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(*b*. Copenhagen, Denmark, 7 October 1885; *d*. Copenhagen, 18 November 1962), *atomic and nuclear physics, epistemology*,

A tradition common to many pioneers in science has been the combination of achievement in actual discovery of natural laws with philosophical reflection on the nature of scientific thinking and the foundations of scientific truth. This combination is essential to such scientists in the sense that epistemological considerations played a decisive part in the success of their investigations and that, conversely, the results of the latter led them to deeper understanding of the theory of knowledge. Niels Bohr in particular was very conscious of this twofold aspect of his scientific activity, deep-rooted as it was in the environment in which he grew up and received his education.

The family in which Bohr was the second of three children belonged to the well-to-do intellectual circles of Copenhagen; his father, Christian Bohr, was a talented professor of physiology at the University of Copenhagen; his mother, Ellen Adler, Came from a wealthy Jewish family that was prominent in such varied activities as banking, politics, classical philology, and progressive pedagogy, The parents allowed the children’s native gifts the fullest development, and formal education was supplemented at every stage by example and encouragement at home. Niels was not as brilliant a pupil as his younger brother Harald, who became an eminent mathematician; they both, however, showed interests in other fields, including sports. At the University of Copenhagen, Niels stood out as an unusually perceptive investigator. His first research project, a precision measurement of the surface tension of water by the observation of a regularly vibrating jet, was completed in 1906, when he was still a student, and it won him a gold medal from the Academy of Sciences. It is a mature piece of work, remarkable for the care and thoroughness with which both the experimental and theoretical parts of the problem were handled.

Bohr’s doctoral dissertation, *Studier over metallernes elektrontheori* (1911), was a purely theoretical work that again exhibited a mastery of the vast subject he had chosen, the electron theory of metals. This theory, which pictures the metallic state as a gas of electrons moving more or less freely in the potential created by the positively charged atoms disposed in a regular lattice, accounted qualitatively for the most varied properties of metals; but it ran into many difficulties as soon as a quantitative treatment was attempted on the basis of then accepted principles of classical electrodynamics.

In order to throw light on the nature of these difficulties, Bohr developed general methods allowing him to derive the main features of the phenomena from the fundamental assumptions in a very direct way. He could thus clearly exhibit the fundamental nature of the failures of the theory, which were in fact attributed to an insufficiency of the classical principles themselves. Thus, he showed that the magnetic properties of the metals could in no way be derived from a consistent application of these principles. The rigor of his analysis gave him, at this early stage, the firm conviction of the necessity of a radical departure from classical electrodynamics for the description of atomic phenomena.

The study of physics, even carried to such unusual depth, did not absorb all of Bohr’s energy; his intellectual curiosity knew no bounds. With his characteristic earnestness and thoroughness he took up the hints that circumstances offered as starting points for highly original philosophical reflections. His father’s scientific work concentrated on the quantitative analysis of physical processes underlying the physiological functions; the school which he founded and which was brilliantly continued by his pupils still flourishes in modernized form, The type of problem that Christian Bohr was investigating required the closest attention to the elaboration of refined techniques of physical measurement, and simultaneously raised profound philosophical questions about the relationship between physical and biological phenomena.

During Niels’s adolescence, the philosophical trend in scientific circles was a reaction against the mechanistic materialism of the preceding generation. In the liberal at mosphere surrounding Christian Bohr’s friends, a group to which the philosopher herald Höffding belonged, this reaction took a moderate and thoughtful form, however. Bohr, the master of the investigation of the physical basis of the physiological processes, insisted on the practical necessity of considering these processes also from the teleological point of view in order to arrive at a complete description. Niels and Herald Bohr were admitted as silent listeners to the philosophical conversations of their father and his friends, and this first confrontation with the epistemological problem of biology, in which apparently conflicting views were found equally in dispensable for a full understanding of the phenomena, made a lasting impression upon Niels’s mind.

He also soon came to share the negative attitude of the progressive bourgeoisie, to which his family belonged, toward the church and religious beliefs in general; but it is characteristic of his independence of judgment that he arrived at this conclusion only after he had convinced himself that the church upheld doctrines that were logically untenable and shunned the pressing task, at the time preoccupying all liberal minds, of alleviating a still widespread pauperism. His approach to social and philosophical questions, even at such an early stage, was marked by the same logical rigor and breadth of vision as his scientific thinking.

It was in the course of his meditations on the human condition that, considering the role of language as a means of communication, he first came across a situation of great generality whose recognition was the source of his later decisive contribution to the epistemology of physics. He was struck by the fact that the same word is currently used to denote a state of consciousness and the concomitant behavior of the body. In trying to describe this fundamental ambiguity of every word referring to mental activity, Bohr had recourse to an analogy drawn from the mathematical theory of multivalued functions: each such word, he said, belongs to several "planes of objectivity", and we must be careful not to allow them to glide from one plane of objectivity to another. However, it is an inherent property of language that there is only one word for the different aspects of a given psychical activity. There is no point in trying to remove such ambiguities; we must recognize their existence and live with them.

After finishing his studies in Copenhagen, Bohr went to Cambridge, hoping to pursue his work on electron theory under the guidance of J.J. Thomson. Unfortunately, Thomson had lost interest in the subject, and failed to appreciate the importance of Bohr's dissertation, which the latter showed him in an English translation he had been at great pains to make; this was turned down by the Cambridge Philosophical Society as too long and too expensive to print, and Bohr's further attempts to get it published were equally abortive.

This grievous disappointment did not prevent Bohr from making the most of his stay in Cambridge, but as soon as he conveniently could, he moved to Manchester, where Ernest Rutherford had established a flourishing laboratory. There, from March to July 1912, working with utmost concentration, he laid the foundations of his greatest achievement in physics, the theory of atomic constitution. It would be difficult to imagine two temperaments more different than those of Bohr and Rutherford; but this first contact initiated, besides a new epoch in science, a lifelong friendship, compounded of filial affection on Bohr's part and of warm cordiality, tinged with respect, on the part of the jovial New Zealander. With his shrewd judgment of people, Rutherford soon sensed the genius in the shy, unassuming young man, and his immense strength, imaginative insight, and directness of approach were an inspiration to Bohr.

Toward the end of 1910, Rutherford had proposed a "nuclear" model of the atom in order to account for the large-angle scattering of α rays observed in his laboratory. Since the discovery of the election as a carrier of an elementary unit of negative electric charge, the atom was thought of as a system of a certain number of electrons, kept together by an equivalent positive charge, somehow attached to the massive substance of the atom (the electron itself being nearly two thousand time lighter than the lightest atom). If this positive charge were spread over the whole atom, the α rays, or positively charged helium atoms, impinging upon it would generally undergo small deviations from their courses; the frequent occurrence of large-angle deviations suggested direct collisions with a strongly concentrated positive substance. A quantitative check fully confirmed this inference and revealed that the massive, positively charged nucleus of the atom had linear dimensions a hundred thousand times smaller than those of the whole atomic structure.

Bohr eagerly took up the new model and soon recognized its far-reaching implications. In particular, he pointed out that the nuclear model of the atom implied a sharp separation between the chemical properties, ascribed to the peripheral electrons, and the radioactive properties, which affected the nucleus itself. This immediately suggested a close relation between the atomic number, which indicates the position of an element in Mendeleev's periodic table, and the number of its electrons, or its nuclear charge, which should thus be more fundamental than its atomic weight. Indeed, the Periodic table showed one or two irregularities in the sequence of atomic weights, and it became increasingly difficult to accommodate in it the newly discovered radioactive products; Bohr showed how all these anomalies could be eliminated if one admitted the occurrence of atomic nuclei of the same charge but different mass, so that there could be more than one species of atom occupying the same place in the periodic table. Somewhat later, the name "isotope" was given to these chemically indistinguishable atomic species of different weights.

According to the nuclear model, radioactive transformations had to be conceived as actual transmutations of the atomic nucleus. Thus, Bohr argued, by the emission of an α ray, the nucleus lost two units of charge and became an isotope of the element two places back in the periodic table. In β decay, on the other hand, the emission of an electron resulted in the gain of one unit of charge, and the product nucleus occupied the next higher place in the periodic table. Simple as it may seem, the inference leading to these "displacement laws" of radioactive elements was far from obvious at that time.

The only person in the laboratory who followed Bohr's thoughts with deep interest and genuine understanding, and who was able to help him in the discussion of the empirical information, was a young Hungarian chemist, Georg von Hevesy, who was himself on the verge of discovering the use of isotopes as tracers, which brought him fame. Indeed, Rutherford himself, insensible to the logical cogency of Bohr's argument, dissuaded him from publishing such hazardous deductions from his own atomic model, to which he was not prepared to ascribe the fundamental significance that Bohr gave it; and when, a few months later, the displacement laws could be discerned by mere inspection of the accumulated experimental evidence, Kasimir Fajans (one of those who then enunciated them) so little understood their meaning that he actually presented them as evidence against the Rutherford atomic Model.

Bohr's survey of the implications of Rutherford's atomic model did not stop at the recognition of the existence of a relation between the atomic unmber (which summarizes the whole physicochemical behavior of the element) and the number of electrons in the atom. He resolutely attacked the much harder problem of determining the exact nature of this relation, which amounts to a dynamic analysis of the atomic structure represented by the nuclear model. Following J.J. Thomson's example, bohr assumed that the electrons would be symmetrically distributed around the nucleus in concentric circular rings. He had then to face the problem, not present in Thomson's model, of how to account for the stability of such ring configurations, which could not be maintained by the electrostatic forces alone.

Bohr had become convinced, form his study of the behavior of electrons in metals, that the validity of classical electrodynamics would be subject to a fundamental limitation in the atomic domain, and he had no doubt that this limitation would somehow be governed by Planck's quantum of action; he knew already how to quantize the motion of a harmonic oscillator, i.e., to select from the infinity of possible motions a discrete series characterized by energy values increasing by finite steps of magnitude hv, where h is Planck's universal constant and v the frequency of the oscillator. One could try to apply a similar quantization to the motions of an atom's electrons, whose frequencies might be identified with the resonance frequencies observed in the scattering of light by the atom.

Thus, an allowed state of motion characterized by a frequency ω^n would have a binding energy of the from $W_n = Knh\omega_n$, where n is an integer numbering the state and K is some numerical factor that could possibly depend on the type of motion. Such a formula could be combined with the relation given by the classical theory between the binding energy and the amplitude of the motion, in order to obtain a relation between the amplitude of motion, whose order of magnitude is known from various evidence about the atomic dimensions, and the corresponding resonance frequency, which is obtained from optical measurements. It was easy to ascertain that the numerical value of Planck's constant, entering such a relation, did lead to the expected orders of magnitude; but this rough check, however encouraging, was clearly insufficient to establish the precise form of the quantum condition.

At this juncture, Bohr obtained a much deeper insight into the problem by a brilliant piece of work, which he—working, as he said, "day and night"—completed with astonishing speed. The problem was one of immediate interest for Rutherford's laboratory: in their passage through a material medium, α Particles continually lose energy by ionizing the atoms they encounter, at a rate depending on their velocity. This energy loss limits the depth to which the particles can penetrate into the medium, and the relation between this depth, or range, and the velocity offers a way of determining this velocity. What Bohr did was to analyze the ionizing process on the basis of the Rutherford model of the atom and thus express the rate of energy loss in terms of the velocity by a much more accurate formula than had so far been achieved—a formula, in fact, to which modern quantum mechanics adds only nonessential refinements.

Bohr's interest in atomic collision problems never faltered. In the early 1930's, when the modern theory of these processes was being elaborated, especially by Hans Bethe, Felix Bloch, and E. J. Williams, he took an active part in the work, a good deal of which took place in Copenhagen; and as late as 1948 he wrote a masterly synthesis of the whole subject, in which one still finds, in modernized form, the arguments of his early analysis.

The success of this analysis showed him, however, that the classical theory, while completely failing to account for the stability of the periodic motions of the atomic electrons, could deal with undiminished power with the aperiodic motions of charged particles traversing a region in which there is an electric field. This means that, however radical the break with classical ideas implied by the existence of the quantum of action, one must expect a gradual merging of the quantum theory into the classical one for motions of lower and lower frequencies. Moreover, one may expect that the effect of a very slow and gradual modification of the forces acting on or within an atomic system will be correctly estimated by the classical theory.

These were shrewd points, which Bohr used skillfully and which he eventually developed into powerful heuristic principles. An immediate application of the second principle helped him to discuss simple models of atomic and molecular structures, which reproduced, at least in order of magnitude, a number of features derived from various experiments and thus further illustrated the fruitfulness of the Rutherford atomic model. Indeed, this model was the first to permit a clear-cut distinction to be made between atom and molecule—a molecule being defined as a system with more than one nucleus—and thereby to open the way to an understanding of the nature of chemical binding. The models studied by Bohr were characterized by the arrangement of the electrons in one or more ring configurations, disposed around the nucleus as the common center in an atom, or symmetrically with respect to the nuclei in molecules. While the absolute dimensions of these configurations depended on quantum conditions that he could only roughly guess, their stability, owing to the argument mentioned above, could be examined by classical methods; thus, he could explain why hydrogen could form a diatomic molecule, while helium could not.

Although these considerations were crude—and are completely superseded by the modern conceptions— they were remarkably successful; in fact, they do embody an important feature of the chemical bond that is part of the modern theory: the fact that this bond is due to the formation of a configuration of electrons shared by the combining atoms. The hydrogen molecule, for instance, was well represented by a ring of two electrons perpendicular to the line joining the two nuclei.

With regard to the determination of the states of motion allowed by the quantum condition mentioned above, Bohr found that the Rutherford model leads to remarkably simple results, at least for the type of configuration he considered. In general, the classical theory of the motion furnishes an additional relation between the binding energy and the frequency, which allows one to eliminate the frequency from the quantum condition and thus obtain for the binding energy W_n an expression depending only on the integer n, with a coefficient that, besides Planck's constant, contains the parameters characterizing the system and the type of motion. Thus, to take the simplest example of the hydrogen atom, consisting of a singly charged nucleus and an electron of mass m and charge e, the classical theory shows that there is proportionality between W^3_n and ω^2_n; this leads, for the allowed states of binding, to the very simple law $W_n = A/n^2$, and the precise value of the coefficient A is $\pi^2 e^4 m/2K^2 h^2$; only the numerical factor K remains in doubt.

When he left Manchester in July 1912, Bohr was filled with ideas and projects for further exploration of this world of atoms that was displaying such wide prospects; but he had another reason to be in high spirits. Since 1911, shortly before his departure for England, he had been engaged to Margrethe Nørlund, a young woman of great charm and sensibility. The marriage took place in Copenhagen on I August 1912 and was a happy and harmonious union. Margrethe's role was not an easy one. Bohr was of a sensitive nature, and constantly needed the stimulus of sympathy and understanding. When children came—six sons, two of whom died young—Bohr took very seriously his duties as paterfamilias. His wife adapted herself without apparent effort to the part of hostess, and evenings at the Bohr home were distinguished by warm cordiality and exhilarating conversation.

In the autumn of 1912, Bohr took up the duties of assistant at the University of Copenhagen; he fulfilled them conscientiously, and used the privilege extended to holders of the doctorate of giving a free Course of lectures. At the same time, he started to write up the account of his Manchester ideas. Then, at the beginning of 1913, the orientation of his thought took a sudden turn toward the problem of atomic radiation, which rapidly led him to the decisive step in the process of incorporating the quantum of action into the theory of atomic constitution. The rest of the academic year was spent reconstructing the whole theory upon the new foundation and expounding it in a large treatise, which was immediately published, in three parts, in the *Philosophical Magazine*.

It had been known since kirchhoff's pioneering work that the spectral composition of the light emitted by atoms is characteristic for the chemical species; a whole science of spectroscopy had developed on this principle and a great deal of extremely accurate material had been accumulated. Obviously, the tables of wavelengths of the characteristic spectral lines must contain very precise information on the structure of the emitting atoms; but since atomic spectra consist of apparently capricious sequences of thousands of lines, it seemed hopeless to try to decipher such complicated codes. It therefore came as a great surprise to Bohr to learn from a casual conversation with a colleague that spectroscopists had managed to discover regularities behind the chaos.

In particular, J. R. Rydberg, of the nearby University of Lund, had found a very simple and remarkable formula expressing the frequencies of several "series" of spectral lines which recurred, with different atoms. The striking feature of Rydberg's formula was that the frequencies were represented by differences of two terms, each of which depended in a simple way on a number which could take a sequence of integral values; a series corresponded to the sequence obtained by keeping one of the terms fixed and varying the other.

Thus, the frequencies v_{nm} of the lines of the hydrogen spectrum could be represented in the simplest possible from in terms of two integers as

with a single parameter, R, of accurately known numerical value. As soon as Bohr saw this formula, he immediately recognized that it gave him the missing clue to the correct way to introduce the quantum of action into the description of atomic systems.

The formal similarity between the terms of the Rydberg formula R/n^2 and the expression for the energies $W_n = A/n^2$ of the possible stationary states of the atom suggested to him, in the spirit of Planck's conception of the quanta of radiation, that the emission by the atom of light of frequency v_{nm} occurred in the form of single quanta of energy hv_{nm}; Rydberg's formula then indicated that in this process the atom passed from an initial stationary state W_n to another stationary state, W_m. An immediate control of this interpretation offered itself: according to it, the value of Rydberg's constant should be given by $Rh = A$, that is, by $R = \pi^2 e^4 m/2K^2 h^3$. Inserting in this expression the known values of e, m, and h, and taking the value 1/2 for k (which would give the correct binding energy W_n for a harmonic oscillator of frequency ω_n), Bohr obtained a value of R as near the experimental one as the errors in the determinations of the other constants allowed.

However convincing such a stringent quantitative test could appear, there was in this new conception of the radiation process a feature that must be considered so unusual as to be almost unthinkable: the frequencies $v_{n\,m}$ of the emitted light did not coincide with any of the allowed frequencies of revolution ω_n of the electrons or their harmonics—a feature of the classical theory of radiation so immediate and elementary that it seemed impossible to abandon it.

That Bohr was not deterred by this consideration was due essentially to the dialectical turn of mind he had acquired in his youthful philosophical reflections. The conflict between the classical picture of the atomic phenomena and their quantal features was so acute that no hopes (such as those Planck was still expressing) could be entertained of solving it by reducing the latter to the former; one had, rather, to accept the coexistence of these two aspects of experience, and the real problem was to integrate them into a rational synthesis. Bohr later said that the clue offered by Rydberg's formula was so transparent as to lead uniquely to the quantal description of the radiation process he proposed; this gave him the conviction that it was right, in spite of the radical break with classical ideas that it implied.

In order to clinic the argument, however, Bohr went a very important step further. He knew that the quantal behavior of a system, whatever it was, had to satisfy the requirement of going over to the corresponding classical behavior in the limiting case of motions involving large numbers of quanta of action. Applying this test to his interpretation of Rydberg's formula, Bohr found that the condition could be fulfilled only by ascribing the value 1/2 to the numerical coefficient K, for which the right value of Rydberg's constant was obtained. Indeed, for large values of the number n, the frequencies $v_{n.n+p}$ are then seen to tend to the values of the frequency of revolution, $\omega_n = 2R/n^3$, and its successive harmonics, $\varrho\omega_{\mathrm{n}}$. Thus, as Bohr expressed it, "the most beautiful analogy" was established—in the sense just indicated—between classical electrodynamics and the quantum theory of radiation.

In his great papers of 1913, Bohr presented his theory as being founded upon two postulates, whose formulation he refined in later papers. The first postulate enunciates the existence of stationary states of an atomic system, the behavior of which may be described in terms of classical mechanics; the second postulate states that the transition of the system from one stationary state to another is a nonclassical process, accompanied by the emission or absorption of one quantum of homogeneous radiation, whose frequency is connected with its energy by Planck's equation. As for the principle by which the possible stationary states are selected, Bohr was still very far from a general formulation; indeed, he was keenly aware of the necessity of extending the investigation to configurations other than the simple ones to which he had restricted himself. The search for sufficiently general quantum conditions defining the stationary states of atomic systems was going to be a major problem in the following period of development of the theory.

A statement in Bohr's first paper gave rise to a controversy that soon ended in triumph for the new theory and in no small degree contributed to its swift acceptance. On the strength of his interpretation of Rydberg's formula, Bohr had pointed out that a certain series of lines attributed to hydrogen ought actually to be ascribed to helium: it had been fitted to the formula for hydrogen with half-integral values of the numbers n, m; in Bohr's view, which required integral values for these numbers, this could only mean that the Rydberg constant for this series was four times that for hydrogen, corresponding to a doubly charged nucleus. The experienced spectroscopist Alfred Fowler received the suggestion with understandable skepticism, but control experiments, which were at once performed in Rutherford's laboratory, confirmed Bohr's prediction. Fowler's last-ditch resistance, in the form of the pointed objection that Rydberg's coefficient for the contested series was not exactly 4R (R being the hydrogen value), was brilliantly countered by Bohr: he showed that the slight difference was to be expected as an effect of the motion of the nucleus, which he had neglected in his first approximation.

There is no doubt that this dramatic incident was decisive in convincing Rutherford and Fowler that there was something after all in this young foreigner's theorizing. This was also James Jeans's attitude when, in the report of Bohr's work he gave at the British Association meeting at Birmingham in September 1913, he pointed out that the only justification of Bohr's postulates "is the very weighty one of success." At Göttingen, that center of mathematics and physics, where the sense of propriety was strong, the prevailing impression was one of scandal, or at least bewilderment, in the face of the undeserved success of such high-handed disregard of the canons of formal logic; but the significance of Bohr's ideas did not escape those who had themselves most searchingly pondered the problems of quantum theory. Albert Einstein and Arnold Sommerfeld.

No one realized more keenly than Bohr himself the provisional character of his first conclusions, and above all the need for a deeper analysis of the logical relationship between the classical and quantal aspects of the atomic phenomena that were embodied in the two postulates. At the same time, he was faced with an overwhelming program of generalizing the theory and unfolding all its consequences. He was naturally more and more dissatisfied with his job at the university, which left him little time for research and (since he had mainly to deal with medical students) little hope of turning out pupils able to assist him in his work.

The academic authorities were slow in realizing that an exceptional situation had arisen, and when Rutherford offered him a lectureship in Manchester, Bohr was glad to avail himself of the opportunity to pursue his work under the most favorable conditions. He remained in Manchester for two years. In the meantime, the Danish authorities had moved to offer Bohr a professorship, which he accepted; and three years later, thanks to the active intervention of a group of friends, who donated the ground, they were at last persuaded to build Bohr a laboratory: this was the famous Institute for Theoretical Physics, of which he was director for the rest of his life. The founding of the institute came just in time to keep Bohr in his native country, for Rutherford, who had just been called to the directorship of the Cavendish Laboratory in Cambridge, had already invited Bohr to join him.

The new institute was meant to be primarily a physical laboratory; what was termed "theoretical physics" would now be called "fundamental physics." Bohr did not draw any sharp distinction between theoretical and experimental research; on the contrary, he visualized these two aspects of research as supporting and inspiring each other, and he wanted the laboratory equipped so as to make it possible to test new theoretical developments or conjectures by appropriate experiments. He managed to put this conception into effect; the experimental investigations carried our at the institute have not been numerous, but have always been of high quality— some of them, indeed, of pioneering importance—and all have been directly relevant to the theoretical questions under consideration. In order to keep up with the changing outlook of current theory, it was imperative to expand and even to renew the experimental equipment in order to adapt it to entirely new lines of research; this Bohr did with remarkable foresight as well as persuasive tenacity in securing the necessary funds.

Bohr's atomic theory inaugurated two of the most adventurous decades in the history of science, a period in which the efforts of the history of science, a period in which the efforts of the elite among the younger generation of physicists were concentrated on the numerous problems raised by the theory and on experimental investigations that further stimulated the theoretical developments or provided the required proof of theoretical predictions. Three experimental advances that furthered the progress of the theory were made as early as 1913 and 1914. The domain of X-ray spectroscopy was opened up by H. G. J. Moseley's brilliant work in Manchester, and its significance for atomic theory, on the basis of Bohr's ideas, was pointed out by Walther Kossel. The experiments of James Franck and Gustav Hertz on the excitation of radiation from atoms by collisions with electrons, and those of Johannes Stark on the modification of the atomic spectra by strong electric fields, offered a new approach to the study of the dynamical behavior of atomic systems; their interpretation was soon outlined by Bohr himself.

Optical spectroscopy, whose importance had been suddenly enhanced, was actively developed, especially by the school established at Tübingen under Friedrich Paschen's leadership; with his collaborators Ernst Back, Alfred Landé, and others, Paschen analyzed in great detail the fine structure of the line spectra and the further splitting of the lines under the action of magnetic fields of increasing strength, and he formulated the regularities obeyed by the frequencies and intensities of the lines in terms of sets of quantum numbers attached to the spectroscopic terms and taking integral or half-integral values.

On the theoretical side, too, the scene was rapidly changing. The isolation in which Bohr had hitherto found himself gave way to a lively collaboration with a growing number of fellow workers all striving toward the common goal, freely exchanging ideas, discussing results and conjectures, sharing the thrill of success and the expectation of further progress. By tacit consent, Bohr was the leader to whom all turned for guidance and inspiration. There were other great schools of theoretical physics, the foremost being those newly established by Sommerfeld in Munich and by Max Born in Göttingen; they pursued their own lines of research, always keeping in close contact with the Copenhagen group. The first to join Bohr in Copenhagen was a young Dutchman, H. A. Kramers, who arrived in 1916 and for the next ten years was Bohr's tireless assistant and talented collaborator. During this period, many others came to Bohr's institute; among them was Bohr's faithful friend Hevesy, as well as younger men— Oskar Klein, Wolfgang Pauli, and Werner Heisenberg.

The first of the main problems requiring consideration was the generalization of the quantum conditions defining the stationary states. Bohr did not at first attempt to make use of the general methods of classical mechanics; this was not his way of tackling problems. He preferred to handle concrete cases and to develop ingenious arguments which, although lacking generality, had the advantage of clearly bringing out the physical features of essential importance. In the present instance, he again started from the premise that slow deformations of a system would not change its quantal state, and developed it into a principle of mechanical transformability, which proved quite efficient within a limited scope. The idea was to transform one type of motion continuously into another by slow variation of some parameter; if the determination of the stationary states had been accomplished for one of the two motions, one could derive stationary states, by such a transformation, for the other. To this end, one could take advantage of the existence of dynamical quantities, the adiabatic invariants, which have the property of remaining unchanged under slow mechanical transformations.

As early as 1911, Paul Ehrenfest had emphasized the important role played by adiabatic invariants in the quantum theory of radiation in thermodynamic equilibrium; but neither he nor Bohr at first succeeded in extending this conception to modes of motion more complicated than simple periodic ones. Decisive progress in this problem was made by Sommerfeld, who at the end of 1915 succeeded in formulating a full set of quantum conditions for the general Keplerian motion, including even the relativistic precession of the elliptic trajectory. Sommerfeld's work not only supplied an explanation (a partial one as it turned out) of the fine doublet structure of the lines of the hydrogen spectrum, but showed the way to the desired generalization of the rules of quantization to more complex atomic systems, whose motions were not simply periodic.

Bohr eagerly followed this new line of attack; he now made full use of the powerful methods of Hamiltonian dynamics, especially in the form adapted to the wide class of motions known as multiply periodic, to which the motions of the electrons in atoms belonged. It was fortunate that Kramers, skilled in the relevant techniques, was at hand to help him; even so, it took years of strenuous effort to bring the work to completion. In their general from, the quantum conditions stated that a certain set of adiabatic invariants should be integral multiples of Planck' constant; but in the process of establishing this result, a formidable hurdle was the occurrence of “degeneracies” of the motions into simple periodic ones, which led to ambiguities in the formulation of the corresponding quantum conditions. This difficulty was eventually overcome by another ingenious application of the principle of mechanical transformability.

The theory of multiply periodic systems offered the possibility of a more rational treatment of the question which Bohr had tackled in his very first reflections on the nuclear atomic model: the gradual building up of the periodicities in the atomic structures revealed by Mendeleev' table. The starting point was the consideration of the individual stationary orbits of each single electron in the electrostatic field of the nucleus, “screened” by the average field of the other electrons; the residual interaction of the electrons could then be treated by the perturbation methods originally developed for use by astronomers. For those spectra originating from quantum transitions of a single electron, usually the most weakly bound one, the quantum conditions provided a characterization directly comparable with the specification of the spectroscopic terms by quantum numbers.

The confrontation of the theory with the relevant spectroscopic evidence led to partial success: the main features of the empirical term sequences were well reproduced by the theory, and the spectroscopic quantum numbers on which these features depended accordingly acquired a simple mechanical interpretation (except for the occurrence of half-integralvalues, which appeared as an arbitrary modification of the quantum conditions); but the finer structure of the term sequences presented a complexity for which the atomic model offered no mechanical counterpart.

In spite of this imperfection, the model could be expected to give reliable guidance at least in the investigation of the broader outlines of atomic structures. The primitive ring configurations of Bohr's previous attempt were now replaced by groupings of individual electron orbits in "shells" specified by definite sets of quantum numbers, according to rules that were inferred from the spectroscopic data. This conception of the shell structure of atomic systems did not merely account for the main classification of the stationary states; its scope could be extended to include the interpretation of the empirical rules established by the spectoscopists for the empirical rules established by the spectroscopists for the intensities of the quintal transitions between these states. This was a much more difficult problem than that of the formulation of quantum conditions for the stationary states; the complete breakdown of classical electrodynamics, reflected in Bohr's quantum postulates, seemed at first to remove the very foundation on which a comprehensive theory of atomic radiation could rest. It was in taking up this challenge that Bohr was led to one of his most powerful conceptions: the idea of general correspondence between the classical and the quantal descriptions of the atomic phenomena.

Bohr seized upon the only link between the emission of light in a quantal transition and the classical process of radiation: the requirement that the classical description should be valid in the limiting case of transitions between states with very large quantum numbers. If the atom were treated as a multiples, each occurring with a definite amplitude; it was indeed possible to verify that the frequencies of quantal transitions between states of very large quantum numbers tended to become equal to those multiples of the classical frequencies given by the differences between these quantum numbers; in the limit of large quantum numbers, then, the classical amplitudes could be used directly to calculate the intensities of the quantal transitions. Bohr boldly postulated that such a correspondence should persist, at least approximately, even for transitions between states of small quantum numbers; in other words, the amplitudes of the harmonics of the classical motion should in all cases give an estimate of the corresponding quintal amplitudes.

The power of this correspondence argument was immediately illustrated by the application Kramer's made of it, in a brilliant paper, to the splitting of the hydrogen lines in an electric field. Not only did the correspondence argument, for want of a more precise formulation, play an indispensable part in the interpretation of the spectroscopic data, but it eventually gave the decisive clue to the mathematical structure of a consistent quantum mechanics.

By 1918 Bohr had visualized, at least in outline, the whole theory of atomic phenomena, whose main points have been presented in the preceding sections. He of course realized that he was still very far from a logically consistent framework wide enough to incorporate both the quantum postulates and those aspects of classical mechanics and electrodynamics that seemed to retain some validity. Nevertheless, he at once started writing up a synthetic exposition of his arguments and of all the evidence upon which they could have any bearing; in testing how well he could summarize what was known, he found occasion to check the soundness of his ideas and to improve their formulation. In the present case, however, he could hardly keep pace with the growth of the subject; the paper he had in mind at the beginning developed into a four-part treatise, "On the Theory of Line Spectra," publication of which dragged over four years without being completed; the first three parts appeared between 1918 and 1922, and the fourth, unfortunately, was never published. Thus, the full impact of Bohr's views remained confined to the small but brilliant circle of his disciples, who indeed managed better than their master to make them more widely known by the prompter publication of their own results.

Bohr's theory of the periodic system of the elements, based essentially on the analysis of the evidence of the spectra, renewed the science of chemistry by putting at the chemists' disposal rational spectroscopic methods much more refined than the traditional ones. This was dramatically illustrated in 1922, by the identification, at Bohr's institute, of the element with atomic number 72. This discovery was made by Dirk Coster and Hevesy, under the direct guidance of Bohr's theoretical predictions of the properties of this element; they gave it the name "hafnium," from the latinized name of Copenhagen. The conclusive results were obtained just in time to be announced by Bohr in the address he delivered when he received the Nobel Prize in physics for that year.

There was never any question of Bohr's resting on his well-deserved laurels. He did not allow the apparent triumph of the quantum theory of atomic systems to mislead him into believing that the model used to describe these systems—simple point charges interacting by electrostatic forces according to the laws of classical mechanics—bore any close resemblance to reality. In fact, the fine structure of the spectroscopic classification manifested an essential insufficiency of this model, whose nature was not yet elucidated; but above all, the peculiar character of the correspondence between the quantal radiation processes and their classical counterpart strongly suggested that the classical model was no more than an auxiliary framework in the application of quantum conditions and correspondence considerations.

After Kramers had succeeded in extending the scope of the correspondence argument to the theory of optical dispersion—thus rounding off a treatment of the interaction of atomic systems with radiation that accounted for all emission, absorption, and scattering processes—Bohr ventured to propose a systematic formulation of the whole theory, in which what he called the virtual character of the classical model was emphasized. In this he was aided by Kramers and a young American visitor, J. C. Slater, and the new theory was published in 1924 under the authorship of all three. The most striking feature of this remarkable paper, "The Quantum Theory of Radiation," was the renunciation of the classical form of causality in favor of a purely statistical description. Even the distribution of energy and momentum between the radiation field and the "virtual oscillators" constituting the atomic systems was assumed to be statistical, the conservation laws being fulfilled only on the average. This was going too far: the paper was hardly in print before A. H. Compton and A. W. Simon had established by direct experiment the strict conservation of energy and momentum in an individual process of interaction between atom and radiation.

Nevertheless, this short-lived attempt exerted a profound influence on the course of events; what remained after its failure was the conviction that the classical mode of description of the atomic processes had to be entirely relinquished.

This conviction was strengthened by the outcome of the other line of investigation most actively pursued in Copenhagen in these years, the search for the missing dynamic element of the atomic model. Pauli approached this arduous problem by trying to unravel the spectroscopic rules governing the fine structure of the terms and the splitting of the spectral lines in an external magnetic field—the anomalous Zeeman effect. He at length recognized that the entire problem could be simplified by attributing to the individual stationary states of each electron an additional quantum number, susceptible to two values only and combining with the other quantum numbers according to definite rules.

This conclusion at once threw light on the systematics of the shell structure of the elements, which Bohr had left incomplete, but which had lately been improved by E. C. Stoner. In fact, Pauli was able, in 1925, to formulate the simple underlying principle of this systematics: each stationary state—including the specification of the new quantum number—cannot be occupied by more than one electron. This exclusion principle has since received considerable extension, and has in fact turned out to be one of the most fundamental in nature. In the same year, decisive progress was made in the interpretation of the new quantum number by two of Ehrenfest's young pupils, S.A. Goudsmit and G.E. Uhlenbeck: they pointed out that the new quantum number could be ascribed to a proper rotation, or spin, of the electron, and that an intrinsic magnetic moment, related to the spin, could then account for the anomalous Zeeman effect. However, the quantization of the spin was at variance with that expressed by the quantum conditions; this circumstance, as well as the exclusion principle, which obviously was quite unaccountable in classical terms, showed in the most striking fashion that not only the radiation field but also the atomic constituents were out of reach of the conceptions of classical physics.

The crisis to which the attempt to treat the atom as a classical dynamic system had led did not last long. By the summer of 1925 Heisenberg had found the clue to the construction of a consistent mathematical scheme embodying the quantum postulates. This momentous progress was the direct outcome of the investigation of the optical dispersion theory initiated by Kramers. Heisenberg had taken an active part in this work and had been much impressed by the stand taken by Bohr, Kramers, and Slater. If classical conceptions could no longer be relied upon to supply at least a framework for the quantum theory, he concluded, what must be looked for is an abstract formal scheme expressing only relations between directly observable quantities, like the stationary states and the amplitudes whose absolute squares should express the probabilities of quantal transitions between these states. The correspondence between classical and quantal amplitudes established in the theory of dispersion, envisaged from this point of view, took the shape of a set of algebraic rules that these quantal amplitudes had to obey and that defined an algorism adapted to the rational formulation of laws of motion and quantum conditions, aswell as the precise calculation of radiation amplitudes.

Heisenberg's program was eagerly taken up in Göttingen, where Born immediately recognized that the noncommutative algebra involved in Heisenberg's relations was the matrix calculus; at the same time, a young Cambridge physicist, P.A.M. Dirac, was developing even more abstract and elegant methods. While in the high places of mathematics the formal scheme of the new quantum mechanics was thus being built up, a more critical attitude prevailed in Copenhagen. Pauli pointed out that by limiting the observable quantities to stationary states and radiation amplitudes, Heisenberg was unduly restricting the scope of the theory, since it was an essential part of the correspondence argument that the new theory should contain as limiting case, for large quantum numbers, the more detailed description of the motion in classical terms.

The fulfillment of this essential requirement necesistated a considerable extension of the mathematical framework of the theory, allowing it to accommodate both discontinuous and continuous aspects of the atomic phenomena. The decisive contribution was unexpectedly made by the "outsiders," Louis de Broglie and Erwin Schrödinger, who were exploring the conjecture that the constituents of matter might, like radiation, be governed by a law of propagation of continuous wave fields.

Although the idea in this one-sided form was at once seen to be untenable, it nevertheless provided the missing element; as Born especially emphasized, the wave fields associated with the particles give the probability distributions of the variables specifying the state of motion of these particles. Thus, the required formal completion of quantum mechanics could be carried out at the beginning of 1927, when Dirac indicated the most general representation of the operators belonging to the physical quantities, and the way to pass at will from any representation to any other according to definite prescriptions which guaranteed the fulfillment of all correspondence requirements. However, such classical features of the motion of particles as a sequence of positions forming a uniquely determined trajectory appeared only as a limiting case of a more general mode of description that was essentially statistical.

The quantum conditions were found to impose a peculiar restriction on the statistical distributions of the values of physical quantities. If, as a consequence of these conditions, the operators representing two such quantities do not commute, the average spreads in the assignment of the values they may take under given circumstances are reciprocal; their product exceeds a limit that depends on the degree of non-commutation and its proportional to Planck's constant. Thus, if in definite experimental circumstances the position of an electron, relative to some fixed frame of reference, is confined within narrow limits, its momentum will have a correspondingly wide range of possible values, each with its definite probability of occurrence, depending on the experimental conditions.

Heisenberg, who in 1927 discovered these remarkable indeterminacy relations, relaized their epistemological significance. In fact, the novelty of quantum mechanics in this respect is that it allows for the possibility of using all classical concepts, even though their precise determinations may be mutually exclusive—as is the case with the concept of a particle localized at a point in space and time, and that of a wave field of precisely given momentum and energy, whose space-time extension is infinite. Indeterminacy relations between such concepts, then, indicate to what extent they may be used concurrently in statistical statements. Heisenberg saw that the origin of these reciprocal limitations must lie in quantal features of the processes in which the quantities in question are observable, and he attempted to analyze such idealized processes of observation from this point of view.

This was the occasion for Bohr to reenter the scene. His role so far had been to inspire and orient the creative efforts of the younger men, especially Heisenberg and Pauli, and he could legitimately consider the new theory as the attainment of the goal toward which he had so long been striving. On the one hand, the radical break with classical physical theories, which he had felt to be inescapable from the very begining, was now formally accomplished by the substitution of abstract relations between operators for the simple numerical relations of classical physics. On the other hand, the abstract character of the new formalism made it at last possible to fulfill the requirement he had always emphasized: not to sacrifice any aspect of the phenomena, but to retain every element of the classical description within the limits suggested by experience.

The peculiar form of limitation of the validity of classical concepts expressed by the indeterminacy relations demanded a more thorough analysis than that which Heisenberg had initiated, however. For this challenging task Bohr was, of course, not unprepared. The occurrence of conflicting, yet equally indispensable, representations of the phenomena evoked the ambiguities of mental processes over which he had pondered in his student days. Now, however, similar dilemmas confronted him in an incomparably simpler form, for the description of atomic phenomena operated with only a few physical idealizations. Bohr hoped that the study of such a transparent case would lead him to a formulation of the epistemological situation that was sufficiently general to be applicable to the deeper problems of life and mind, and he devoted all his energy to it. Although he very soon was able to elucidate the essential features, he spent most of the following decade patiently refining the formulation of the fundamental ideas and exploring all their implications.

In any investigation of the scope of physical concepts, the method to follow is prescribed by the nature of the problem: one has to go back to the definition of the concepts by means of apparatus—real or idealized—suited to the measurement of the physical quantities they represent. The analysis of such measuring operations should then reveal any limitation in the use of these concepts resulting from the laws of physics. This had been the method followed by Einstein in establishing the relativity of simultaneity; the same method was followed by Heisenberg and Bohr to elucidate the indeterminacy relations. It emerged from Bohr’s analysis that the decisive element brought in by the quantum of action is what he called the individual character of quantal processes: any such process—for instance, the emission of radiation by an atom—occurs as a whole; it is well defined only when it is completed, and it cannot be subdivided like the processes dealt with in classical physics, which involve immense numbers of quanta, into a sequence of gradual changes of the system.

In particular, the measurement of a physical quantity pertaining to an atomic system can be regarded as completed only when its result has been recorded as some permanent mark left upon a registering device. Such a recording cannot be performed without some irreversible loss of control of the quantal interaction between the atomic system and the apparatus. Thus, if we record the position of an electron by a spot on a rigidly fixed photographic plate, we lose the possibility of ascertaining the exchange of momentum between the electron and the plate. Conversely, an apparatus suited to the determination of the momentum of the electron must include a mobile part, completely disconnected from the rigid frame of spatial reference, whose position, when it exchanges momentum with the electron, therefore necessarily escapes our control. Here is the root of the mutual exclusion of the application of such concepts as position and momentum in the extreme case of their ideally precise determination. More generally, by relaxing the accuracy requirements, it is possible to limit the reciprocal exclusion to the extent indicated by the indeterminacy relations, thus allowing for the concurrent use of the two concepts in a description that is then necessarily statistical.

It thus appears that in order to reach full clarity in such a novel situation, the very notion of physical phenomenon is first of all in need of a more careful definition that embodies the individuality or wholeness typical of quantal processes. This is achieved by inserting in the definition the explicit specification of all the relevant experimental arrangement, including the recording devices. Between phenomena occuring under such strictly specified conditions of observation, there may then arise the type of mutual exclusion for which an indeterminacy relation is the formal expression.

It is this relationship of mutual exclusion between two phenomena that Bohr designated as complementarity; by this he wanted to strees that two complementary phenomena belong to aspects of our experience which, although mutually exclusive, are nevertheless indispensable for a full account of experience. The introduction of the notion of complementairty finally solved the problem of the consistent incorporation of the quantum of action into the conceptual framework of physics—the problem with which Bohr had struggled so long. Complementarity was not an arbitrary creation of Bohr’s mind, but the precise expression, won after patient efforts demanding a tremendous concentration, of a state of affairs entriely grounded in nature’s laws, one that, according to Bohr’s familiar exhortation, had to be learned only from nature. It consecrated the recognition of a statistical from of causality as the only possible link between phenomena presenting quantal individuality, but made it plain that the statistical mode of description of quantum mechanics was perfectly adapted to these phenomena and gave an exhaustive account of all their observable aspects.

From the epistemological point of view, the discovery of the new type of logical relationship that complementarity represents is a major advance that radically changes our whole view of the role and meaning of science. In contrast with the nineteenthcentury ideal of a description of the phenomena from which every reference to their observation would be eliminated, we now have the much wider and truer prospect of an account of the phenomena in which due regard is paid to the conditions under which they can actually be observed—thereby securing the full objectivity of the description, since the description is based on purey physical operations intelligible and verifiable by all observers. The role of the classical concepts in this description is obviously essential, since those concepts are the only ones adapted to our capabilities of observation and unambiguous communication.

In order to establish a link between these concepts and the behavior of atomic systems, we have to use measuring instruments composed—like ourselves—of large numbers of atoms, and this requirement unavoidably leads to complementary relations and a statistical type of causality. These are the main lines of the new structure of scientific thought that gradually unfolded itself as Bohr, with uncompromising consistency, pursued his epistemological analysis to its limits. That some of the greatest representatives of the type of physical thinking with which he was so decisively breaking refused to follow him is under standable; that Einstein should be among them was always a matter of surprise and regret to Bohr. On the other hand, the progress of his work owed much to Einstein's opposition; indeed, its successive stages are marked by the refutation of Einstein's subtle objections. Bohr himself retraced the dramatic course of this long controversy in an article of 1949, which marks the nearest he ever came to a systematic exposition of his argumentation.

The role of complementarily in quantum mechanics is above all to provide a logical frame sufficiently wide to ensure the consistent application of classical concepts whose unrestricted use would lead to contradictions. Obviously, such a function is of universal scope, and an occasion soon presented itself to put its usefulness to the test. In the early 's the extension of the mathematical methods of quantum mechanics to electrodynamics was beset with considerable formal difficulties, which raised doubts regarding the possibility of upholding the concept of the electromagnetic field in quantum theory.

This was clearly a point of crucial importance, since it bore upon the fundamental issue of a possible limit to the validity of the correspondence argument, hitherto unchallenged. According to Bohr's point of view, one had to inquire whether every component of the electromagnetic field could, in principle, be measured with unlimited accuracy, and whether the measurements of more than one component were subject only to the reciprocal limitations resulting from their complementary relationships. Bohr took up this investigation, which occupied him and Leon Rosenfeld during most of the period from 1931 to 1933. He succeeded in devising idealized measuring procedures, satisfying all requirements of relativity, by means of which all consequences of the quantization of the electromagnetic field could be confirmed. In view of the significance of the issue at stake, this work had a wider repercussion than its immediate effect of establishing the consistency of quantum electrodynamics; it showed how essential a part Bohr's epistemological standpoint played in the conception of the quantum phenomena.

By the middle 1930's the main interest had shifted, in Copenhagen as elsewhere, to the rapidly expanding field of nuclear physics. On the theoretical side, the results of the experiments on the reactions induced by the impact of slow neutrons on nuclei, carried out by Enrico Fermi and his school at Rome, created a critical situation. In discussing the processes involving the impact of charged particles, α particles or protons, on a nucleus, it had been found sufficient to represent the effect of the forces acting between the nucleus and the impinging particle schematically by an attractive potential well extending over the volume of the nucleus; to this was added the repulsive electrostatic potential, forming a coulomb barrier around the nucleus. It was therefore natural to analyze the neutron reactions with the help of the same potential, without the coulomb barrier; and it was a surprise that this model did not even qualitatively account for the observed effects. In particular, it was impossible on this basis to understand the very large probabilities with which the capture of the neutron by the nucleus occurred for a sequence of resonance energies.

Faced with this puzzling problem, Bohr proceeded to look for cases of capture processes occurring under a simpler form than in the range of low energies, in which they appeared to be tied to resonance conditions. As it happened, he had only to return to James Chadwick's earliest experiments, performed with neutrons of higher energy; he noticed that the different reactions induced by these neutrons all occurred at any energy with about the same probability, whose order of magnitude indicated that almost every neutron hitting the nucleus was captured by it. This strikingly simple result suggested to him a reaction mechanism radically different from the distortion of neutron waves by a potential well; indeed, in contrast with the quantal character of the character of the latter model, the analogy Bohr proposed was completely classical. He visualized the nucleus as an assembly of nucleons held together by short-range forces, and thus, in effect, behaving like the assembly of the molecules forming a droplet of liquid.

The energy of a particle impinging upon such a system of similar particles moving about and continually colliding with each other will be rapidly distributed among all of them, with the result that none has enough energy to leave the system; the impinging neutron is captured, and a "compound nucleus" is thus formed in a state of high excitation. This state will subsist during a time that is long on the nuclear scale, i.e., which corresponds to many crossings of the nuclear volume by any single nucleon. It will decay as soon as some random fluctuation in the energy distribution has concentrated a sufficient amount of energy on some nucleon, or group of nucleons, to allow it to escape, a process comparable to evaporation from the heated droplet. It was also easy to understand that the density of possible states of the compound nucleus would rapidly increase with the energy of excitation; this explained the absence of resonance effects at high energies as well as their presence in the low-energy range.

Bohr’s “droplet model” of nuclear reactions, refined in various ways since it was proposed in 1936, still holds as the adequate mode of description of one of the most important types of nuclear processes. It is of course an idealized model, and its basic assumptions are not always sufficiently fulfilled to ensure its validity. Thus, another type of reaction has been found to occur, in which the interaction of the impinging particle with a single mode of motion of the target nucleus leads directly to a transfer of energy large enough to complete the process, without formation of a compound nucleus; these “direct interaction” processes are successfully treated with the help of the old method of the potential well, in which provision is made for the possibility of capture by a formal trick imitating the way in which the absorption of light is taken into account in classical optics. Compound nucleus and “optical” potential have now shed all apparent opposition and are blended into a comprehensive theory.

The most important application of Bohr’s theory was the interpretation of nuclear fission. This is a type of reaction that may be initiated by impact of a neutron on a very heavy nucleus: the compound nucleus formed by the capture of the neutron has so little stability that it can split into two fragments of about the same mass and charge. It was Otto Hahn’s and Fritz Strassmann’s chemical identification of such fragments as decay products of uranium under neutron bombardment that led O.R. Frisch and Lise Meinter to recognize that the fission mechanism was the only conceivable interpretation.

The first experiments actually showing the emission of the fragments were performed in Copenhagen by Frisch in January 1939. By then Bohr had left for the United States, where he had been invited to spend a few months at Princeton. It was on his departure that he had heard of Frisch’s idea and project; during the voyage and shortly after his arrival, also in January 1939, he outlined the whole theory of the process. In the following months, this theory was refined and elaborated in great detail, with J. A. Wheeler’s collaboration.

A point that at first seemed surprising was that such a splitting of the nucleus into two parts, obviously initiated by a relative oscillation of these parts with increasing amplitude, could occur with a probability comparable to that of more familiar processes, such as the emission of a γ ray, which results from a stable motion affecting only a very few nucleons. As Bohr pointed out, however, this is a direct consequence of the statistical law of energy distribution among the various modes of the compound nucleus. It seemed harder to explain the differences in the efficacy of show and fast neutrons in inducing fission in different nuclei, but Bohr solved this problem as soon as he was confronted with the experimental data. By one of his most brilliant feats of rigorous induction from experiment, he unraveled the complex case of uranium, concluding that only the rare isotope of mass number 235 was fissile by slow neutrons, while the abundant isotope of mass 238 was not; and he showed by a very simple argument that this difference of behavior was due solely to the fact that the numbers of neutrons in the two isotopes were odd and even, respectively.

The discovery that the highly unstable fission fragments emitted neutrons immediately raised the question of the possiblity of a chain reaction leading to the liberation of huge amounts of energy of nuclear origin. The answer to this question was soon found, and, coming as it did at a critical moment in the social and political evolution of the world, the unfolding of its consequences was precipitated with unprecedented violence. If this was a fateful development in the history of mankind, it also deeply affected Bohr’s individual fate.

The work with fission, continued after his return to Copenhagen during the first three years of World War II, was the last piece of research he carried to completion in the quiet and serene atmosphere he had himself done so much to create. Only much later, during the last two summers of his life, did he for a while manage to concentrate again on a phenomenon very near to those with which he had started his scientific career: the superconductivity of metals, in which the quantum of action manifests itself, so to speak, by macroscopic effects. He tried, without success, to put the somewhat abstract theory of these effects on a more physical basis. In 1943, however, he was dragged into the turmoil of the war, and when he later came back to Copenhagen, he had to cope with profoundly changed conditions of scientific work that banished from his institute the intimacy of bygone years.

Bohr did not fare well among statesmen. In their eyes his candor and directness appeared strange and suspicious, and his clearsightedness was beyond their grasp. The physicists who were desperately striving, under great moral and intellectual stress, toward the dark goal of the nuclear weapon, felt the need of calling Bohr to their support. Bohr was transported in 1943 from Copenhagen to England, through Sweden, not without danger to his life, and was suddenly faced, to his surprise and dismay, with the advanced stage of a project he had deemed beyond the realm of technical accomplishment. Although he did take part, both in England and in the United States, in discussions of the physical problems related to the development of nuclear weapons, his main concern was to make the statesmen, as well as the physicists, aware of the political and human implications of the new source of power.

It is a striking example of his optimism that, besides the obvious dangers, he also stressed the potential advantages of the situation: the existence of a weapon equally threatening to all nations, he argued, offered a unique opportunity for reaching a universal agreement never to use it, which could become the foundation of an era of lasting peace. The condition for setting up such an agreement, he added with his customary logic, was universal knowledge of the issue. More concretely, he urged the Western leaders to initiate contacts with the Russians, with the view of creating a climate conducive to the establishment of peaceful relations and mutual confidence between the West and the East. Although these thoughtful considerations were appreciated by some of the men in key positions, his attempts to put them before Roosevelt and Churchill ended in failure. The fulfillment of his darkest predictions in the following years did not prevent him from presevering and in 1950 he decided to publish an open letter to the United Nations, in which he repeated his plea for an “open world” as a precondition for peace. The timing of such an appeal was the worst possible; but it is now as relevant as ever, and may still perhaps find a response some day.

Apart from this unhappy excursion into the realm of world politics, Bohr devoted much time and energy to the more immediate tasks he was called upon to fulfill. In Denmark, the expansion of his institute occupied him to the last, and he also took a leading part in the foundation and organization, in 1955, of a Danish establishment for the constructive application of nuclear energy. When the European Center for Nuclear Research was founded in 1952, its theoretical division was installed in Bohr's institute, until it could move nearer the experimental divisions at Geneva in 1957; it was then replaced in Copenhagen by a similar institution of more restricted scope, the Nordic Institure for Theoretical Atomic Physics, created with Bohr's participation by the five Nordic governments to accommodate young theoretical physicists from those countries. In these years of unprecedented expansion of scientific research all over the world, Bohr's advice and support were sought on many occasions, and never in vain. He was more than ever a public figure, and honors were conferred on him from every quarter.

Unaffected by this lionization, Bohr made the best of it. An invitation to give a lecture was the occasion for him to orient his thought toward the particular aspect of science that would be familiar to his audience, and to reflect on the possible bearing on it of the new epistemological conceptions he had developed in quantum theory. Thus, in the 1930's he had given a lecture entitled "Light and Life" before a congress of phototherapists, and had spoken of the complementary features of human cultures in an assembly of anthropologists. In the postwar period, he went on in this vein and expressed thoughts about the human condition which for him were inseparable from a proper understanding of the aim and meaning of science. His writings on such topics were collected in three books, published in 1934, 1958, and 1963. These have been translated into several languages, and one must hope that in spite of the difficulty of style they may exert the same influence on the philosophical attitude of coming generations as on the minds of those who heard Bohr himself.

In fact, the form of publication of Bohr's essays is not felicitous. The books contain some repetition, especially in elementary expositions of the physical background, and the main points are often suggested to the reader rather than plainly stated. Involved sentences try to embrace all the shades of an uncommonly subtle dialectical form of thinking. Such obstacles ought not to deter those who are genuinely concerned with the problems, but the unprepared audiences to whom the message was addressed have too often failed to appreciate its true character. Bohr put an enormous amount of work into the composition of his essays, and they contain the most carefully weighed expression of his philosophy.

Bohr's essays strikingly illustrate the continuity of his thought. He was striving all the time to find more precise formulations and to disclose new aspects of the complementary relationships he was exploring, but the basic conception remained the same in all essentials from his youth to his last days. Critics endeavoring to trace foreign influences on his thinking are quite misguided: he was no doubt interested when analogies were pointed out to him between his own conceptions and those of others, but such comparisons never led to any modification of his argumentation—this argumentation, in contrast with the other, was so solidly founded in the analysis of the clear and precise situation offered by the development of quantum theory that there was no need for any firmer foundation. Indeed, Bohr repeatedly stressed the fortunate circumstance that the simplicity of the physical issue made it possible for him to arrive at an adequate formulation of the relations of complementarity he perceived in all aspects of human knowledge.

The domain in which complementary situations manifest themselves most immediately is the realm of psychical phenomena—which had been the starting point for Bohr's early observations. He was now able to express in terms of complementarity the peculiar relation between the description of our emotions as revealed by our behavior and our consciousness of them; in such considerations he liked to imagine (on slender evidence, it must be said) that sayings of ancient philosophers and prophets were groping expressions for complementary aspects of human existence.

In the development of human societies, Bohr emphasized the dominant role of tradition over the complementary aspect of hereditary transmission in determining the essential elements of what we call culture; this he held in opposition to the racial theories then propagated in Germany. Nearer to physics, he pointed out that the two modes of description of biological phenomena which were usually put in absolute opposition to each other—the physical and chemical analysis on the one hand, the functional analysis on the other—ought to be regarded as complementary. Altogether, he saw in complementarity a rational means of avoiding the exclusion of any line of thought that had in any way proved fruitful, and of always keeping an open mind to new possibilities of development.

In his last years, he followed with the deepest satisfaction the spectacular advance of molecular biology. In the last essay he wrote, "Light and Life Revisited," he made it clear that in upholding the use of functional concepts in biology, he did not have in mind any insuperable limitation of the scope of the physical description; on the contrary, he saw in the recent progress the unlimited prospect of a full account of biological processes in physical terms, without prejudice to an equally full account of their functional aspect.

The origin of Bohr's epistemological ideas in a purely scientific situation confers on them the character of scientific soundness and certainly. Bohr was always careful to stress both the necessary, in epistemological investigations, of divesting oneself of all preconceived opinions and of seeking guidance exclusively in the data of experience and the equally stringent necessity of recognizing in every case the limitations inherent in the concepts used in the account of the phenomena. In order to understand the unique significance of his contribution to epistemology, it is necessary to realize that complementarity is a logical relationship, referring to our way of describing and communicating our experience of a universe in which we occupy the singular position of being at the same time, and inseparably, spectators and actors. Far from excluding any aspect of the universe from our reach, complementarity enables us, so far as we can judge, to account for all aspects of the phenomena—comprehensively, rationally, and objectively. By the rigor of his rational thinking, the universality of his outlook, and his deep humanity, Bohr ranks among the fortunate few to whom it has been given to help the human mind take a decisive step toward a fuller harmony with nature.

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This biography is based mainly on personal experience and conversation with Niels Bohr and his closest collaborators, as well as on the correspondence and documents in the Niels Bohr Archive in Copenhagen. Detailed biographical material is published in *Niels Bohr, His Life and Work as seen by his Friends and Colleugues*, S. Rozental, ed. (Amsterdam, 1976). See also the report of the Niels Bohr Memorial Session held in Washington, D.C., on 22 April 1963 in *Physics Today*, **16** , no. 10 (Oct. 1963), 21–62; and an earlier essay of a more personal character: L. Rosenfeld, *Niels Bohr: An Essay* (Amsterdam, 1945; rev. ed., 1961). There is much autobiographical material in Bohr's Rutherford memorial lecture, "Reminiscenes of the Founder of Nuclear Science and of Some Developments Based on His Work," in *Proceedings of the Physical society of London*, **78** (1961), 1083–1115.

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The three volumes of collected essays are *Atomic Theory and the Description of Nature* (Cambridge, 1934; repr. 1961); *Atomic Physics and Human Knowledge* (New York, 1958); and *Essay 1958–1962 on Atomic Physics and Human Knowledge* (New York, 1963);

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