results of J. J. Thomson, Drude, Lorentz, and Abraham. The depth and clarity of Lorentz's studies particularly delighted Bohr.

Having defended the dissertation and received a fellowship of the Carlsberg Foundation for a foreign visit, Bohr traveled in 1911 to Cambridge to work at the Cavendish Laboratory with J. J. Thomson. In 1906, Thomson had received the Nobel Prize for "theoretical and experimental investigations into the electrical conductivity of gases," although, possibly, it should have been awarded to him for the discovery of the electron (1897).

He proposed a model of the atom in which the electrons move in a positively charged cloud. The people around Thomson accepted this model without question.

In the dissertation that Bohr took to Cambridge, hoping to publish it, he rejected, in particular, Thomson's explanation of diamagnetism as due to Larmor motion of the electrons in the magnetic field. Bohr showed that the diamagnetism of electrons in a volume is suppressed by the current that flows around the metal and arises when electrons are reflected from the surface. It appears that Bohr already understood that the magnetic properties of metals cannot be explained in the framework of classical mechanics and electrodynamics.

Thomson was hardly pleased by the criticism that Bohr brought forward at the first encounter. It is hardly surprising that the director of the Cavendish Laboratory, occupied not only by science but also by receptions and administrative work, did not find time to read the dissertation.

Bohr wrote with enthusiasm about Thomson's brilliant lectures to his fiancée Margrethe. He was particularly delighted by his lecture on the motion of the ball for the game of golf: "You cannot imagine what a joy and instruction was this lecture! With what sparkling humor Thomson gave it and what beautiful experiments he showed! He is just to my taste—and I am after all slightly mad about such things... ."

Soon Rutherford came to Cambridge from Manchester. He made a very great impression on Bohr.

At that time, Rutherford was at the peak of his fame and talent. For his investigations on radioactive decay and the chemistry of radioactive substances he had received in 1908 the Nobel Prize for Chemistry, and in 1911 he proposed the planetary model of the atom. Bohr immediately became a supporter of this model. At the invitation of Rutherford, he went to Manchester in March 1912.

The possibility of direct contact with the great experimentalist, working at the frontier of science, was a great good fortune for Bohr. In 1937, writing an obituary of Rutherford, Bohr said[7]: "When I first had the privilege of working under his personal inspiration, he was already a physicist of the greatest renown, but nevertheless he was then, and always remained, open to listen to what a young man might have on his mind."

With Rutherford there were working at this time Hans Geiger, Ernst Marsden, and Georg von Hevesy, who would in 1943 receive the Nobel Prize for the method of tagged atoms. Bohr was in intimate contact with them, and with Hevesy he maintained a long friendship.

Discussions with the colleagues at the laboratory at Manchester directed Bohr to the first thoughts that the serial number in the Periodic Table of the elements is equal to the charge of the nucleus and that this explains the displacement law in radioactive decay.

Soon after, Bohr returned to Denmark. Even before his departure, he began work on the deceleration of charged particles passing through matter on the basis of Rutherford's atom; previously, such calculations had been based on Thomson's model. The paper was published in the Philosophical Magazine in 1913.[8] Bohr returned to this problem once more, in 1915.[9]

Such were the events of the private and scientific life of Niels Bohr up to the commencement of work on his paper "On the constitution of atoms and molecules." We now recall the events in quantum physics that preceded the appearance of this paper and formed Bohr the physicist.

We shall keep returning to these events, particularly in the second part of the paper, when discussing quantum mechanics, in the creation of which the part played by Bohr cannot be overestimated.

2. Black body radiation

Already in the middle of the last century, in 1859, Gustav Kirchhoff established a remarkable law: In thermal equilibrium, the ratio of the emissivity of a body to its absorbtivity is a universal function $K(\nu, T)$. If the coefficient of absorption is equal to unity, the body is said to be "absolutely black." According to Kirchhoff's law, the radiation intensity of a black body depends neither on the matter nor the structure of the body, but only on the frequency and the temperature.

The proof of this law was based on the impossibility of a "perpetual motion machine of the second kind": if the function $K(\nu,T)$ were not universal, one body could go on being cooled forever, heating a different one. The generality of this formula and the unshakable status of its proof could not but agitate theoreticians and experimentalists. Attempts to find the function $K(\nu,T)$ continued up to the beginning of the 20th century.

In 1883, Wilhelm Wien established a "displacement law," according to which the function K has the form

$$K=\nu^3 f\left(\frac{\nu}{T}\right),$$

where f is an as yet unknown function.

In 1886 Wien proposed that $f \propto \exp(-a\nu/T)$ ("Wien's law"). As will become clear later, this proposal is confirmed experimentally only at large $a\nu/T$.

At approximately the same time, the theory of black body radiation began to occupy Max Planck. In his paper in 1900, "On irreversible radiation processes,"[10] he set himself the task of justifying the concept of entropy and temperature for radiation. This paper was a review of previous studies on the application of the Second Law to "phenomena of thermal radiation treated from the point of view of the electromagnetic theory of light." Planck showed that despite the uniqueness of the solutions of Maxwell's equations the introduction of statistical concepts is possible not only for mechanical systems but also in a purely electromagnetic problem.

To carry out this idea, Planck had to find a mechanism that establishes thermal equilibrium of radiation. He therefore introduced oscillators (or resonators) that interact with the radiation and can be regarded as a model of atoms for which the concepts of temperature and entropy can be introduced in a manner already known, so that one can find an expression for the mean energy of an oscillator as a function of the frequency and the temperature, and then, using Eq. (1), determine the dependence of the radiation intensity on these quantities.

Planck obtained an important result: the mean energy $\overline{E}$ of an oscillator of frequency ν in equilibrium with the radiation is proportional to the intensity ρ of the radiation of the same frequency. The intensity of the radiation—the energy of the radiation per unit volume and unit frequency interval—is related to Kirchhoff's function: $\rho = 8\pi K/c$. The mean energy of the oscillator is

$$\overline{E}(\nu, T)=\frac{c^3}{8\pi\nu^2}\rho(\nu, T). \tag{1}$$

According to Planck, this relationship holds for any structure of the oscillator. The oscillator could be a vibrating charge or an electromagnetic resonator; it is only necessary that its damping be weak and determined solely by the interaction with the radiation.

Planck's relation can be made physically clear by using Rayleigh's formula for the number of characteristic electromagnetic oscillations per unit volume per unit frequency interval: $N_\nu = 8\pi\nu^2/c^3$.

The energy of one electromagnetic "oscillator" E_γ is $\overline{E}_\gamma = \rho/N_\nu$, and Planck's equation reduces to equality of the energies of the material and the electromagnetic oscillators—a very natural result. Two systems with identical Hamiltonians in thermal equilibrium with a medium have the same mean energy:

$$\overline{E}=\overline{E}_\gamma. \tag{1'}$$

In commencing these studies, Planck also assumed that Wien's law holds for the complete range of frequencies. He postulated an expression for the entropy S of an oscillator:

$$S=\frac{\overline{E}}{a\nu}\left(1-\ln\frac{\overline{E}}{b\nu}\right). \tag{2}$$

It is readily seen that Wien's law follows from this expression. Indeed, using $1/T = dS/dE$, we find for the mean oscillator energy the expression

$$\overline{E}=b\nu\exp\left(-\frac{a\nu}{T}\right)$$

and in accordance with (1) we obtain Wien's law:

$$\rho=\frac{8\pi b\nu^3}{c^3}\exp\left(-\frac{a\nu}{T}\right). \quad (3)$$

Here it is necessary to stop. How could Planck fail to note, or note but not discuss, the fact that the expression for the mean energy of an oscillator is in monstrous contradiction with classical mechanics and statistical physics? All too well did he know the energy equipartition law, proved 30 years earlier by Maxwell and Boltzmann, according to which the mean energy of an oscillator in thermal equilibrium must be kT!

What explains Planck's silence? The only explanation of this psychological paradox is that Planck found it painfully difficult to arrive at a conclusion that contradicts the laws of 19th century physics, which seemed unshakable. He kept on hoping that it would be possible to find a painless way of reconciling his results with classical physics.

In June 1900, Rayleigh published a paper[11] in which the laws of statistical physics were applied directly to radiation. Rayleigh immediately found that the radiation intensity must, by virtue of the equipartition law, have the form $\rho = c_1\nu^2 T$. In order to satisfy Wien's law at large values of ν/T, Rayleigh proposed the relation (which satisfies the displacement law)

$$\rho=c_1\nu^2 T\exp\left(-\frac{c_2\nu}{T}\right).$$

Experimentalists close to Planck—Heinrich Rubens, Ferdinand Kurlbaum, and also Otto Lummer and Peter Pringsheim—knew Rayleigh's relation and already in 1900 tested this formula.

Rayleigh's relation is an interpolation formula which describes the two limiting cases of small and large ν/T. Naturally, in the intermediate region it contradicts experiment, as is always the case with interpolations. But now we come to a remarkable exception to this rule.

We trace Planck's steps as he arrived for the first time at his famous formula for the intensity of black body radiation. It must be almost a unique example in the history of physics in which an expression valid in the complete range of the variables has been found from the two limiting cases, i.e., when an exact relation has been guessed by means of an interpolation procedure.

Only by a deep understanding of thermodynamics (Planck's teachers were Kirchhoff and Helmholtz) can one explain the idea of using for interpolation, not the expression for the radiation intensity, as Rayleigh had done, but a more natural, as it seemed, quantity—the oscillator entropy. But let us begin from the beginning.

In 1938, Planck, then 80, recalled that his formula was discovered on Sunday, October 7th, 1900. During the day, the Plancks had visitors, the Rubenses, and Heinrich Rubens told Planck that at small ν/T the experiments gave a proportionality of the intensity ρ to the temperature. That same evening, Planck obtained an expression for ρ that at small ν/T gives a proportionality to the temperature but at large ν/T goes over into Wien's formula. Thus was guessed—not derived—the law of the intensity distribution with respect to the frequencies—Planck's formula. Here it is in modern notation:

$$\rho=\frac{8\pi\nu^3 h}{c^3}\,\frac{1}{e^{h\nu/kT}-1}. \quad (4)$$

On October 19th, 1900, Planck gave a paper at a session of the German Physical Society, "On an improvement of Wien's radiation law,"[12a] in which he explained the procedure by means of which he obtained his formula. Planck suggested that one should use the following connection between the entropy of an oscillator and its mean energy:

$$\frac{d^2S}{d\overline{E}^2}=\frac{\alpha}{\overline{E}\,(\overline{E}+\beta)}.$$

This expression was constructed in order to yield the correct dependence of $d^2S/d\overline{E}^2$ on $\overline{E}$ in the two well-known limiting cases of small and large $\overline{E}$.

Let us attempt to reconstruct the line of Planck's arguments. The quantity $d^2S/d\overline{E}^2$ has a simple physical meaning. It determines ε^2, the mean square fluctuation of the oscillator energy:

$$\left(\frac{d^2S}{d\overline{E}^2}\right)^{-1}\sim\overline{(E-\overline{E})^2}=\overline{E^2}-\overline{E}^2\equiv\varepsilon^2.$$

At small $\overline{E}$, corresponding to Wien's formula, we have from (2)

$$\frac{d^2S}{d\overline{E}^2}=-\frac{1}{a\nu\overline{E}},\qquad \varepsilon^2\sim\overline{E}.$$

At large $\overline{E}$, the equipartition law holds, $\overline{E}=kT$, whence it follows that

$$\frac{dS}{d\overline{E}}=\frac{1}{T}=\frac{k}{\overline{E}},\quad \frac{d^2S}{d\overline{E}^2}=-\frac{k}{\overline{E}^2}.$$

Therefore, in this case $\varepsilon^2\sim\overline{E}^2$. The simplest way of describing by one formula both limiting cases of small and large $\overline{E}$, is to take ε^2 of the form $\overline{E}+\beta\overline{E}^2$, and this corresponds to the expression for $d^2S/d^2\overline{E}^2$ used by Planck. From this it can also be seen that ε^2 is the most natural and simple object for the interpolation.

The coefficients α and β can be determined from the limiting cases: At small $\overline{E}$, $d^2S/d\overline{E}^2=\alpha/\beta\overline{E}=-1/a\nu\overline{E}$, while $d^2S/d\overline{E}^2=\alpha/\overline{E}^2=-k/\overline{E}^2$ at large $\overline{E}$; therefore, $\alpha=-k$ and $\beta=a\nu k=h\nu(a=h/k)$.

Thus, Planck's expression is the interpolation formula

$$\frac{d^2S}{d\overline{E}^2}=-\frac{k}{\overline{E}\,(\overline{E}+h\nu)},$$

which gives the correct result at both small and large values of $\overline{E}$ and is in a remarkable manner true at all $\overline{E}$.

From this expression, using the relation $dS/d\overline{E}=1/T$, it is easy to obtain the mean oscillator energy $\overline{E}(\nu,T)$, and from it, by means of formula (1) the expression (4). We note that Planck did not introduce the notation used in this exposition on October 19 but sometime later, at the meeting of the German Physical Society on December 14, 1900, when for the first time he called $h\nu$ an "element of energy" and h a "universal constant." Much the same development was presented by Planck in his 1943 paper "On the history of the discovery of the quantum of action."

In his talk on October 19, 1900, Planck said that his choice of the expression for $d^2S/d\overline{E}^2$ was preceded by the construction of "completely arbitrary equations for the entropy," from which the simplest was chosen.

These words of Planck have led some historians of physics to the thought that Planck's derivation contained something more than an interpolation. They prefer to call Planck's procedure a "free construction." But it was Rubens's telling him of the proportionality of the radiation intensity to the temperature at long wavelengths that brought to life Planck's paper. And, in addition, all his "arbitrary expressions" satisfied from the very beginning the requirements at large $\overline{E}$ that followed from the equipartition law ($\overline{E} = kT$) well known to Planck.

We note that from the methodological point of view the "interpolation" amounts to a double application of the correspondence principle (in our case, at small and large $\overline{E}$) and the use of the "simplicity" requirement (choice of the simplest interpolation).

Planck's formula (4) was confirmed experimentally at all frequency and temperature ranges known at that time.

Comparison with experiment made it possible to determine not only Planck's constant h but also Boltzmann's constant k. From this there followed a new determination of Avogadro's number $N = R/k$, where R is the gas constant. Further, from the Faraday number F Planck found the electron charge $e = F/N$. The value found by Planck ($e = 4.69 \cdot 10^{-10}$ cgs) is close to the currently adopted value ($e = 4.803 \cdot 10^{-10}$ cgs).

It is assuming that the value that Planck found was regarded by some physicists at that time as a shortcoming of the theory, since it contradicted the result of J. J. Thomson that was accepted at that time ($e = 6.5 \cdot 10^{-10}$ cgs).

Pais notes[2]: "Even if Planck had stopped after October 19, he would forever be remembered as the discoverer of the radiation law. It is a true measure of his greatness that he went further. He wanted to interpret formula (4). That made him the discoverer of the quantum theory." In 1931, Planck said that this was "an act of desperation... . I had to obtain a positive result, come what may, at any price... ."

In fact, there was no derivation, and the reason for the success became clear only after Einstein had advanced his hypothesis of light quanta. I give Planck's derivation, hardly deviating from the original.[12]

Let there be N resonators (oscillators) of frequency ν, N' of frequency ν', and so forth. The problem is to find the distribution of energy between the individual resonators in a group of resonators of frequency ν. Let the energy E_N of this group of resonators consist of an exact number of equal parts ε. The numper P of such energy elements is E_N/ε. We count up the number of combinations by which it is possible to distribute these P elements over the N resonators. Here is one of the possible combinations for $N = 10$ and $P = 100$:

$$\frac{1 \quad 2 \quad 3 \quad 4 \quad 5 \quad 6 \quad 7 \quad 8 \quad 9 \quad 10}{7 \quad 38 \quad 11 \quad 0 \quad 9 \quad 2 \quad 20 \quad 4 \quad 4 \quad 5}.$$

We shall assume that two combinations are distinguishable if the corresponding series contain the identical numbers but in different order. The theory of permutations gives for the number W of possible combinations the expression

$$W = \frac{N(N+1)(N+2)\ldots(N+P-1)}{1,\,2,\,3\ldots P} = \frac{(N+P-1)!}{P!\,(N-1)!}$$

By means of Stirling's formula, we obtain approximately $W \approx (N+P)^{N+P}/N^N P^P$. Determining the total entropy of the resonators as $S_N = k \ln W$, we find

$$S_N = NS = kN\left[\left(1+\frac{\overline{E}}{\varepsilon}\right)\ln\left(1+\frac{\overline{E}}{\varepsilon}\right) - \frac{\overline{E}}{\varepsilon}\ln\frac{\overline{E}}{\varepsilon}\right],$$

where $\overline{E} = E_N/N$ is the energy and $S = S_N/N$ the entropy of one oscillator. Using the relation $dS/d\overline{E} = 1/T$ and (1) and setting $\varepsilon = h\nu$, we obtain Planck's formula.

It is remarkable that Planck continues to say nothing directly about his main discovery, namely, that the frequency distribution of the intensity of black-body radiation can be explained only by assuming that the energy of an oscillator of frequency ν is an integer multiple of $\varepsilon = h\nu$. The oscillators can take only discrete values of the energy, $E_n = E_0 = nh\nu$. Planck—a convinced supporter of classical physics—made the great revolution against his own conviction!

From the point of view of the physics of that time, this derivation of Planck's formula could not sustain serious criticism. Above all, as statistical object it considers energy elements, to which are ascribed, as it were, a particle sense. Further, since the work of Boltzmann, it had become clear that statistics can be applied only to quantities for which there is a "stirring" mechanism. Unconvincing too is the use of the connection between the energy and the radiation intensity obtained classically while the basis of the derivation is the assumption of integral portions of energy of each oscillator, in categorical contradiction to classical mechanics. And, finally, systematic application of the laws of statistical physics would immediately lead to an undesirable result: The energy of each oscillator would be kT, and for the radiation there would occur what later became known as the "Rayleigh-Jeans catastrophe," or "ultraviolet catastrophe."

But at the same time it is precisely the shortcomings of this derivation that carry the imprint of genius—we now know that the identical energy elements are photons and the division of the number of permutations by $P!$ (identity of photons) corresponds to what is now called Bose-Einstein statistics.

Many years later, analyzing Planck's proof, Einstein wrote: "The imperfections [of this derivation; A. M.] were originally not noticed, and this was unusual good luck for the development of physics."

In 1918, Planck would receive the Nobel Prize "for services to the development of physics brought about by his discovery of the quantum of energy."

In a paper in 1906,[13] "On the theory of the generation and absorption of light," Einstein gave a deep analysis of the Planck derivation of the formula for black-body radiation and concluded: "In my opinion, the results presented above can in no way refute Planck's theory of radiation; on the contrary, they show that in his radiation theory Planck introduced into physics a new hypothetical element—the hypothesis of light quanta."

The next step in the development of quantum physics was Einstein's paper of 1907: "Planck's theory of radiation and the theory of specific heat."[14] Einstein obtained an expression for the mean energy of an oscillator as follows:

$$\overline{E} = \frac{\int e^{-E/kT} E\omega(E)\,dE}{\int e^{-E/kT}\omega(E)\,dE}.$$

For a classical oscillator, the weight function is $\omega(E) = \text{const}$, from which we obtain $\bar{E} = kT$. But if the oscillator can take on only discrete energy values, multiples of $h\nu$, then $\omega(E)$ has sharp maxima at these discrete values. As we would now say, $\omega(E) = \sum_n \delta(E - E_n)$, $E_n = nh\nu$. Substituting in $\bar{E}$ and summing the simple series in the numerator and the denominator, we obtain

$$\bar{E} = \frac{h\nu}{e^{h\nu/kT} - 1}, \qquad (5)$$

from which, using the relation (1), we find Planck's formula.

Einstein then used the expression (5) to determine the specific heat of a solid, making the assumption that all atoms vibrate at the same frequency. His task was to explain the deviations from the law of Dulong and Petit at low temperatures. As we now know, Einstein's assumption is too schematic, and his expression gives an exponential decrease of the specific heat with decreasing temperature. But the important thing for us is the fact of the application of Planck's quantization idea to the vibrations of a solid. Later, Debye,[15] using the expression (5) and introducing a distribution of the elastic vibrations with respect to the frequencies, obtained for the specific heat c at low temperatures $c \approx T^3$.

3. The light quanta hypothesis

Einstein's 1905 paper "On a heuristic point of view concerning the generation and transformation of light"[16] turns the next important page in the history of quantum physics. In the introduction to this paper, Einstein says: "...It could be that the theory of light, operating with continuous spatial functions, leads to a contradiction with experiment when it is applied to phenomena involving the generation and transformation of light." An later he says: "...According to the suggestion made here, the energy of a beam of light emitted from a point is not distributed continuously in an ever increasing volume but is made up of a finite number of spatially localized indivisible energy quanta that are absorbed or generated only as wholes.

Below I shall outline the thoughts and facts that guide me in this direction in the hope that the point of view put forward here may help other investigators in their researches too."

Einstein begins the paper with an analysis of the difficulties of the theory of thermal radiation. He applies to Planck's oscillators the energy equipartition theorem and finds what is today known as the "Rayleigh-Jeans catastrophe."

Further, he finds the radiation entropy density, which corresponds to Wien's empirical law, valid at large ν/T. If Wien's law is written in the form

$$\rho = b\nu^3 e^{-a\nu/T},$$

then for the entropy density φ of monochromatic radiation of frequency ν we obtain

$$\frac{d\varphi}{d\rho} = \frac{1}{T} = -\frac{1}{a\nu} \ln \frac{\rho}{b\nu^3},$$

whence

$$\varphi(\rho, \nu) = -\frac{\rho}{a\nu} \ln \left(\frac{\rho}{b\nu^3} - 1\right).$$

Suppose the radiation is in volume v. Then the entropy is

$$S = v\varphi\Delta\nu = -\frac{E}{a\nu} \ln \left(\frac{E}{vb\nu^3\Delta\nu} - 1\right), \quad E = \rho v \Delta\nu.$$

Denoting by S_0 the entropy of the radiation in volume v_0, we obtain

$$S - S_0 = \frac{E}{a\nu} \ln \frac{v}{v_0}.$$

Setting $a = h/k$ and denoting $E/h\nu = n$, we obtain analogous expression for an ideal gas if it is assumed that n is an integer.

Einstein concluded from this that radiation at high frequencies and when its density is low behaves as a gas of independent particles with energy $h\nu$.

It is important that in this analysis Einstein does not use the expression (1) obtained by Planck but determines the radiation entropy phenomenologically on the basis of Wien's experimental law.

So far, the analogy between the fluctuations in the density for radiation and for a gas of molecules merely looks like an interesting and unexpected fact. But then comes the following step, which makes this paper truly revolutionary.

Einstein writes: "But if monochromatic radiation (of sufficiently low density) behaves as a discrete medium in the sense of the volume dependence of the entropy, consisting of energy quanta of magnitude $h\nu$, one is led to ask this question: Are the laws of generation and transformation of light such as would be if light consists of such energy quanta?" Thus, Einstein extended Planck's idea of quantizing the oscillators to the electromagnetic radiation. From this point of view, the Planck oscillator changes its energy by emitting or absorbing a corresponding light quantum.

Einstein applied the idea of light quanta in the first place to the theory of the photoelectric effect.

This effect was first discovered by Hertz in 1887 in an investigation of the propagation of electromagnetic waves from a radiating to an absorbing resonator. When Hertz covered the receiving resonator with a screen, in order to see better the jumping spark, he found that the screen affects the conditions of formation of the discharge and that the reason for this is the light from the spark of the emitter. He investigated this phenomenon, and showed that when the screen is illuminated by the light of an electric arc the ionization of the air behind the screen is increased, and the spark jumps at a lower voltage.

The photoelectric effect, like radioactivity and x rays, was discovered by chance. But the history of science shows that such "chances" almost always happened to experimentalists of the first class.

In 1888, A. G. Stoletov investigated the photoelectric effect in more detail and established that illumination of a metal plate gives rise to a flux of negative particles, the magnitude of the electric current being proportional to the radiation intensity.

Later, the photoelectric effect was studied by many authors. A detailed investigation was begun in 1902 by Phillip Lenard. In 1905, he received the Nobel Prize "for the investigation of cathode rays." He established the remarkable fact that the energy of the electrons emitted in the photoelectric effect is completely independent of the intensity of the incident light.

This was the same Lenard that later acquired notoriety by becoming the official head of physics in Hitler Germany and leading the struggle against "Jewish physics," in particular the theory of relativity.

From the hypothesis of light quanta, Einstein obtained for the photoelectric effect

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

where E_{max} is the maximal energy of the emitted electrons, and P is the work function, the energy required to remove an electron from matter.

This expression can also be written in the form $E_2 = E_1 = h\nu$, where $E_2 - E_1$ is the change in the energy of the electron when the quantum is absorbed. But in such a form, this relation is already extremely close to the condition on the frequencies that in 1913 was postulated by Bohr. And, of course, it helped Bohr to arrive at his postulate. In his book *Lectures on Atomic Mechanics*,[17] Max Born writes: "After Bohr had demonstrated the great fruitfulness of this equation for the example of the hydrogen atom, it became known as the Bohr frequency condition."

Thus, Einstein's formula predicted that the function $E_{max}(\nu)$ is a straight line with a slope that does not depend on the material but is determined by Planck's constant as found from the intensity distribution of black-body radiation.

A detailed experimental confirmation of these relations was made only in 1915 by Robert Millikan. In the same year, William Duane and Franklin Hunt found one further confirmation by studying x rays. The upper limit of the frequency of x rays is determined by the relation $h\nu_m = eV$, where V is the potential in which the electrons have been accelerated. This relation was predicted by Einstein in his 1906 paper.[13]

In his 1909 paper "On the present state of the radiation problem,"[18] Einstein returned once more to the properties of radiation and obtained a formula for the fluctuations of the energy of equilibrium radiation.

The derivation of this formula is based on a simple expression that Einstein obtained in his paper "On the general molecular theory of heat."[19] From the expression for the mean energy obtained earlier (at the end of Sec. I.2), it is easy to obtain by differentiation with respect to $\beta = 1/kT$ the relation

$$\varepsilon^2 = \overline{E^2} - \overline{E}^2 = -\frac{\partial \overline{E}}{\partial \beta} = kT^2 \frac{\partial \overline{E}}{\partial T}.$$

For equilibrium radiation $\overline{E} = \rho v \Delta\nu$, where ρ is the energy density of the radiation per unit frequency interval, and v is the volume under consideration. Using for ρ the Planck expression (4), we obtain

$$\varepsilon^2 = \overline{E} h\nu + \frac{c^3 \overline{E}^2}{8\pi\nu^2 \Delta\nu v}. \tag{6}$$

This relation acquires a particularly simple form if we consider the fluctuations of the energy E_γ of one electromagnetic oscillation. The mean energy of the oscillation is $E_\gamma = \overline{E}c^3/8\pi\nu^2 v\Delta\nu$. Instead of (6), we obtain

$$\varepsilon_\gamma^2 = \overline{E_\gamma^2} - \overline{E}_\gamma^2 = \overline{E}_\gamma h\nu + \overline{E}_\gamma^2. \tag{6'}$$

It was just such an expression that Planck used in his interpolation derivation of the expression (4) (with $\overline{E}_\gamma$ replaced by $\overline{E}$). The expression (6') is valid for any oscillator in thermal equilibrium (systems with identical Hamiltonians fluctuate in the same manner).

At small values of $h\nu/kT$, when Planck's formula goes over into the Rayleigh-Jeans law (classical limit), there remains only the second term, which represents the fluctuation in the energy density of the electromagnetic waves produced by interference. At low radiation density, when Planck's formula goes over into Wien's law, there remains only the first term, which must be interpreted as representing the fluctuations in the energy due to the corpuscular structure of light.

Indeed, dividing the left- and right-hand sides of the expression (6) by $(h\nu)^2$ and setting $\overline{E} = \bar{n}h\nu$, we obtain an expression for the fluctuations in the number of quanta:

$$\overline{(n-\bar{n})^2} = \bar{n} + \frac{c^3}{8\pi\nu^2\Delta\nu v}\bar{n}^2, \quad \overline{(n_\gamma - \bar{n}_\gamma)^2} = \bar{n}_\gamma + \bar{n}_\gamma^2.$$

The first term does indeed have the form of the fluctuations for a classical gas. It is remarkable how deeply the Planck distribution is related to the corpuscular structure of light!

The method employed by Einstein in treating fluctuations in the momentum density of equilibrium radiation is instructive. It involves introducing into the radiation a mirror that can move in the direction perpendicular to its plane and transmits all waves except those that lie in the interval, $\nu, \nu + \Delta\nu$, which are completely reflected. The laws of statistical equilibrium are applied to the mirror. One then obtains an expression analogous to (6), also consisting of two terms, one of which has a wave nature and corresponds to fluctuations in the light pressure of a system of waves, while the other must be understood as the fluctuations of the momentum transferred to the mirror by the quanta.

In the paper "On the development of my views on the essence and structure of radiation"[20] (paper given at a conference in Salzburg in 1909), Einstein said: "...It is my opinion that the next phase in the development of theoretical physics will bring us a theory of light that can be interpreted as a kind of fusion of the wave and the emission theory." In the word *fusion*, Pais sees the first hint of the idea of complementarity. It seems to me that Einstein's idea was too general and did not have that constructive value that the principle of complementarity does.

With astounding depth and simplicity, Einstein showed further that by considering the motion of the same mirror but without taking into account the fluctuations in the momenta of the quanta it is not possible to obtain thermal equilibrium between the molecules of a gas and radiation. Here, for the first time, albeit implicity, the momentum of a quantum is introduced.

Pondering these papers, we cannot but note a certain impoverishment of theoretical physics that occured as its methodological formalism was enriched. Contemporary papers, except the innovative ones, are based less and less on qualitative considerations. They no longer contain what Einstein's called "musicality in the sphere of thought." No one will now use a piece of mirror to obtain new relations or a "grain of dust," which it would be sufficient to introduce into a radiation field in order to bring it into equilibrium, or other thought experiments. Theoretical techniques become ever more formalized, the style of papers considered in bulk loses its sparkle, like the mass production of a jeweler... .

Einstein's investigations into the light quantum hypothesis ended in 1916 with the paper "On the quantum theory of radiation"[21] (the same paper was also published in 1917[21]). This paper considers equilibrium between molecules and radiation. Einstein introduces the probabilities of stimulated emission and absorption, and also the probability of spontaneous emission. Using the principle of detailed balance, he obtained in a remarkably simple way Planck's formula. We recall this derivation.

In accordance with the principle of detailed balance,

$$p_n e^{-\varepsilon_n/kT} B_n^m \rho = p_m e^{-\varepsilon_m/kT} (B_m^n \rho + A_m^n);$$

where n and m are two states of a molecule, p_n and p_m are the statistical weights of the states, B_n^m and B_m^n are the probabilities of stimulated absorption and emission, and A_m^n is the probability of spontaneous emission. If there is an increase in T, then ρ increases unboundedly, and therefore $p_n B_n^m = p_m B_m^n$. Hence we find

$$\rho = \frac{A_m^n / B_m^n}{e^{(\varepsilon_m - \varepsilon_n)/kT} - 1}.$$

Using Wien's law, we find $A_m^n = (8\pi h\nu^3/c^3) B_m^n$, $\varepsilon_m - \varepsilon_n = h\nu$, from which Planck's formula follows.

Then, investigating the equilibrium between molecules and radiation, Einstein concluded that a molecule, absorbing or emitting a quantum of energy $h\nu$, acquires or gives up a momentum equal to $h\nu/c$.

Pais correctly finds it strange that the father of special relativity wrote down the formula $\varepsilon = h\nu$ side by side with $p = h\nu c$ only 11 years after the introduction of light quanta. Incidentally, a light quantum does not necessarily have a definite momentum. For example, for the problem of internal conversion of γ rays it is more convenient to use quanta in the spherical representation, in which the momentum of a quantum is indefinite and on the average equal to zero, while the angular momentum has a definite value.

The most important thing in this paper of Einstein was the introduction of probability for the description of microscopic objects. Besides the probabilities of spontaneous and stimulated emission, it is also necessary to assume a random direction of the emission of a quantum from a molecule—the direction of emission cannot be predicted.

A probability of spontaneous emission was introduced for the first time by Rutherford in 1900, when he wrote down the equation for radioactive decay.

Who decides at what moment and in what direction a particle is emitted? To the end of his days, Einstein regarded the probability description as a shortcoming of the theory.

Even after the experiments of Millikan and Duane and Hunt the light quantum hypothesis did not inspire confidence among physicists. In 1913, Planck, Nernst, Rubens, and Warburg proposed Einstein for membership of the Prussian Academy of Sciences. In his book, Pais gives the conclusion of their recommendation: "In sum, one can say that there is hardly one among the great problems in which modern physics is so rich to which Einstein has not made a remarkable contribution. That he may sometimes have missed the target in his speculations, as, for example, in his hypothesis of light-quanta, cannot really be held too much against him, for it is not possible to introduce really new ideas even in the most exact sciences without sometimes taking a risk."

At the Solvay congress in 1911, Einstein emphasized the preliminary nature of the hypothesis of light quanta, since it was difficult to reconcile this idea with well-tested consequences of the wave theory. This remark, dictated by scientific conscientiousness, was taken by many as a retreat. In 1916, Millikan said: "Einstein himself, I believe, no longer holds to it." In 1918, Rutherford, commenting on the Duane-Hunt result, wrote: "Up to the present day, no reasonable physical explanation of this remarkable relationship between energy and frequency has been found."

The negative attitude of physicists to the light-quantum hypothesis is also reflected in the formulation of the Nobel committee. Einstein was awarded the Nobel Prize in 1921 (it was handed to him in 1922 "...for his services to theoretical physics and especially for his discovery of the law of the photoelectric effect." Of the discovery of quanta of the electromagnetic field—not a word.

What explains this stubborn hostility to the hypothesis? Pais gives two reasons. One—which is obvious—the impossibility at that time of reconciling the hypothesis of quanta with the electromagnetic theory of light. The second is related to the fact that, in contrast to the discoveries of Planck and Bohr, this hypothesis did not lead to such detailed and exact predictions.

But it is still difficult to understand such prejudice. For if statistical physics is applied to radiation (Rayleigh, 1900[11]), it is also natural to extend the idea of quantizing a material oscillator to "oscillators" corresponding to standing or traveling waves of the electromagnetic field in a box. This generalization was made by Ehrenfast (1906) and Debye (1910). They obtained Planck's formula by extending the idea that the possible energy values are discrete to the electromagnetic degrees of freedom. But for some reason this discreteness was not directly related to the problem of wave-corpuscle dualism, although, possibly, Einstein sensed this connection and therefore never deviated from the light-quantum hypothesis.

It was only in 1923–1924, after the investigation of the Compton effect, that quanta ceased to be hypothetical particles. The word *photon* was introduced by Gilbert Lewis in 1926 in a paper in which he considered the light photon as some indivisible atom. His ideas were rapidly forgotten, but the new word almost immediately became part of the language. In October 1927 the Fifth Solvay Congress took place; it was devoted to "electrons and photons." The photon became and elementary particle as good as any other, with spin 1 and mass equal to zero.

4. "On the constitution of atoms and molecules"

By his experiments on the scattering of α particles in 1911, Rutherford demonstrated the inescapability of the planetary model of the atom. This date can be regarded as marking the beginning of the nuclear age. The significance of this discovery for the theoretical physics of that time was later very accurately characterized by Bohr[1]: "The decisive point in the atomic model of Rutherford was the complete clarity with which it demonstrated the impossibility of explaining the stability of atoms on the basis of classical physics and that the quantum postulate is the only possible way out of an acute dilemma. It was the acuteness of the incompatibility that forced me to believe absolutely in the correctness of the quantum postulate."

Bohr arrived in Manchester in the spring of 1912, when the entire Rutherford Laboratory was struggling to clarify the advantages and shortcomings of this model. As we have already said, Bohr immediately became a supporter of the model. For all that, many years later, in June 1922, he would say to the youthful Heisenberg: "I never took the planetary model literally..."

At the end of 1912, Bohr, returning to Denmark, left with Rutherford a "Memorandum," which has only partly been preserved in the archives. There appears in it for the first time the idea of stable orbits—the suspicion that there are nonclassical laws in the microscopic world and that the electron orbits are related to the structure of the Periodic Table of the Elements. As answer he received the exhortation "not to hurry," very untypical of Rutherford, who himself attacked things with colossal energy and carried a piece of work through to the end, never stopping half-way. Rutherford assumed that no one would occupy themselves with these problems.

Bohr soon discovered how much Rutherford erred. In the journal *Monthly Notices of the Royal Astronomical Society* of Great Britain, several papers were published by the Cambridge astrophysicist J. Nicholson devoted to the theoretical interpretation of the spectral radiation of stars.[22] Nicholson extended Planck's idea to atoms, making the assumption that the angular momentum of an electron (or rather its projection) is quantized: $M = nh/2\pi$, where n is an integer. This gave rise to an atom with discrete orbits, on each of which groups of electrons gyrated. Nicholson assumed, as was natural at that time, that the electrons radiate electromagnetic waves with frequency equal to the gyration frequency. We recall that Planck's oscillators had discrete values of the energy but emitted light with frequency equal to the frequency of the classical oscillations of the oscillator. Such an assumption was approximately correct for highly excited atoms, and Nicholson explained many features in the radiation of stars and nebulae.

The event that for Bohr proved to be the final push occurred at the beginning of February 1913. By pure chance, he met a friend from his student days, Hans Hansen, a specialist on spectroscopy. When Bohr explained to him his ideas about the structure of matter based on a planetary atom with orbits stable for unknown reasons, Hansen asked: "But how does your theory explain the spectral formulas?" And here, to his huge amazement, he discovered that Bohr knew nothing about the spectral formulas obtained by Balmer (1885), Rydberg (1890), and Ritz (1908)... .

Bohr recalled: "As soon as I saw Balmer's formula, everything immediately became clear to me." This was a flash that illuminated the entire picture—in less than a month the first, most important part of the paper "On the constitution of atoms and molecules,"[23] was ready.

Bohr's paper begins with general physical considerations. Then follows the derivation of the spectra with Rydberg's constant from dimensional arguments, and, finally, a theory in which a coefficient, undetermined in the estimates, is found from the correspondence principle.

What is important for us is that in this paper Bohr gave us the heuristic arguments that helped him to obtain his result. Unfortunately, in modern papers this is not the case.

The paper exhibits a characteristic feature of Bohr's proofs: None of the arguments he gives is, taken by itself, conclusive, but taken together the arguments create a convincing picture.

In the Introduction, Bohr comments that Rutherford's classical atom does not have a radius, i.e., does not contain a quantity with the dimensions of length. A radius appears only after the introduction of Planck's constant.

Bohr begins the first part of the paper, which is called "Binding of electrons by positive nuclei," with the classical expression for the frequency ν_0 of gyration of an electron in an elliptical orbit and for the semimajor axis a of the orbit. In modern notation, these relations have the form

$$\nu_0 = \frac{\sqrt{2}}{\pi}\,\frac{W^{3/2}}{Ze^2\sqrt{m}}, \qquad a = \frac{Ze^2}{2W}; \tag{7}$$

where W is the energy required to strip the electron from the given orbit.

Bohr speaks further of the inapplicability of classical electrodynamics, which would lead to the electron's falling into the nucleus, something that does not occur in nature—a real atom preserves for a long time a definite size and frequencies. In addition, the energy emitted by an atom is immeasurably less than the energy that would be released if the electron fell into the nucleus. He then turns to estimates of the energy and the radius. From Planck's radiation theory it follows that the amount of energy emitted in each radiation event is $h\nu$. Now suppose, says Bohr, that the electron first arrives at a high orbit with a low gyration frequency; then it goes over to the final orbit, emitting n portions of radiation. Suppose that the mean radiated frequency $\bar{\nu}$ is half the final gyration frequency ν_0. Then the stripping energy is $W = nh\nu_0/2$. And such an estimate leads to the exact result! This is one of the examples of Bohr's remarkable intuition.

Substituting ν_0 from this estimate in the expression (7), we obtain

$$W = \frac{mZ^2e^4}{\hbar^2}\,\frac{1}{2n^2}, \qquad \nu_0 = \frac{1}{2\pi}\,\frac{m(Ze^2)^2}{\hbar^3 n^3}, \qquad a = \frac{\hbar^2}{me^2}\,\frac{n^2}{Z}. \tag{8}$$

If in these expressions we vary n, we obtain the W and a corresponding to the possible configurations of the system. According to Bohr, these will then be the stationary states in which an electron does not radiate. The value of W is maximal when n is equal to unity. This corresponds to the most stable orbit. Substituting the experimental values known at that time for e, m, and h, Bohr obtained the estimates $a = 0.55 \cdot 10^{-8}$, $\nu_0 = 6.2 10^{15}$, $W/e = 13\,\text{eV}$, and concluded: "We see that these values are of the same order of magnitude as the linear dimensions of the atoms, the optical frequencies, and the ionization potentials."

Further, Bohr refers to the work of Nicholson, which we have already mentioned. He proves the incorrectness of Nicholson's main thesis, that the radiation frequency is equal to the gyration frequency—for after the emission of each portion of energy the gyration frequency changes. Nicholson's theory is incapable of explaining the formulas of Balmer and Ritz.

Bohr lists the assumptions made in his calculations: "1) That the dynamical equilibrium of the systems in the stationary states can be discussed by help of the ordinary mechanics, while the passing of the systems between different stationary states cannot be treated on that basis. 2) That the latter process is followed by the emission of a *homogeneous* radiation, for which the relation between the frequency and the amount of energy emitted is the one given by Planck's theory." And then a remarkable statement, which I give in full: "The first assumption seems to present itself; for it is known that the ordinary mechanics cannot have an absolute validity, but will only hold in calculations of certain mean values of the motion of the electrons. On the other hand, in the calculations of the dynamical equilibrium in a stationary state in which there is no relative displacement of the particles, we need not distinguish between the actual motions and their mean values." Here is an assumption analogous to Einstein's, namely, that the relation (1), obtained by Planck for a classical oscillator, also remains true when allowance is made for quantization. But, in addition, there is also a hint of an idea of Bohr, still in the future and, as we now know, erroneous—that the conservation laws in microscopic systems are satisfied only on the average. The guess of the connection between the mean radiated frequency and the gyration frequency in the final state will be confirmed later by means of the correspondence principle.

The second section, "Emission of line spectra," begins with the words "General evidence indicates that an atom of hydrogen consists simply of a single electron rotating round a positive nucleus of charge e." This assumption was motivated by the fact that in J. J. Thomson's experiment with positive rays hydrogen was the only element never encountered with positive charge greater than the electron charge. Such was the level of the conclusions attained at that time about atomic properties.

From (8), Bohr obtained an expression for the radiation frequency $h\nu = W_2 - W_1$:

$$\nu = RZ^2\left(\frac{1}{n_2^2} - \frac{1}{n_1^2}\right), \quad R = \frac{2\pi^2 me^4}{h^3}, \tag{9}$$

from which Balmer's series is obtained for $n_2 = 2$, and for $n_2 = 3$ the infrared series predicted by Ritz and observed by Paschen. The calculated Rydberg constant R differs from the observed constant by only a few percent.

Then follows the remark that the number of observed lines in a series is limited by the condition that the atomic diameters, which increase with increasing n, must not exceed the distances between the particles of the gas in which the radiation is observed.

Bohr points out that from his theory it is not possible to obtain the other series ascribed to hydrogen, and that it is natural to ascribe them to helium. Indeed, for $Z = 2$ the expression (9) gives the series that were obtained by Fowler and Pickering.

For complicated atoms, there was the Rydberg-Ritz rule: $\nu = F_i(n_2) - F_k(n_1)$, where $F_i(n) = K/(n + \alpha_i)^2$. Bohr shows that K is a universal constant, equal to Rydberg's constant. Here is his argument: For large n, any atom becomes hydrogenlike, since the orbit exceeds the atomic dimensions, and, therefore, the constant $K = R = 2\pi^2 me^4/h^3$.

In the third section, which continues general arguments, Bohr sets himself the aim of obtaining his results without the arbitrary assumption about the mean frequency of the radiated light. He assumes that the energy W is connected to the frequency ν_0 by the relation $W = f(n)h\nu_0$, and then, from formula (7), the radiation frequency is

$$\nu = Z^2 \cdot \frac{1}{4}\left(\frac{1}{f^2(n_1)} - \frac{1}{f^2(n_2)}\right) R.$$

To obtain the Balmer series, it is necessary to set $f = cn$. To determine c, Bohr considers large n and equates the radiation frequency to the gyration frequency, i.e., essentially uses what later became known as the correspondence principle. He obtains $c = \frac{1}{2}$, confirming the estimate made at the beginning of his paper. Thus, to confirm the estimate, it is necessary to assume, first, that "...1) the radiation is sent out in quanta $h\nu$, and (2) that the frequency of the radiation emitted during the passing of the system between successive stationary states will coincide with the frequency of revolution of the electron in the region of slow vibrations." Bohr continues: "As all the assumptions used in this latter way of representing the theory are of what we may call a qualitative character, we are justified in expecting—if the whole way of considering is a sound one—an absolute agreement between the values calculated and observed for the constant in question [the Rydberg constant; A. M.] and not only an approximate agreement. The formula (9) may therefore be of value in the discussion of the results of experimental determinations of the constants e, m, and h."

Bohr shows further that, assuming circular motion and using the equality of the modulus of the total energy to the mean values of the kinetic energy, we obtain from (8) quantization of the angular momentum—the angular momentum of the electron is equal to an integral multiple of $h/2\pi$, irrespective of the charge of the nucleus. At the same time, Bohr refers to Nicholson.

We note that in his calculation Bohr did not assume circular orbits. His original expressions (7) correspond to the general case of elliptic motion. He implicitly used the fact that the energy of a hydrogenlike atom depends to a high accuracy only on the principal quantum number.

Bohr notes that if one regards a free electron as a state with very large n, the condition for the frequency is equal to Einstein's formula for the photoelectric effect. This is one further argument in support of Bohr's postulate of the connection between the energy and the frequency.

In the following fourth section, "Absorption of radi-

FIG. 1. Niels Bohr, 1916.

ation," Bohr explains monochromatic x-ray radiation, which "...is emitted during the settling down of the systems after one of the firmly bound electrons escapes, e.g., by impact of cathode particles." Similar arguments are then applied to possible changes in the energy of a fast electron passing through an atom. It "...will loose [*sic*] energy in distinct finite quanta."

In the 64 pages of the paper, Bohr analyzed from the point of view of his theory all the then existing experimental data relating to atoms and molecules. His favorite device is to exhibit the same formula in different manifestations. The arguments are made convincing, not by any one indisputable fact, but by the general picture. It was certainly this feature that brought fourth Rutherford's desire to shorten the paper. More than once he admonished Bohr for verbosity, commenting that long papers frighten the reader and that the English fashion is to present the subject very briefly and strikingly, in contrast to the German method which regards as a virtue the ability to be as boringly long winded as possible. Rutherford was to submit Bohr's paper to the *Philosophical Magazine,* and sent him a letter with the postscript: "I hope you will not object if at my discretion I remove from your paper the parts that appear to me unnecessary!... ." Receiving the letter, Bohr dashed to Manchester, and in long arguments managed to defend all his propositions and formulations; the paper did not become shorter. All the collaborators of Bohr said that any attempt to shorten what he had written would lead to the corrected variant being still longer.

As was already said, Bohr ascribed to helium the spectral lines of Pickering and Rydberg, which Fowler had succeeded in seeing in a laboratory device. Bohr analyzed in detail this idea in a brief paper in 1913: "The spectra of helium and hydrogen."[24]

If the difference between hydrogen and helium were determined solely by the nuclear charge, then

$$K=\lambda\left(\frac{1}{n_1^2}-\frac{1}{n_2^2}\right)$$

for hydrogen should be four times greater than for helium; however, experiment gave 4.0016, close to four, but the deviation exceeds the error of the experiment. The constants e, m, and h, which were not known sufficiently accurately, do not occur in this ratio. Bohr explained the discrepancy by the difference between the reduced masses of the electron in hydrogen and in helium. Using the formula corrected in this manner, he obtained the theoretical value 4.00163, in exact agreement with the experiment.

The extremely close agreement between the results of the theory and the experiment made Bohr's theory particularly convincing.

In the same paper, Bohr predicted numerous other lines of helium which should be somewhat displaced compared with the Balmer lines and not coincide with them, as followed from the previous point of view. Within a year, Evans discovered these lines in the previously calculated positions. Evans's results to a large degree determined Einstein's relation to Bohr's theory.

Hevesy wrote to Bohr: "When I asked him [Einstein; A. M.] what he thought of his theory, he answered that it was a very interesting, very important theory if, of course, it was not wrong, etc., etc., and that he himself many years ago had had very similar ideas but had not enough wit to develop them. I reported to him that is has now been shown that the Pickering-Fowler spectrum belongs to helium. He was shaken and only muttered: "But in that case the frequency of the light is completely independent of the electron frequency! (I understood him thus??) But this is a colossal discovery! This means Bohr's theory must be correct."

It can be seen from this what was the most difficult thing in Bohr's paper. Planck applied energy quantization to an oscillator, Einstein to radiation and to elastic vibrations, and Nicholson to the atom. The main difficulty was to decide to dispense with making the radiation frequency equal to the frequency of gyration in the orbit!

These papers of Bohr were a decisive stimulus for all subsequent development of atomic physics. But at that time he revealed himself, not as a philosopher, as subsequently, but as a first class theoretical physicist with a deep intuition, inclined to estimates and qualitative understanding of the phenomena to a greater degree than to their mathematical description. In his paper "Quantum theory and its interpretation,"[1] Heisenberg wrote: "...mathematical clarity had in itself no virtue for Bohr. He feared that the formal mathematical structure would obscure the physical core of the problem, and in any case, he was convinced that a complete physical explanation should absolutely precede the mathematical formulation."

5. Quantum physics up to 1923

Niels Bohr's Nobel Prize speech "The structure of the atom" (1922)[25] contains a brief review of the most important experimental and theoretical events in quantum physics. There now follows a brief summary of this speech.

The most direct confirmation of Bohr's atomic theory were the experiments of Franck and Hertz on the excitation of atoms by electron beams. Approximately in the same period (1913–1914) belong the investigations of Moseley—experiments to determine the relation between the frequency of the spectral lines of characteristic x-ray radiation and the serial number of the element; these proved that the charge of the nucleus is equal to the serial number. The experiments confirmed Bohr's theory, according to which $\nu_\alpha = RZ^2$.

Bohr mentions in his speech the theory of multiply periodic motions, the quantization conditions [Bohr-Sommerfeld (1915–1916); A. M.], the appearance of new quantum numbers, the explanation of the fine structure of the spectral lines of hydrogen and helium and the fine structure of x-ray spectra (Sommerfeld, 1915), the explanation of the Stark effect (Schwarzschild, Epstein, 1916), the theory of the Zeeman effect, from which spatial quantization followed (Sommerfeld, Debye, 1917) and the remarkable confirmation of this prediction in the Stern-Gerlach experiments (1922), and, finally, the development of the correspondence principle (Bohr, Ehrenfest, Einstein).

Whereas in the 1913 paper the correspondence idea was used only to determine the numerical factor in Rydberg's constant, from Bohr's 1918 paper[26] onward the correspondence principle was used to determine the intensity and polarization of spectral lines (Kramers, 1920).

In his speech, Bohr noted that the existing quantum theory gave an explanation of the molecular spectra in satisfactory agreement with the experimental facts.

He then turned to a detailed discussion of Mendeleev's periodic table in the light of atomic theory. He commented that it gave "...an explanation of the characteristic deviations from simple periodicity in the system of the elements" and traced the formation of the family of rare-earth elements. This made it possible to predict the properties of the element with the atomic number 72, which had been erroneously classified as a rare earth. In 1923, Coster and Hevesy showed that this element, as follows from the theory, has chemical properties close to those of zirconium. It was called *hafnium* in honor of the ancient name of Copenhagen.

In discussing Mendeleev's table, Bohr introduced a suggestion that anticipated the Pauli exclusion principle (Pauli, 1925–1926). He had to assume that closed configurations are energetically advantageous and that after a shell has been closed electrons occupy only higher orbits. This hypothesis undoubtedly helped Pauli to arrive at his exclusion principle.

Bohr summarized the entire development of the old atomic theory in 1923 in the paper "On the application of quantum theory to atomic structure. 1. The basic postulates of the quantum theory."[27] Of particualr interest for us is the final section, "On the formal nature of the quantum theory," in which Bohr asks how one can reconcile the concept of discontinuity in the atom with the continuity of classical electrodynamics. He considers possible ways of overcoming this difficulty and, as one of them, Einstein's light-quantum hypothesis, according to which "...the propagation of radiation does not take place by ordinary wave motion but in such a way that energy...is concentrated in a small section, and the absorption process takes place as a whole... ." From the following phrase it can be seen how Bohr at that time regarded the idea of light quanta: "...This hypothesis leads to insuperable difficulties in the explanation of interference phenomena... ." And further: "...Therefore, the hypothesis of light quanta is not suitable for giving a complete picture of the processes... ."

Another way was to assume that the probabilities of different processes of exchange between an atom and electromagnetic waves in empty space corresponds to "hidden" radiation reactions "but the mechanism of such a coupling does not begin to act immediately."

This means that a transition in an atom need not coincide in time with the change in the energy of the electromagnetic field, i.e., causality may be violated. (In this section, Bohr uses his injunction "never express yourself more clearly than you think!") In Bohr's opinion, "...both the law of conservation of momentum and the law of conservation of energy are ill suited for one to be able to draw conclusions by means of them about the nature of the processes... ." Here we already find the ideas that will be developed in the paper of Bohr, Kramers, and Slater, in which they abandoned causality and the conservation laws in individual events.

6. Doubts about the conservation laws and causality

In the introduction to the paper "The quantum theory of radiation,"[28] written by Bohr in collaboration with Kramers and Slater (BKS), the authors state: "On the correspondence principle it seems nevertheless possible, as it will be attempted to show in this paper, to arrive at a consistent description of optical phenomena by connecting the discontinuous effects occurring in atoms with the continuous radiation field in a somewhat different manner from what is usually done."

What dictated this desire? The authors wished to preserve the continuity of electrodynamics, since they believe that only classical electrodynamics can explain interference phenomena.

There are two possibilities: Either an electron radiates on the transition, i.e., simultaneously with the transition a light quantum is created (the conservation laws and causality are respected in an individual event), or the electron radiates continuously but preserves its state—it has only a probability of transition, which is chosen to be such that on the average the difference between the electron energies is equal to the energy given to the field. The authors chose the second possibility.

Doubts about the conservation laws and causality for microscopic objects have a long history. In his book, Pais mentions Einstein's letter to his friend Laub, written in 1910: "I now have great hopes of solving the radiation problem without quanta... ." In fact, Einstein immediately abandoned this thought and in the following letter wrote: "The devil has played a trick on me... ." In 1911, he raised this question once more at the Solvay congress, speaking of two possible interpretations of his formula for the fluctuations of black-body radiation—either the hypothesis of light quanta or the abandonment of the conservation laws. Einstein rejected the second possibility: "...Who has the daring to do that?... ."

In 1916, violation of a conservation law in individual events was proposed by Nernst. In 1922, Sommerfeld believed that the least drastic way of saving the wave theory of light and quantum phenomena would be to sacrifice the energy conservation law. In 1922, Bohr also began to think in these terms.

Bohr, Kramers, and Slater based their arguments about probabilities of transitions on Einstein's 1916 paper[21]: "...Such considerations have been introduced by Einstein, who has shown how a remarkably simple deduction of Planck's law of temperature radiation can be obtained... ."

As we have already said, in this paper Einstein introduced probabilities of stimulated absorption and emission and the probability of spontaneous emission. According to BKS, there is no spontaneous radiation, transitions being caused by a "virtual radiation field." The atom need not necessarily know what transition it will later make. Thus, the authors gave up not only the conservation laws but also causality.

The paper did not contain any theory, the only talk was of a preferred direction for the development of quantum physics. The BKS idea was refuted by experiment, but this paper stimulated studies to test causality and the conservation laws in individual events. In 1924, Walther Bothe and Hans Geiger established by means of a coincidence technique that in the Compton effect the times of emission of the electron and the secondary quantum coincide to an accuracy $\Delta\tau < 10^{-3}$ sec, whereas according to BKS there should be no correlation between these two events. Later, in 1955, other experimentalists obtained $\Delta\tau < 10^{-11}$ sec. Thus, the experiment made it necessary to give up the idea of causality violation. In 1925, Compton and Simon, investigating the Compton effect by means of recoil electrons in a Wilson chamber, confirmed not only causality but also the energy and momentum conservation laws in individual events. In 1936, confirmation of the energy and momentum conservation laws in electron-positron annihilation was obtained by L. A. Artsimovich, A. I. Alikhanov, and A. I. Alikhan'yan.

Bohr accepted the condemnation by the experiment with dignity. He wrote to Fowler: "...Our revolutionary attempts must be buried with full honor... ."

In a postscript on his 1925 paper "On the action (*Wirkung*) of atoms in collisions,"[29] Bohr wrote: "This coupling ...forces us to a corpuscular picture of light propagation." Thus, it was only in 1925 that Bohr accepted the existence of light quanta. He concluded: "...We must be prepared to accept that the required generalization of the classical electrodynamic theory necessitates a far reaching revolution in the concepts on which the description of nature has hitherto rested."

Bohr was now ready for an even more decisive revolution; he stood before the creation of a new idea in natural philosophy, the concept of complementarity.

II. PHYSICS AND PHILOSOPHY

The new quantum theory led to a much more decisive reexamination of the concepts of classical physics than the stormy events of 1900, 1905, and 1913. These events had merely shown the inapplicability of classical mechanics and electrodynamics for the description of the microscopic world. They merely uncovered the "sores" of classical physics. The question of the causes of the breakdown of the classical ideas was not posed. These were jumps, unprecedented in the history of science, into the unknown—the most important quantitative relations were found almost intuitively, without a clear picture of what now constitutes the foundations of the theory. The new quantum physics posed the problem of elucidating the structure and essence of the theory, and the task of creating the philosophy of quantum physics.

I begin with an analysis of the principal discoveries of the new quantum theory, and then explain how, as it appears to me, one should understand the words "philosophy of physics," or, more narrowly, the "philosophy of quantum physics." Only then will one be able to understand the unique part that Niels Bohr played in establishing quantum theory.

1. The new quantum theory

The 1923 paper of de Broglie[30] on matter waves was an unexpected but natural continuation of Einstein's 1905 paper on light quanta with the difference that in the case of light the wave properties were discovered before the corpuscular. It was shown by de Broglie that for "matter waves," as for light, the group velocity is equal to the velocity of the motion of the particles.

The guess that de Broglie made was based on an incomplete analogy: The wave properties of light were established for the classical electromagnetic field. They do not have a direct relationship to the wave properties of particles. In the first case we are dealing with physical waves, in the second with probability waves. The analogy is made exact only for a light intensity so low that individual independent quanta, whose behavior is described by their wave function, participate in the phenomenon. The connection between the wavelength and the momentum is preserved, since the frequency and wave vector of this wave function are equal to the frequency and wave vector of the corresponding electromagnetic wave.

In 1925, matrix mechanics was created. In a letter to Kronig (given in the Pauli memorial collection of Ref. 31), Heisenberg explained how he arrived at matrix mechanics. According to his words, matrix mechanics arose—almost unambiguously—from the correspondence principle. According to this principle, the probability of emission of a wave with frequency $m\nu_0$ is proportional to the square of the mth harmonic in the Fourier expansion with respect to the time of the classical dipole moment (ν_0 is the classical frequency of gyration in a high orbit). With this harmonic, Heisenberg associated a matrix element $d_{n,n'}$, which corresponds to the transition from the state n to the state $n' = n \pm m$. Heisenberg assumed that between the matrix elements there exist the same relations as between the Fourier components of the corresponding classical quantities. Thus, in the case of an oscillator, the matrix element of the momentum is related to the matrix element of the coordinate q by the equation $\dot{p}_{n,n'} = \ddot{q}_{n,n'} = -\omega_0^2 q_{n,n'}$ ($\omega = 2\pi\nu_0$).

Since in the Fourier expansion of the classical motion of an oscillator there exists only one harmonic with frequency ν_0, the matrices of the momentum and the coordinate contain only elements next to the diagonal. Constructing from the matrices a quantity with the same properties as the Poisson bracket, one can readily obtain the commutation condition $pq + qp = \hbar/i$, and as a result a system of equations for determining the matrices and eigenvalues arises.

The next step was the Schrödinger equation (1926),[32] which arose as a generalization of de Broglie's idea to the case of motion in an external field. In the words of Dirac,[33] Schrödinger first wrote down the ordinary wave equation containing the second derivative with respect to the time of the wave function, i.e., the equation that we now call the Klein-Gordon-Fock equation. However, he decided not to publish it, perhaps being embarrassed by the negative values of the energy. Going to the nonrelativistic limit, Schrödinger obtained his equation, which was used first of all to determine the stationary orbits and energy eigenvalues of the hydrogen atom.

The stable Bohr orbits and the Bohr-Sommerfeld quantization rules were immediately explained. For a stationary solution of the Schrödinger equation to be obtained, an integral number of de Broglie wavelengths must fit into the dominant region of the motion.

If matrix mechanics arose under the influence of Bohr's ideas, the de Broglie waves and wave mechanics derive from Einstein. For Schrödinger, the decisive stimulus to the creation of his equation was Einstein's formula for the fluctuations of the energy density and the particle density in an ideal Bose gas. This formula is obtained in the same manner as formula (6), and differs from it only in having a different law of dependence of the mean particle energy on the temperature.

In the case of Bose particles, in contrast to quanta, an unexpected feature was the wave term, which Einstein, who had obtained this formula immediately after de Broglie's paper, explained by the interference of "matter waves."

In the paper on the hydrogen atom, Schrödinger wrote[32]: "Einstein's theory of the gas [ideal Bose gas; A. M.] can be obtained by considering standing waves which satisfy de Broglie's dispersion law. The arguments given above for the case of an atom can be regarded as a generalization of these considerations."

Analysis of thought experiments on interference and the assumption that the interference persists even when the flux is so low that the particles impinge on the screen independently of one another made it necessary to go over to a probability interpretation of the wave function. If the wave field were a physical wave, one electron would be sufficient to give the complete interference pattern. The "wave pilot" idea of de Broglie and Schrödinger's idea of real "wave packets" describing the motion of particles were rejected in favor of the probability interpretation (Max Born, 1926).

The probability interpretation of the wave function was prepared by Niels Bohr's papers in 1923–1924. There, the probability idea had been applied to electron transitions. This idea, in its turn, came from Einstein's 1916 paper,[21] in which he introduced the probabilities of spontaneous and stimulated transitions, and the idea of a probabilistic interpretation of microscopic objects appeared for the first time.

In 1927, the experiments of Davisson, Germer, and Thomson on the diffraction of electrons by crystals confirmed not only the wave nature of the electron but also de Broglie's expression for the wavelength. In the same year there appeared simultaneously the uncertainty relation of Heisenberg and the complementarity principle of Bohr. The uncertainty relation is the concrete expression of the general principle of complementarity, of which we shall speak in detail.

Even after the mathematical formalism of quantum mechanics had been constructed, huge efforts were needed to comprehend the results that had been obtained. For the first time equations were obtained for a probability field that described the behavior, not of a statistical system, but individual particles.

Physicists encountered new concepts, a new type of epistemology, and a new interpretation of the measurement process. It became necessary to create a philosophy of the quantum theory.

2. Philosophy of physics

If Bohr had restricted himself to only the studies that appeared before the creation of quantum mechanics, he would hardly, despite the grandeur of his achievement, have assumed the position of predominant influence in quantum physics. But during the period in which quantum mechanics was created, Bohr revealed himself as a deep philosopher who played a leading role in the comprehending of the new physics.

One should probably define more accurately the concept of *philosopher* as applied to Niels Bohr. His attitude to professional philosophers was always sceptical. In the paper "The forties and the fifties,"[1] Stefan Rozental wrote: "It was always a source of sorrow to Bohr that the professional philosophers, who after all should be the very people to apply more broadly the important view-points which had emerged during the development of atomic physics, did not seem to be sufficiently interested in the problems. He made use of every opportunity to talk to philosophers, both in Denmark and elsewhere, but most often without any satisfactory results." Bohr spoke of the difference between a specialist and a philosopher: The former strives to know as much as possible in a narrow field and as a result knows everything about nothing, while the latter, wishing to comprehend everything, knows at the end nothing about everything... .

Bohr believed that professional physicists should occupy themselves with the philosophy of physics. And such a concrete philosophy is entirely necessary for the development of science. For it prepares the ground on which the unexpected sparks of intuition arise.

The ever increasing specialization of science in the last decades has had the consequence that "natural philosophy" as a whole has become a field too large for constructive investigation by the methods of the theory of knowledge. Physicists, biologists, psychologists,..., working creatively in their own field, must attack this problem. Such a point of view is erroneously called positivism, since "applied philosophy" does not stand in opposition to philosophy in general but prepares the ground for more far reaching generalizations.

If one traces from this point of view the development of physics in the 20th century, one can recognize that it was applied philosophy that gave the stimulus to science. One of the best examples of this is the history of the creation of the special theory of relativity.

The idea that science should not employ concepts that cannot be formulated in the language of a real or a thought experiment—the principle of observability—forced Einstein to question the intuitive concept of simultaneity and introduce a definition of it that can be tested experimentally.

In his popular paper "The measurement of time"[34] of 1898, Henri Poincaré advanced the remarkable idea that the definition of simultaneity is a matter of convention. In this paper there is no talk of the relative passage of time in different inertial systems. All that is discussed is the simultaneity of events in two distant points. Poincaré concludes: "The simultaneity of two events or the order in which they occur, and the equality of two durations must be defined in such a way that the laws of nature are formulated in as a simple a manner as is possible. In other words, all these rules, all these definitions are merely the fruit of an unrecognized convention."

What different conclusions were drawn by two great men—Poincaré and Einstein—from one and the same thought! Einstein, having established the relativity of simultaneity, concluded, on the basis of the principle of observability, that time flows differently in two different inertial systems. But Poincaré accepted the Newtonian concept of time and space. He maintained a conventionalistic philosophy, according to which arbitrary conventions lie at the basis of the mathematical and natural-scientific theories. This led him to believe that Einstein's propositions are conventional and to reject the theory of relativity.

The theory advanced by Lorentz and developed by Poincaré is not the theory that we call the theory of relativity. For both Lorentz and Poincaré, in contrast to Einstein, the Lorentz contraction is obtained, not as an unavoidable consequence of kinematics, but as the result of a change in the balance of the forces between the molecules of a solid in motion.

In 1909, Poincaré gave a lecture at Göttingen entitled "The new mechanics."[35] He listed the postulates adopted in his theory: 1) the physical laws do not depend on the chosen inertial system; 2) the velocity of a material body must not exceed the velocity of light; and, finally, 3) bodies contract along the motion. Of this third postulate, Poincaré says: "It is necessary to accept a much stranger hypothesis, contradicting everything to which we are accustomed, that a body in motion undergoes a deformation in the direction of motion—however strange, it must be recognized that this third hypothesis is excellently confirmed... ." As is evident from these words, the Lorentz contraction appears from the Lorentz-Poincaré position as a remarkable event, which must somehow be realized for all forms of forces. But with Einstein it is a direct consequence of his two postulates: the requirement that the laws of nature should not change on the transition from one inertial system to another and the constancy of the velocity of light.

The idea of arbitrary conventions hardly applies unconditionally in the experimental sciences. The coordinate systems of Ptolemy and Copernicus are logically on an equal footing, but without Copernicus's "convention" Kepler's laws and the law of gravity would not have been found. One could also construct a new mechanics on the basis of the Lorentz-Poincaré "convention." But precisely on account of the third postulate it would be incomparably more complicated than the theory of relativity. As we know, it is necessary to elucidate, for example, the form of the forces that ensure equilibrium of the electron, and to introduce a "Poincaré pressure."[36]

Just as without the transition to the heliocentric system there would have been no celestial mechanics, without Einstein's "convention" there would have been neither the theory of gravitation nor modern field theories.

Lorentz and Poincaré made extremely deep contributions to the theory of relativity but did not bring about a revolution. After Poincaré's 1898 paper and Lorentz's 1904 paper, it remained to take one step, but this step required a different cast of mind, a different philosophy. Lorentz was prevented from taking this step by his deep adherence to the philosophy of physics of the past century. Poincaré's powerful mathematical intuition came into conflict with the physical intuition needed for this problem. It is possible that his mathematical past is what gave rise to a conventionalistic theory of knowledge that was too flexible. In his paper "Henri Poincaré and physical theories," de Broglie said[37]: "...The young Albert Einstein, who at that time was only 25 years old and whose mathematical knowledge could not be compared with the deep understanding of the French genius-scientist, nevertheless found before Poincaré the synthesis that immediately eliminated all the difficulties by using and justifying all the attempts of his predecessors. This decisive stroke was made by a powerful intellect guided by a deep intuition and understanding of the nature of physical reality... ."

Physics is inconceivable without mathematics and mathematical concepts, but does not reduce to them. Moreover, the main thing in physics is not the formulas but their interpretation—the understanding, for that is what feeds the intuition. Physics is not developed by means of mathematical logic but by physical intuition.

It will be difficult for a physicist from a mathematical background, who regards theoretical physics as a branch of applied mathematics, to accept these assertions. He will be astounded: "Why do you accord the main service in the creation of the theory of relativity to Einstein if the Lorentz transformations were obtained earlier?" or "Why do you ascribe the main role in the understanding of quantum mechanics to Bohr when the basic equation of the theory was found by Schrödinger (or, in matrix form, Heisenberg)?"

I should like to think that this paper may convince the reader of the necessity for distinguishing the methods of understanding employed by physics and mathematics, for distinguishing the philosophies of these two sciences.

The views of physicists on the interrelationship of physics and mathematics vary from time to time depending on the successes of the "intuitive" or "formal" directions.

Thus, Bohr's views of 1913 (see the final paragraph of Sec. I. 4) underwent a certain change after the appearance of matrix mechanics. In the paper "Atomic theory and mechanics" matrix mechanics. In the paper "Atomic theory and mechanics" (1925) he wrote:

"It will interest mathematical circles that the mathematical instruments created by the higher algebra play an essential part in the rational formulation of the new quantum mechanics. Thus, the general proofs of the conservation theorems in Heisenberg's theory carried out by Born and Jordan are based on the use of the theory of matrices, which go back to Cayley and were developed especially by Hermite. It is to be hoped that a new era of mutual stimulation of mechanics and mathematics has commenced. To the physicists it will at first seem deplorable that in atomic problems we have apparently met with such a limitation of our usual means of visualization. This regret will, however, have to give way to thankfulness that mathematics in this field, too, presents us with the tools to prepare the way for further progress."

One of the most important heuristic principles making it possible to seek the truth in physics, as, incidentally, in other sciences, is the concept of the beauty of a theory, law, or conception. Although the term *beauty* is subjective, the concept itself is rather objective and seldom leads to disagreements in estimates.

Bohr said of Heisenberg's theory of elementary particles: "This theory is not sufficiently crazy to be correct." Bohr's expression is frequently quoted and has harmed not a little dilettantes, who take absurdity of a theory as a sufficient condition of its value. It is possible that by the words "This theory is not sufficiently crazy to be correct" Bohr wished to say that Heisenberg's theory does not propose new methods of describing nature. However, after the creation of quantum mechanics none of the further improvements, colossal successes in both the physics of condensed media as well as in the theory of elementary particles notwithstanding, has changed our methods of describing nature. Therefore, it might be more natural to suggest that Bohr's phrase was dictated by his politeness. A more accurate statement would have been that this theory cannot be correct, since it is not sufficiently beautiful.

The philosophical principle of observability and the correspondence principle led Heisenberg to the creation of matrix mechanics, which contains only observable quantities—matrix elements—and later to the uncertainty relation.

A fundamental task—the search for the symmetry and unity of the laws of nature—has had a huge influence on the development of theoretical physics and continues to do so. The idea of symmetry permeates all modern physics, from the theory of elementary particles to the theory of metals. The modern attempts to construct unified field theories are a palpable embodiment of the philosophical idea of the unity of nature.

Physicists extract many consequences from the causality principle, the value of which follows from the fact that it can be tested experimentally, as we saw in the discussion of the Bohr-Kramers-Slater paper.

Physicists use one further methodological idea so frequently that it appears almost trivial—the correspondence principle: A new theory must go over into the old one at its limit of applicability... .

The Bohr-Einstein controversy, of which we have yet to speak, was an example of a collision of different philosophies. Einstein's theory of knowledge did not admit the introduction of categories such as a probabilistic description of reality. But for Bohr the idea of complementarity made the probabilistic interpretation not only natural but necessary. But it was in fact from Einstein that many theoreticians expected the development of a consistent philosophy of quantum mechanics.

Deep physical ideas are always the fruit of philosophical interpretation of physics. It is only after such explanations that I venture to call Niels Bohr a philosopher and assert that his main role in the creation of the quantum theory was precisely in the development of the concept that made a probabilistic interpretation of quantum mechanics acceptable for physics.

Bohr's philosophical ideas created the ground, or, more precisely, prepared the subconscious of the physicists for discoveries such as matrix mechanics, the uncertainty relation, or the probabilistic interpretation of the wave function.

Let us recall the tortured disputes between Bohr and Heisenberg and Schrödinger—they brought Heisenberg to tears and Schrödinger to the sick bed. In discussing the wave-corpuscle problem, Heisenberg attempted to emphasize the corpuscular aspect of particle behavior, while Schrödinger assumed that all properties of particles are determined by the wave picture. Bohr, in the words of Heisenberg,[1] "...Attempted in everything to take into account the simultaneous existence of the corpuscular and the wave pictures. He maintained the conviction that only these two pictures together can give a complete description of atomic processes...I felt a repugnance to such a view of things... ." In these disputes, there arose already then Bohr's idea of complementarity, and the philosophy of quantum physics was born.

A feature of such "applied" philosophy is that after the solution of the problem the philosophical problem disappears. So it was with the wave-corpuscle paradox. After the creation of quantum electrodynamics, when it became clear that the light quantum is a portion of an excitation of an electromagnetic wave, the problem disappeared. It disappears whenever we can answer any reasonable question posed by an experiment. This is one of the reasons for a certain neglect of the philosophical side of physics, a neglect that is particularly widespread among young theoreticians. Another reason is that one can successfully undertake theoretical physics without any philosophy if one limits oneself to the development of the consequences of already existing theories. Such studies are attractive on account of their "certainty" and "reliability" and border on applied mathematics. They do not contain significant assumptions that require testing, but precisely for this reason such studies do not by themselves lead to the appearance of new theories. Many problems of quantum mechanics were solved by physicists who were not interested in the logical structure of quantum theory. But for the discovery, for example, of the formulation of quantum mechanics in the form of a path integral (Feynman, 1948) a deeper understanding of quantum theory, a philosophy of physics, was needed.

Occupying oneself with the philosophy of physics is a thankless business. Those who prepared the ground and sowed the seeds are frequently forgotten, and the honor of discovery is carried away by the one who collects the fruits. For the philosophy of physics one needs the rarest combination of depth of mind, strength of conviction, and spiritual purity—as Niels Bohr had them.

3. Complementarity

Bohr did not like to work alone. He never himself wrote but always dictated his thoughts either to Margrethe, or to his secretary Betty Schultz, or to students—Pauli, Kramers, Rosenfeld... . He had a continuous need for a listener to make his thought take its final form. Kramers quotes Bohr as follows: "My method of working takes the form that I attempt to say that which, in essence, I am incapable of saying, since I simply do not understand it!" Hendrik Casimir wrote in his paper "Recollections from the years 1929–1931"[1]: "While pondering the philosophical problem of the description of nature he perfected to an even higher degree the art of obtaining qualitative or semiquantitative results without detailed calculations. This type of analysis that was partly based on an amazing skill in separating effects according to orders of magnitude was characteristic of all his work."

Bohr's cast of mind was manifested best in his unusual and unexpected dialectics.

Let me give some of his favorite sayings. He said: "Every judgement that I make must be understood, not as an assertion, but as a question." Or again: "There are two kinds of truths—the trivial, which it is stupid to deny, and the deep, for which the opposite assertion is also a deep truth." This means that an assertion is nontrivial to the extent that it can be refuted.

In his paper "The versatility of Niels Bohr,"[1] Dirac speaks of this thought in more detail: "For reasoning about abstract philosophical questions Bohr was very conscious of the limitations imposed by possible ambiguity in the meaning of words. This ambiguity may govern the truth or falsity of a statement. Bohr considered that the highest wisdom necessarily involves words whose meaning cannot be defined unambiguously. Thus the truth of a statement of the highest wisdom is not absolute, but is only relative to a suitable meaning for the ambiguous words in it, with the consequence that the converse statement also has validity and is also wisdom."

Bohr said: "Never express yourself more clearly than you think." He loved a Chinese proverb: "We are all simultaneously actors and spectators in the drama of life"; similar thoughts can be found in Kierkegaard.

The principle of complementarity, of which we are to speak, was the summit of Bohr's dialectics.

At the beginning of 1927, two events occurred almost simultaneously. After bitter disputes, Bohr and Heisenberg separated for a time, and when they met again Heisenberg had the derivation of the uncertainty relation and Bohr had conceived the principle of complementarity. The uncertainty relation was the quantitative embodiment of the general idea of complementarity, and before this relation had been obtained Bohr was still to take a further step. From the virtuosity of his thought experiments revealed later in the disputes with Einstein, one sees how readily he could have taken this step. But Bohr, the cast of his mind being what it was, could arrive at concrete results only after philosophical clarification.

In his 1928 paper "The quantum postulate and the recent development of atomic theory,"[38] Bohr repeated the content of his paper read in September 1927 on Lake Como during the conference dedicated to the memory of Alessandro Volta (1745–1827). At the start of the paper, Bohr writes: "The quantum theory is characterized by the acknowledgement of a fundamental limitation in the classical physical ideas when applied to atomic phenomena... . Our usual description of physical phenomena is based entirely on the idea that the phenomena concerned may be observed without disturbing them appreciably...Now, the quantum postulate implies that any observation of atomic phenomena will involve an interaction with the agency of observation not to be neglected."

Then follows what could be the most important: "...If in order to make observation possible we permit certain interactions with suitable agencies of measurement, not belonging to the system, an unambiguous definition of the state of the system is naturally no longer possible, and there can be no question of causality in the ordinary sense of the word. The very nature of the quantum theory thus forces us to regard the space-time co-ordination and the claim of causality, the union of which characterizes the classical theories, as complementary but exclusive features of the description, symbolizing the idealization of observation and definition respectively. Just as the relativity theory has taught us that the convenience of distinguishing sharply between space and time rests solely on the smallness of the velocities ordinarily met with compared to the velocity of light, we learn from the quantum theory that the appropriateness of our usual causal space-time description depends entirely upon the small value of the quantum of action as compared to the actions involved in ordinary sense perceptions. Indeed, in the description of atomic phenomena, the quantum postulate presents us with the task of developing a "complementarity" theory the consistency of which can be judged only by weighing the possibilities of definition and observation."

Bohr wrote the uncertainty relation in the form

$$c^2\,\Delta m\cdot\Delta T\sim h$$

and said: "In the language of the relativity theory, the content of the relations may be summarized in the statement that according to the quantum theory a general reciprocal relation exists between the maximum sharpness of definition of the space-time and energy-momentum vectors associated with the individuals. This circumstance may be regarded as a simple symbolical expression for the complementary nature of the space-time description and the claims of causality."

Bohr then analyzed the question of the placing of the boundary between the object and the instrument. He writes: "In connection with the measurement of the position of a particle, one might, for example, ask whether the momentum transmitted by the scattering could not be determined by means of the conservation theorem from a measurement of the change of momentum of the microscope—including light source and photographic plate—during the process of observation. A closer investigation shows, however, that such a measurement is impossible, if at the same time one wants to know the position of the microscope with sufficient accuracy."

Very characteristic of Bohr was the striving to study a problem in all manifestations. For example, in discussing the *question of the placing of the boundary between the object* and the measuring instrument, he posed the question: "What is the organ of perception of a blind person holding a walking-stick—the end of the stick or the arm?" And the answer was as follows: "If the stick is held firmly, its end receives the perception; if weakly, the arm."

The first reaction of physicists to the theory of complementarity was restrained. Dirac said that since this idea does not change our description of nature it has no content. Only Pauli, usually of a critical disposition, accepted complementarity and even suggested that quantum mechanics should be called the "theory of complementarity." Bohr later returned many times to the idea of complementarity in popular papers and lectures.

In the words of Rosenfeld, "Bohr undertook an immense and intensive study of the application of the complementarity concept in other fields of knowledge. He regarded this as no less important than purely physical investigations."

Can biological laws be reduced to physicochemical processes? At first glance, all biological processes are determined by the motion of the particles that make up living matter. The ultimate expression of such a point of view is the definition of physiology as the "physical chemistry of nitrogen-bearing colloids." But such a view reflects only one aspect of the matter. Another aspect, more important, are the laws governing living matter, which, although determined by the laws of physics and chemistry, do not reduce to them. Biological processes are characterized by finalism, corresponding to the question "Why?" But physics is interested only in the questions "For what reason?" and "How?" Vitalists believe that only the biological law is important, denying the physicochemical aspect of biological processes.

A correct understanding of biology is possible on the basis of the complementarity of physicochemical causality and biological purposefulness. The concept of complementarity makes it possible to construct a description of living processes on the basis of mutually complementary approaches.

In the paper "Light and life," Bohr comments[39] that a continuous exchange of substances between an organism and the surrounding medium is needed to sustain life, so that a clear definition of an organism as a physicochemical system is not possible. One can therefore expect that any attempt to draw a sharp line, in order to make an exhaustive physicochemical analysis, will call forth a change in the exchange of substances to an extent incompatible with the life of the organism.

Speaking at the International Congress of Anthropological and Ethnological Sciences in Copenhagen in 1938, Bohr spoke in his paper "Natural philosophy and human cultures"[40] of the complementarity of different cultures: "...Each culture represents a harmonious balance of traditional conventions by means of which latent potentialities of human life can unfold themselves in a way which reveals to us new aspects of its unlimited richness and variety." Bohr developed the idea that the nations have equal rights and play complementary parts in the human community.

Bohr thought much about the application of the concept of complementarity in psychology. *In the same lecture*, he said: "We all know the old saying that, if we try to analyze our own emotions, we hardly possess them any longer and in that sense we recognize between psychical experiences, for the description of which words such as "thoughts" and "feelings" are adequately used, a complementary relationship similar to that between the experiences regarding the behavior of atoms... ."

Continuity and discontinuous change of physical phenomena are complementary concepts. Measurements always lead to continuous functions. There are jumps, at least over a short interval, but they are smoothed. In the atom, the smoothing is determined by the finite width of spectral lines; in phase transitions, by the fact that the number of atoms in the sample is finite. In this sense, the assertion "nature does not make jumps" is correct. But at the same time this ironing out does not eliminate the abrupt change; it remains as a reasonable approximation, the accuracy of which increases as the smoothing phenomena are eliminated.

The physical picture of a phenomenon and its mathematical description are complementary. The creation of a physical picture requires the neglect of details and takes us away from mathematical accuracy. And conversely the attempt at exact mathematical description of the phenomena hinders a clear understanding. To the question: "What is complementary to the concept of truth?" Bohr answered: "Clarity."

4. Aspects of the new quantum theory

From the complementarity principle in general and from Bohr's interpretation of the measurement process in particular all the unusual aspects of quantum theory follow.

The predictions of quantum mechanics do not give an unambiguous answer but only the probability of a particular result. However accurately we may determine the state of a particle before it reaches a screen with a slit, we cannot predict the precise point of the photographic plate placed behind the screen at which the electron will show up.

This lack of uniqueness contradicts the determinism of classical physics. The successes of celestial mechanics in the 17th–18th centuries inspired deep faith in the possibility of unambiguous predictions. This faith was expressed by Pierre

Laplace: "An intelligence that at some moment knew all the forces acting in nature and the relative position of its constituent parts could, if, in addition, it were sufficiently omnipotent to analyze all these data, grasp in a single formula the motions of the most vast bodies in the universe and the lightest atom; for it nothing would be obscure and both the past and the future would be there before its eyes..." Laplace assumed that if the coordinates and velocities of all particles are known it is possible to predict the future of the universe. Just as unambiguous are the predictions of classical electrodynamics.

In quantum mechanics, uncertainty is fundamental; it follows from the complementarity of the quantum nature of microscopic objects and the classical methods of description. It is not possible to specify the "coordinates and velocities of all particles." The most that can be done is to specify at the initial instant the wave function. Quantum mechanics makes it possible to find the wave function uniquely at any later time. Causality in the Laplacian sense is violated, but in a more precise quantum-mechanical understanding it is respected. From a maximally completely determined initial state there follows uniquely the possible final state. Only the meaning of the word *state* has been changed.

The principal discovery of quantum mechanics was the probabilistic nature of the laws of the microscopic world.

A probabilistic description of physical phenomena (statistical physics) arose prior to quantum mechanics in complicated systems, in which a small change in the initial conditions leads after a sufficiently great time to a strong change in the state. These systems are described by strictly deterministic equations of classical mechanics, and the probability arises from the averaging over an interval of initial states.

In contrast to this, quantum mechanics says that the probabilistic description is valid for not only complex but also for the simplest systems and does not require any additional averaging of the initial conditions.

Bohr always emphasized that the reason for the probabilistic description of predictions is that the properties of microscopic objects cannot be studied without regard to the means of observation. Depending on the means of observation, and electron appears either as a wave, or as a particle, or as something intermediate. Of course, there are properties that do not depend on the means of observation: the mass, charge, spin of the particle, the baryon charge, magnetic moment,... . But whenever we wish to measure simultaneously any complementary quantities, the result will depend on the method of observation. V. A. Fock called this property of quantum objects "relativity with respect to the means of observation." Pre-quantum physics knew only relativity associated with motion—relativity of the velocity, relativity of the shape. In quantum theory, the result of a measurement depends on how and what one measures in the same coordinate system.

The reasons for this cannot be eradicated—we are forced to describe quantum objects in the language of classical physics, in which we describe our means of observation and in which we formulate our ideas. But just as the objectivity of the phenomena of nature is not diminished but clarified by the theory of relativity, the relativity with respect to the means of observation in quantum theory in no way hinders the definition of the objective properties of microscopic objects.

The history of the development of the universe is not rendered less objective by the fact that we describe it in our human language. Inescapably but without loss we use subjective instruments to describe the objective. In his book *The Philosophy of Physics*, Heisenberg quotes von Weizsäcker: "Nature existed before man, but man before the natural sciences." But the too frequent mentioning of the word "observer" in the description of measurements in quantum mechanics leaves a disagreeable feeling with many. One can avoid saying "observer," and by the word "observation" understand a method of answering a question formulated in classical language. In some way, we comprehend the shape of a multidimensional object by studying its three-dimensional projections, intersecting it along different planes.

The wave function is not a physical field but an information field. From this many features of quantum mechanics follow. First of all, a particle need not necessarily have a wave function. To ascribe to a system a wave function it is necessary to choose a state in which each of a complete set of commuting operators that determine the behavior of the system has a definite value. This can be formulated as follows: On the system it is necessary to perform a maximally complete experiment. But if the experiment is not complete, the theory permits less definite predictions. In some cases, for example, for open systems, such a situation can be described by a density matrix.

The uncertainty relation also holds when a particle does not have a wave function. Of course, since the Schrödinger equation does not contradict this relation, the mean square deviations of canonically conjugate quantities calculated by means of the wave function also satisfy it. But the physical meaning of this result is completely different from what it is in the relation obtained by Heisenberg. Whereas Heisenberg's relation reflects the complementarity of the classical concepts of canonically conjugate quantities and applies to any experiment, complete or incomplete, the relationship between the mean square deviations is a mathematical consequence of Schrödinger's equation and, therefore, presupposes the existence of a wave function.

In quantum mechanics, the superposition principle holds—the wave function is made up of the wave functions of mutually exclusive events.

Suppose there is a screen with two openings. The attempt to determine the path of a particle more accurately by selecting cases when it passes through one opening destroys the interference. This is a manifestation of the complementarity of the classical space-time description of a particle and its wave properties.

One further feature: After every measurement, the wave function changes abruptly. Indeed, suppose an electron has a definite momentum. In this state, before impinging on a photographic plate, the electron may be found with equal probability anywhere; after the blackening of a grain of the plate, the uncertainty in its position has been changed abruptly during a negligible time—it is now specified by the size of the grain. There is a "reduction of the wave function" or "reduction of the wave packet."

It is clear that no physical field can possess such properties. The speed of the propagation of light being finite, it is, of course, impossible to change a physical field in a short time in a large region of space. The abrupt change in the wave function means only a different choice of complementary conditions—in our example, we seek the wave function subject to the condition that a given grain is blackened. Here is a fairly close analogy: Imagine a telescope switched rapidly from one star to another, which is distant; all that has happened is a choice of the position of observation that is unrelated to any physical influences of the telescope on the stars or of one star on the other.

I hesitated before speaking of these long established truths, but it appears that even now one can meet among theoretical physicists a distorted picture of quantum mechanics. There recently arose a dispute about what is most important in quantum mechanics. One of the disputants asserted that it is the superposition principle. Another actively supported him. A third kept on repeating that the main thing is the Hilbert space and a vector in it. It was not possible to cool the ardor of the supporters of superposition by the example of classical electrodynamics, in which there is superposition but no quantum phenomena. It is obvious that we must first determine for what quantity we speak of superposition or a Hilbert space... .

The correct answer was given by the experimentalists: One said the most important thing was the probabilistic description, while another mentioned the uncertainty relation. And this last answer is perhaps the most general.

As we know, the nonlinearity of electrodynamics causes a violation of the superposition principle in strong fields. One could imagine a quantum theory in which this principle also ceases to hold under certain conditions for the wave function. But it is almost impossible to imagine a quantum theory in which there is violation of the uncertainty relation or the probabilistic interpretation of the wave function.

After these preliminary comments, we can turn to the discussion of the Bohr-Einstein debate.

5. Bohr-Einstein debate

Bohr and Einstein met for the first time in Berlin in spring 1920. Einstein was 41, Bohr 34. They had long known and esteemed each other. The personal encounter made a great impression on each of them. Soon after the meeting, Einstein wrote to Bohr: "Not often in life has a human being caused me such joy by his mere presence as you did." At the same time, he wrote to Ehrenfest: "Bohr was here, and I am as much in love with him as you are. He is like an extremely sensitive child who moves around in this world in a sort of trance." Bohr, in his turn, wrote to Einstein: "To meet you and speak with you was one of the greatest events of my life."

Pais recounts a discussion with Helena Dukas, who was for many years Einstein's secretary and after his death arranged the archive. She spoke of the intimate relationship between Bohr and Einstein: "They loved each other warmly and dearly." These relations continued throughout their life. The disputes did not cool the mutual love. In 1949, Einstein wrote in this connection: "It is only with brothers and close friends that one can have a real dispute; to others one is not close enough..."

The first scientific disagreements began (if one discounts Bohr's hostility to the idea of light quanta) with the Bohr-Kramers-Slater paper in 1924.[28] Einstein adopted a sharply negative position. We recall that he himself had had ideas about the violation of the conservation laws in individual events but had then rejected them after consideration.

It is an irony of fate that Bohr—the future creator of the principle of complementarity—attempted in his work up to 1925 to preserve classical electrodynamics, failing to understand that the wave-particle dualism discovered by Einstein in 1905 was the first example of complementarity. Later, when almost all physicists accepted the probabilistic interpretation of the wave function, Einstein reacted negatively to this interpretation, although he had himself in the 1916 paper[21] introduced for the first time transition probabilities... .

In October 1927, Bohr met Einstein at the Fifth Solvay Congress, at which all the creators of quantum mechanics were present. The participants of the congress were witnesses of how, everyday after breakfast, Einstein proposed to Bohr a new proof of the violation of the uncertainty relation in an experiment he had thought up. But by the evening of the same day, Bohr had shown that if the situation is considered carefully the uncertainty relation is confirmed.

They met again in 1930 at the Sixth Solvay Congress, devoted to magnetism. Neither the one nor the other visited the sessions, using all the time for discussions. Einstein believed that this time he had found an unanswerable example of the violation of Bohr's relation $\Delta E \cdot \Delta T \sim h$. Here is his thought experiment.

A box with radiation is placed on a balance. Within the box, there is a clock mechanism which for a short time opens a shutter that covers an opening in the box. The clock mechanism fixes the moment at which the shutter is opened. While the shutter is withdrawn from the opening, one quantum escapes from the box. From the difference in the weight m of the box before and after the escape the energy $E = mc^2$ of the quantum can be determined.

At first glance, the energy of the quantum can be measured with an arbitrarily small error $\Delta E = c^2 \Delta m$. Decreasing Δm and determining the time of escape from the clock, we see that Bohr's relation is violated.

Bohr explained the misunderstanding the following morning. I give his proof, since it is the most instructive of the thought experiments proposed by Einstein, and even more instructive is Bohr's explanation, which exhibits the style of his arguments.

Suppose the box is suspended on a spring and is attached to a pointer that can be displaced along a scale. The momentum transmitted by the force of gravity to the box during the time T of weighing is $p = Tgm$, where g is the acceleration due to gravity. The classical error $\Delta_1 p$ in the determination of this quantity due to the inaccuracy in the measurement of the mass is $\Delta_1 p = Tg\Delta m$. If the readings of the balance are to be sufficiently definite, the quantum uncertainty $\Delta p = h/\Delta q$, where Δq is the uncertainty in the co-

FIG. 2. L. A. Artsimovich, I. D. Rozhanskiĭ, N. Bohr, I. E. Tamm, and A. P. Aleksandrov. Moscow, May 1961 (from the archive of the I. E. Tamm Department of Theoretical Physics at the P. N. Lebedev Physics Institute).

ordinate of the box, must be less than the classical error $\Delta_1 p$. Thus, $Tg\Delta m > \Delta p = h/\Delta q$. But according to the general theory of relativity, a clock displaced in the direction of the force of gravity through a distance Δq changes its rate by an amount ΔT, the relation $\Delta T/T = g\Delta q/c^2$ holding.

From these relations, we obtain

$$\Delta E \cdot \Delta \tau = \Delta P_x \cdot \Delta x = \Delta P_y \cdot \Delta y = \Delta P_z \cdot \Delta z = h.$$

The uncertainty in the energy is related to the uncertainty in the time of escape by Bohr's relation. Bohr defeated Einstein with his own weapon—by using the theory of gravitation.

Ehrenfest, who was present at all the disputes and was a close friend of both, said to Einstein: "I am ashamed of you Einstein: You attack the new quantum theory in exactly the same way that your opponents attacked the theory of relativity."

Despite the love and mutual respect, the disputes were without compromise. When Einstein in the spirit of his philosophy proposed: "Let us first establish firmly those of your ideas that I can accept from my point of view and, proceeding from this basis, we can discuss logically further," Bohr answered in his style: "I would regard it as a treachery against science if I were to agree to establish anything firmly in this new field, in which everything is still unclear." Many years later Einstein said that Bohr always argued not as a man who knew the truth but as one eternally seeking it.

Even when Einstein finally came to feel that he could not find a weak point in the uncertainty principle or in the logic of quantum mechanics, he declared that this entirely consistent point of view contradicted his physical intuition

FIG. 3. N. Bohr, I. E. Tamm, and V. I. Veksler, Moscow, May 1961 (from the archive of the I. E. Tamm Department of Theoretical Physics at the P. N. Lebedev Physics Institute).

and, in his opinion, could not be the final solution: "...God does not play dice..."

In 1935, the dispute, which had died down, flared up again with the publishing of the paper by Einstein, Podolsky, and Rosen: "Can quantum-mechanical description of physical reality be considered complete?"[41] Suppose that two subsystems interact for a certain time and then become separated by a large distance. The authors note: "...Since at the time of measurement the two systems no longer interact, no real change can take place in the second system in consequence of anything that may be done to the first system." But according to quantum mechanics it is possible to change the wave function of the second system by means of measurements in the first... .

We examine this phenomenon in an extremely simple example, in which it is made trivial. Suppose we note the momenta of two particles before a collision, and suppose that after the collision one remains on the Earth, while the other travels to the Moon. If a terrestrial observer obtains a definite value of the momentum of the remaining particle, he can, using the momentum conservation law, calculate the momentum of the particle on the Moon. Therefore, the wave function of this particle is determined as a result of the measurement on the Earth, and it corresponds to a definite momentum.

If we regard the wave function as a physical field, this is complete nonsense. But if one bears in mind that the wave function is an information wave, the result is natural: It is the usual change in the probability of predictions that occurs when new information becomes available. We pose the question: What is the probability that the lunar experimentalist finds a particular value of the momentum under the subsidiary condition that on the Earth the momentum of the other particle has been found? This means that it is necessary to take the complete set of multiple measurements of the momentum in both laboratories and select from this set the cases when a given momentum was obtained on the Earth. Under this condition, the lunar measurements will give a definite and known momentum, in accordance with the momentum conservation law. The influence of measurements in one subsystem on the predictions about the behavior of the other subsystem must be understood precisely in the sense of selection of cases corresponding to a definite condition. Different subsidiary conditions force us to select a different sequence of events. It is clear that if the selection conditions are changed the wave function changes.

Two subsystems separated by large distances are in no way physically coupled; they are independent; however, the conditional probability depends of course on the state of one of the subsystems that we select. This phenomenon is also present in classical physics, indeed, even in everyday life. A prediction is changed abruptly when the conditions for selecting events are changed.

In the collection published to mark Einstein's 70th birthday, Bohr submitted the paper "Discussion with Einstein on epistemological problems in atomic physics."[42] He

FIG. 4. N. Bohr and L. D. Landau at the time of the student festival Archimedes at the main entrance of the Physics Faculty of the Moscow State University, May 1961 (from the Archimedes archive).

analyzes in it in detail Einstein's objections and his answers to them. In the conclusion, Bohr writes: "The discussions with Einstein which have formed the theme of this article have extended over many years which have witnessed great progress in the field of atomic physics. Whether our actual meetings have been of short or long duration, they have always left a deep and lasting impression on my mind, and when writing this report I have, so-to-speak, being arguing with Einstein all the time, even in discussing topics apparently far removed from the special problems under debate at our meetings..."

Essentially, this was a collision of two philosophies, two theories of knowledge—the lucid view of the old physics nurtured on classical mechanics and electrodynamics with their unique determinism, and a more flexible philosophy that absorbed into itself the new facts of the quantum physics of the 20th century and armed with the principle of complementarity.

Is it necessary to seek a different interpretation of quantum mechanics? Quantum mechanics, in conjunction with the theory of measurements, is a consistent and exceptionally beautiful theory. All attempts at its "improvement" have hitherto been found lacking and, in the best case, have been restricted to the following question: "How can one obtain the already known results of quantum mechanics less beautifully and more complicatedly?..."

As a result of the stormy disputes about the completeness of the quantum-mechanical description there arose an idea: Might it not be possible to explain the uncertainty in the behavior of an electron by the fact that its state depends not only on the momentum, coordinate, and spin projection but also on certain hidden parameters? Then the uncertainty of the result, as in statistical physics, arises from the arbitrariness in the value of these parameters. In principle, if the hidden parameters could be determined the predictions would be made definite, as in classical mechanics.

Of course, this is a very clumsy and unpleasant method—saving determinism by introducing extra variables. Particularly as at the beginning it was only possible to confirm already known quantum-mechanical relations.

For a single measurement, it was possible to achieve agreement with quantum mechanics by playing with hidden parameters. However, in the case of repeated measurements this is not always possible. The first measurement so restricts the range of the hidden parameters that their freedom in the second measurement is already insufficient to achieve agreement with quantum mechanics. This was shown most convincingly by John Bell in 1965.[43] For the proof, it was sufficient for him to show that the values of the hidden parameters in separated systems are independent. But these parameters were only introduced in order to avoid the probabilistic "dependence" of separated objects prescribed by quantum mechanics... .

Thus, Bell showed the experiments in which one could see a difference between the predictions of quantum mechanics and the theory of hidden variables. Such an experiment was made in 1972 by Freedman and Clauser.[44] They observed light emitted by excited calcium atoms. Under the conditions of their experiment, a calcium atom emitted successively two photons of visible light which could be distinguished by means of ordinary color filters. Each photon was detected by a corresponding counter, passing through a polarimeter that selected a definite direction of the polarization. They studied the number of coincidences of the counters as a function of the angle between the directions of polarization of the two photons. The theory of hidden variables predicted dips in the curve representing this dependence. The experiment revealed not only the absence of any dip but in fact a complete coincidence between the entire experimental curve and the theoretical curve obtained from quantum mechanics. Later, other more accurate experiments, also in agreement with quantum mechanics, were made.

Thus, the theory of hidden parameters, at least in its present form, contradicts experiment. Quantum mechanics has been confirmed yet again. But for all that the assertion that quantum mechanics is unshakable, particularly in the unexplored region of supersmall scales, would run counter to the spirit of Bohr's philosophy.

CONCLUDING REMARKS

The new physics required a new cast of mind and a new style of working. In classical physics, beginning with Newton, deep and perspicuous physical ideas preceded the completed theory. The creators of quantum theory moved forward without sure foundations, through confused guesses, which were gradually made more precise.

The most important quantitative results were obtained before their physical comprehension was achieved. Understanding developed as progress was made.

One further characteristic feature of the new style was the continual many sided discussion of all the obscurities and guesses. It was this that Einstein did not like. In this connection, Bohr said: "Quantum theory required discussion, but he was used to doing everything himself..."

To this new approach nothing could more closely correspond than the cast of mind and style of working of Niels Bohr.

One cannot admire the man too much—Niels Bohr, Citizen of the Earth. He made huge efforts to unite the international scientific community. From the first days of its existence, the embodiment of these efforts was the Bohr Institute at Blegdamsvej, where physicists of many countries worked and where they found refuge in difficult times for the world.

Many German physicists were forced to emigrate from Germany. Others became conformists and came to terms with fascism as an unavoidable evil. I asked Bohr what he thought about Jungk's book *Brighter Than a Thousand Suns*,[45] in which he described the journey of Heisenberg to Copenhagen and his encounter with Bohr in October 1941. Jungk writes that Bohr did not understand Heisenberg and that their relations had cooled. But Bohr said: "In this account, there is not a shadow of truth. I understood him excellently. He proposed to me collaboration with the Nazis... ." I sensed that the remarkably gentle and delicate Bohr became inflexible and even hard when the discussion turned to principles.

Even before Hiroshima, Bohr addressed the governments of the USA and Great Britain, sending memoranda; he crossed the ocean during the war in war planes, obtaining personal meetings to convince Churchill and Roosevelt of the danger of nuclear weapons. To Churchill's shame, he did not understand, or did not want to understand, that Bohr's activity was due solely to fear for the fate of mankind, and only the intervention of several physicists saved Bohr from arrest on a charge of espionage.

In his "Open letter to the United Nations" in 1950,[46] he spoke of the necessity of complete declassification of all studies in nuclear physics and the use of nuclear energy solely for peaceful purposes. His letter ends with the words:

"The efforts of all supporters of international co-operation, individuals as well as nations, will be needed to create in all countries an opinion to voice, with ever increasing clarity and strength, the demand for an open world."

Niels Bohr had a harmonious personality. He could express himself in all human manifestations, and he did more than a man could do. He could with justice say of himself with Hölderlin: "...What more could I wish if I were to live once like the Gods!"

[1)]Transl. editor's note. Many of the quotations in the Russian text could not be traced to their original sources, and are retranslated from the Russian translations, thus possibly deviating to some extent from the original text.

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