NIELS BOHR

and nuclear physics

By John Archibald Wheeler

NO one can take up the background of the contributions of Niels Bohr to nuclear physics without being reminded at once of the first paper he ever published, "The Determination of the Surface Tension of Water by the Method of Jet Vibration".[1] It appeared in 1909 when he was 24 years old. It and a second paper on the same subject in the following year[2] marked early stages in Bohr's lifelong interest in liquid drops and their vibrations. Twenty-eight years later the vibrating fluid sphere found in his hands a new application as a model for nuclear excitations;[3] thirty years later it became his model for nuclear fission.[4, 5]

Still more important for the foundations of nuclear physics, however, was Bohr's concept of a reaction through a state of the compound nucleus—put forward in 1936 at the same time as the liquid-drop model—and his postwar contribution to the concept of collective excitation and to the collective or unified model of the nucleus.

As Bohr's droplet model had been foreshadowed by his work on capillarity, so his concept of the compound nucleus and his ideas on collective modes of excitation of the nucleus had behind them a rich background of work on atomic constitution, on resonance phenomena, and on the transition from adiabatic conditions to nonadiabatic conditions in atomic processes. What attracted his attention both in atomic and nuclear physics was not so much the nature of the interacting particles as the mechanism of the interaction. To compare and to contrast atoms and nuclei and liquid drops in terms of mechanisms of binding and of excitation was with him a decisive way to approach the problems of nuclear physics. One hardly requires a separate anniversary to celebrate Bohr's contributions to nuclear physics. One can speak about them with all appropriateness this year because July 1963 is the 50th anniversary of his quantum theory of atomic structure.

J. A. Wheeler is professor of physics at Princeton University.

If atomic physics formed the background for Bohr's later work in nuclear physics, it is also true that nuclear physics provided the milieu in which he did his decisive early work on atomic constitution. Alpha particles passing through matter, their loss of energy, and what this had to tell about the state of binding of electrons in atoms formed the subject matter of Bohr's first paper on stopping power.[6] This subject held a lifelong interest for him. It became the focus of new discussions after the advent of wave mechanics. Felix Bloch has given us a much appreciated account[7] of the issues and how they were resolved.

A still closer involvement with nuclear physics seemed destined for Bohr when he went to join Rutherford in Manchester in April 1912. As Bohr put it,[8] "In the first few weeks of my stay in the laboratory, I followed, on Rutherford's advice, an introductory course on the experimental methods of radioactive research which, under the experienced instruction of Geiger, Makower, and Marsden, was arranged for the benefit of students and new visitors. However, I rapidly became absorbed in the general theoretical implications of the new atomic model"—and there is no longer any mention of experimental work!

Still, Bohr kept an interest in the nucleus as well as in the electrons circulating around it. "Thus," he records, "when [in the first part of 1912] I learned that the number of stable and decaying elements already identified exceeded the available places in the famous table of Mendeleev, it struck me that such chemically inseparable substances to the existence of which Soddy had early called attention, and which later by him were termed 'isotopes,' possessed the same nuclear charge and differed only in the mass and intrinsic structure of the nucleus. The immediate conclusion [to which I was forced] was that by radioactive decay the element, quite independently of any change in its atomic weight, would shift its place in a periodic table by two steps down or one step up, corresponding to the decrease or increase in nuclear charge accompanying emission of alpha or beta rays, respectively.

"When I turned to Rutherford to learn his reaction to such ideas, he expressed, as always, alert interest in any promising simplicity, but warned with characteristic caution against overstressing the bearing of the atomic model and extrapolating from comparatively meager experimental evidence."[8] So Bohr did not publish his idea; and some months later, with more experimental evidence to go on, Soddy and Fajans put forward the same displacement law.

Bohr continued to keep close touch with the developments in nuclear physics after he returned to Copenhagen, organized the Institute for Theoretical Physics, and in 1921 moved into a new building. Advances made there and elsewhere in the 1920's towards the understanding of atomic structure and of quantum mechanics cleared the way for a new approach to nuclear physics. Developments in this area went more and more rapidly following the discovery of the neutron, the positive electron, and induced radioactivity.

I myself was fortunate enough to be working in nuclear physics with Gregory Breit in 1933 and 1934, and to have his strong approval for my spending the second year of my fellowship in Copenhagen. I wrote to the National Research Council that I wanted to work with Bohr because he could see ahead more deeply into the still unsolved problems of physics than anyone else I could name.

In one way, September 1934 was a sad time to arrive in Copenhagen. Bohr had just lost his oldest son through a sailing accident. For this reason it was difficult in the beginning for anyone to find opportunity to speak to him. However, after some weeks new issues came to the fore, not least through the weekly seminars. Soon everyone's attention was engrossed in a problem of nuclear physics which was to foreshadow the discovery of the meson.

Experiments of Bothe, Rossi, and others on the cosmic radiation had shown that great thicknesses of lead were penetrated not only by particles of positive charge, but also by particles of negative charge. Were these particles to be identified as electrons, violating the laws of quantum electrodynamics as they were then understood, or were they negative specimens of some new variety of particle? The opinion was widespread at that time that quantum electrodynamics was a subject shrouded with doubt and, in particular, that one had no justification for believing its predictions at energies as high as 137 times the rest energy of the electron. On this account it was often supposed that electrons of cosmic-ray energies could very well violate the expected laws of quantum electrodynamics. The predictions themselves were clear cut. Bethe had calculated the rate of radiation of energy by electrons during their passage through the Coulomb field of atomic nuclei. The predicted mean free path for an electron against loss of half or more of its energy was less than a centimeter of lead. If the theory were sound, it followed that the observed penetrating negative particles could not be electrons.

Was quantum electrodynamics correct in this important prediction? Here was an issue of engrossing discourse. Vital contributions to it came not only from von Weizsäcker and occasional visitors to the Institute but also especially from E. J. Williams, an active collaborator with Bohr in many long stays at Copenhagen.

The relativity point of view was introduced into the discussion. One looked at the collision not from the point of view of the electron moving relative to the nucleus, but from that of the lead nucleus moving relative to the electron. One analyzed the effect of the field of force of the nucleus on the electron in terms of its "equivalent spectrum". It turned out that the major part of the predicted effect of the nucleus on the electron had to do with the "equivalent photon spectrum" at energies of the order of magnitude of the rest energy of the electron, energies of a half-million electron volts. At these energies one knew very well that the laws of electrodynamics held good, not only theoretically but by the soundest and most carefully done of experiments on the scattering of x rays by electrons. Consequently one could conclude—and this Bohr strongly emphasized—that a modest but inescapable transfer of energy to scattered radiation must take place in the moving frame of reference. The same radiation, viewed back in the laboratory frame of reference, is highly energetic. An electron of cosmic-ray momentum must therefore lose energy in lead at an enormous rate, corresponding to a mean free path of less than a centimeter. This vital—but previously doubted—point could be seen[9] to be an unavoidable consequence of the simplest, most firmly established principles of physics.

On this decisively strengthened foundation of theory Carl Anderson could proceed with his studies of the penetrating power of cosmic-ray particles of measured momenta. He established firmly that one was dealing with a new kind of particle and put the meson on the books of physics.

A second issue of much concern in 1934–35 had to do with the anomalous scattering of gamma rays by nuclei and the Delbrück effect. Experiments of Gray and Tarrant and of Meitner and Hupfeld had shown a block of lead scattering many more gamma rays of substantial energy at large angles than one could reasonably account for on the basis of the scattering power of atomic electrons or atomic nuclei. Could it be that a new process was at work?

Delbrück had already pointed out that electrodynamics had a place for a new phenomenon. A gamma ray passing through the electric field of force of a nucleus can create there a virtual pair of positive and negative electrons. The two particles can then recombine and emit a gamma ray of the original energy into a new direction. To calculate the cross section for this process for comparison with observation is one of the most difficult of problems in quantum electrodynamics. The task transcended even the powers of Delbrück. Therefore there was no well-defined theoretical result to compare with the observations.

Happily some help could be found in bridging the gulf between theory and observation. On the one hand, an estimate of the scattering could be made on the basis of dispersion theory. The estimate was so small that the Delbrück effect could not reasonably be considered to be the source of the observed anomalous scattering. On the other hand, it was possible to look more carefully—especially in conjunction with E. J. Williams and Ernest Plesset—into the complicated secondary effects which could occur in a large block of lead. The entering gamma ray produces in one portion of the lead a photoelectron, or a Compton electron, or a pair of positive and negative electrons. Secondary particles formed by all three mechanisms suffer large scattering in the lead. The electrons emit new and penetrating radiation—bremsstrahlung—into directions greatly different from the original direction. In addition, the positive electrons annihilate and give off still harder photons. The radiation produced by the bremsstrahlung and annihilation together gave a simple and elementary account of the mysterious anomalous scattering. There was no call for any new effect.

Attention had nevertheless by now been drawn to the question of the coherent scattering of light by a nucleus. As a way to express this scattering in terms of the cross sections for known processes it was natural to call upon the familiar dispersion formula. An unpublished manuscript along this line circulated in the Institute in a small circle of which Bohr was an active member. He counseled that one could not be certain that the dispersion approach was correct. Therefore nothing was published. It was three years before Kronig, independently motivated, supplied a definitive proof that the dispersion formula holds, even at high energies, simply as a consequence of the principle of causality—thereupon opening the door to the application of the dispersion formula to a variety of high-energy processes.

The most impressive, the richest in its consequences, and the most mysterious of the three effects that received widespread attention in the Institute in 1934–1935 was neither the bremsstrahlung of fast electrons nor the anomalous scattering of gamma rays, but the resonance capture of neutrons. Møller visited Rome in the spring of 1935 and came back with details of the puzzling results of Fermi and his collaborators. How could a nucleus manifest a cross section for interception of a neutron hundreds of times larger than its own geometric cross section? The Copenhagen discussions centered on this issue. I had to leave before they came to a conclusion.

I may recall that the following year, 1936, saw two decisive, practically simultaneous, and quite independent steps towards understanding the resonance capture of neutrons, one by Bohr at Copenhagen (address to Danish Academy of 27 January 1936 [10] and in London February 11 [11]) the other by Breit and Wigner at Madison (article "Capture of Slow Neutrons" received by *The Physical Review* 13 February 1936; [12] also paper No. 30 delivered at the meeting of the American Physical Society of 21–22 February 1936). Breit and Wigner envisaged as a mechanism leading towards capture an exchange of energy between the incident particle and one of the bound nucleons. On this basis they established their famous formula for the cross section as a function of energy in the neighborhood of a narrow resonance. They recognized that this resonance formula applied to a wider range of circumstances than those envisaged in the original mechanism of exchange of energy between one incoming and one bound particle.

Bohr did not deal with resonance theory as such. Still less did he treat the detailed shape of resonances. He analyzed rather the message to be read out of the observed level widths and level spacings as to nuclear constitution and the nature of nuclear reactions. One principal conclusion from his analysis was summarized in the concept of the *compound nucleus:* the idea—to put it loosely—that incoming radiation of resonant energy impinges on the target nucleus to form a system which lives so long that it has no memory of the mechanism by which it was formed. The compound nucleus is thus to be compared in a straightforward way with any of the species of radioactive nuclei studied so thoroughly by Rutherford and his collaborators. The resonant state can break up by alpha-particle emission, or it can undergo beta decay, give off a photon, or emit a neutron or a proton—and thus transform to a new quantum state—with probabilities per second, or radioactive transformation constants, for these distinct processes, which are not only independent of each other, but also independent of the mechanism of transformation. Bohr thus stated in all generality, but without spelling out some of the obvious mathematical details, the same idea which Breit and Wigner had independently developed and which they had invested with the full mathematical completeness of resonance theory—though ostensibly for the special physical mechanism of a two-particle trade of energy.

So much for the conclusion to be drawn from (1) the observed existence of states of excitation with a natural *width small* compared to the distance to the next level. What about two important additional features of the experimental evidence? (2) In a given nucleus one slow-neutron resonance is quite *similar* to another both in its probability for emitting a gamma ray and in its radioactive transition constant for re-ejecting a neutron. (3) Between thus similar levels the average *spacing* is *smaller* by powers of ten than one would expect from the picture of a nucleon moving nearly independently through an effective average field of force. The states of excitation of a nucleus are in this respect very different from the states of excitation of an atom, any electron of which can be conceived, to a good approximation, to move through an effective average atomic field of force. Between nuclei and atoms there must thus be a great difference in character, at least so far as concerns states above the energy limit for particle emission—states such as are explored in particle-bombardment experiments.

Bohr was therefore led to explore a model for the nucleus which is the direct opposite of a planetary system with a long mean free path for each particle. He compared the nucleus to a drop of liquid in which each molecule has a mean free path very short relative to the dimensions of the system. This droplet model was evidently more special than the concept of a compound nucleus. However, it had the great advantage of allowing one to visualize an explicit mechanism by which a nucleus could retain an excitation energy as much as 10 MeV for a time a million times longer than the time required for a particle to pass from one side of the system to the other. In an early lecture on the subject, of which we have an account by a correspondent,[11] Bohr showed a collection of billiard balls held in a certain neighborhood on the table by a slight saucer-shaped depression. Another billiard ball was sent in from outside with far more than enough energy to dislodge any single one of these balls from the depression. Yet it was instead captured into the depression itself. Turning from the model to the nucleus, Bohr emphasized that "the excess energy of the incident neutron will be rapidly divided among all the nuclear particles with the result that for some time afterwards no single particle will possess sufficient kinetic energy to leave the nucleus".

It is interesting for someone who knows of the liquid-drop model of the nucleus to read Bohr's own carefully written and famous five-page publication on neutron capture and nuclear constitution.[10] One thinks of the drop model as implying a mean free path for nucleons very short compared to the nuclear radius. That was also a point of view most useful for a later first treatment of nuclear vibrations and nuclear fission. Yet Bohr takes care not to commit himself to anything so explicit. The paper is expressed in more general terms. It retains its validity today when we know that mean free paths are neither very short *nor* very long compared to nuclear dimensions, but comparable to them. So far as we now know, the formulation of this paper will be considered sound and clear for ages to come. How does this balance of view come into being?

No one who has struggled with a lively physical issue side by side with Bohr will forget the turbulence of the discussion, its ups and downs, the trial first of one extreme point of view and then another, and the forceful words with which Bohr so often summarized one or another conclusion reached along the way. "How can one *possibly* believe such and such a view when such and such is so *absolutely* clear?" One or another model, whether the individual-particle model, or the liquid-drop model, or some other model, gets drawn on the board in a vivid diagram and pushed to the limit of its predictive power. But of all such details and special views there is often no trace to be found in the final paper. The extremes having been tried, one knows between what limits the truth must be found. Just because these limits are often so wide, the wording may seem foggy to the uninitiated and unforewarned reader. A few quotes from the paper on neutron capture[10] may illustrate this point:

In making his new contrast between the structures of nuclei and of atoms, Bohr states that "the possibility of counting by means of such collisions the individual atomic particles and of studying their properties is due above all to the openness of the systems concerned, which makes an energy exchange between the separate constituent particles very unlikely. In view of the close packing of the particles in nuclei we must be prepared, however, for just such energy exchanges to play a predominant part in typical nuclear reactions." Some care must be taken in reading this last sentence to recognize how carefully it says what it is reasonable to say about the mean free path of a nucleon. Never is the author committed to a statement so sharp as: "The mean free path is small compared to nuclear dimensions," although, without ever revealing it, he already has investigated in some detail many of the reasonable consequences that flow from so simple an assumption.

Another illustration of his caution, taken from the same paper,[10] has to do with the question whether the particles observed to come out of the nucleus also inside the nucleus have the properties of free particles. He refers to the reasons for thinking of beta particles as created in the act of decay rather as pre-existing nuclear constituents. Going on to nucleons, he remarks, "Especially the fact that all nuclear masses in the first approximation are integral multiples of a unit nearly equal to the neutron mass, makes it very reasonable to regard particles of such masses as mechanical entities within nuclei. On account of the small difference between the masses of the neutron and proton . . . it would, however, seem more hypothetical to assume the existence in nuclei of particles with the same electric and magnetic properties as those possessed by free neutrons and protons."

Later in the paper, analyzing why level spacings are so much smaller for an excited nucleus than one would have expected from the idea of single particle excitations familiar in atoms, he remarks that "expressions like α-ray levels or proton levels, such as are used in the ordinary discussion of these effects [α-ray resonance transmutations], based on the attribution of the excitation to a single nuclear particle, lose all meaning"—and again, cautiously —"on the view of nuclear excitation adopted here."

Finally, in the conclusion of this fundamental paper leading into modern views of nuclear structure come these words, ". . . even if the problem of nuclear constitution does lack the special simplicity in a mechanical respect characteristic of the structure of the atom which has so much facilitated the . . . [clarification of atomic structure] . . . it presents nevertheless . . . peculiar facilities for a *comprehensive interpretation of the characteristic* properties of nuclei in allowing a division of nuclear reactions into well-separated stages to an extent which has no simple parallel in the mechanical behavior of atoms."

As this paper of Bohr ends, so his subsequent papers dealing with nuclear reactions begin with this concept of a division of nuclear reactions into well-separated stages, of which the first is the uptake of the particle to form a compound nucleus, and the second is the competition between the various modes of disposition of this energy.

Among subsequent papers on such issues, one of the most important was a 1937 treatment of models of nuclear constitution by Bohr and Kalckar.[13] It attempted a general analysis, in the light of the information available at that time, of both nuclear binding and nuclear excitations. It deals with the kinetic energies of individual nucleons, the compressibility of nuclear matter, and the modes of dilatation of the nucleus. It goes on to consider those modes of excitation which one can compare with the shape oscillations of a droplet of an incompressible fluid. It discusses briefly the issue of nuclear rotations. Then comes a statistical analysis of the total energy resulting from simultaneous excitation of several of these independent modes. Reasonable conclusions are drawn as to the trend of nuclear level density with excitation. Finally, with due account of the fundamental resonance formula of Breit and Wigner, an analysis is given of the variation of nuclear reaction cross sections with energy, both in the lower-energy region where resonance phenomena occur, and in the higher-energy region where the levels overlap each other.

This 1937 paper had to close with many issues not cleared up, among them how to understand the cross section for absorption of gamma rays at energies where the resonances run together. An elucidation of these problems only came in 1939 in a fundamental paper by Bohr, Peierls, and Placzek.[14] They showed that the cross section, as estimated by primitive considerations, has to be corrected by a large factor which depends on the ratio of level width to level spacing.

Such was the state of knowledge of nuclear constitution and nuclear reactions when, on Sunday, the 16th of January 1939, the Swedish-American liner *Gripsholm* pulled up to the pier in New York and Bohr stepped off.

Only after he had been safely on board the ship in Europe had Frisch and Meitner dared to tell him of the discovery of Hahn and Strassmann and their preliminary views of what it meant. They had feared that he would all too readily reveal the new concept to everyone before there had been a chance for them to digest the discovery themselves. One can well imagine the journey across the Atlantic and all the thoughts that had come up there! I recall the excitement when just a few minutes after our greetings I was told in confidence about the splitting of uranium.

Bohr was spending a substantial part of the spring semester in Princeton. He and I were engrossed at once in analyzing the new effect. For this purpose an ideal background was supplied by the views of nuclear constitution that had come out of the previous three years. Naturally new issues arose. They furnished applications and tests of the concept of the compound nucleus and the more special liquid-drop model. They also supplied a framework of ideas for working out new features of nuclear constitution and of reaction-rate theory.

An early issue was the release of energy in fission. Here, guidance came from the idea of Gamow and Weizsäcker for a semiempirical account of nuclear masses. A supplementary term was required to describe the difference in mass between even-even and odd-even nuclei, a difference so important in accounting at another stage of the analysis for the very different fission properties of U^{235} and U^{238}. Another change had to be introduced in the semiempirical mass formula to recognize the windings of the valley of nuclear stability. Only then could one determine, with some approach to accuracy, the energy release on division into fragments of one or another relative mass. Then, as now, nothing was found in this energy release to suggest that asymmetric fission should be preferred to symmetric fission. In addition to the kinetic energy of the fragments, one could calculate from the revised mass formula approximate values for the energy release in each successive beta decay of any given fragment. One could compare this decay energy for the mother with the binding energy of a neutron in the daughter. One could conclude that in a few rare decay products the daughter is left sufficiently excited after beta decay to emit a neutron. Thus one could understand the then still-puzzling observation of delayed neutron emission following fission.

The quantities of chief importance for fission following from the semiempirical analysis of nuclear masses were, however, not these transformation energies, but estimates of the electrostatic energy and surface energy of the compound nucleus before fission. They were the starting point for a calculation of the critical energy required to trigger the division—an amount of energy obviously much smaller than the energy *release* in fission! For the first stages of this calculation we had already (several times) gone up the stairs of Fine Hall from office to library two steps at a time to refer to the *Collected Papers* of Lord Rayleigh, for whose physical insight and wisdom Bohr expressed great admiration on various occasions through the years. Starting with the surface energy of a spherical droplet, and considering a small departure from sphericity, one could, from Rayleigh, read off the change in surface energy to the second order of small quantities. There was no motive for Rayleigh to consider the electrostatic energy of a uniformly electrified mass of fluid, but he and others had analyzed what was the same quantity except for a change in sign—the gravitational energy of a sphere subjected to a small tidal deformation.

One thus ended up knowing two energies in their dependence upon shape for small deformations—the electrostatic energy and the surface energy. The absolute value of each energy is found from the semiempirical mass formula. The *change* relative to the value for the original sphere is measured for each energy by a coefficient in a term of the second order in the distortion. In the case of uranium, the calculated positive coefficient measuring the increase of the surface energy came out larger than the negative coefficient that governs the decrease of the electric energy in a small deformation. In other words, uranium was calculated to be stable against small changes in shape, a conclusion in obvious agreement with experience.

The stability in the case of other nuclei was calculated to depend upon the ratio, Z^2/A, between the square of the charge number and the first power of the mass number. The critical value of Z^2/A for the transition from stability to instability came for nuclei far short of being twice as heavy as uranium. It was reasonable to conclude that one has in this circumstance one natural explanation (over and above alpha instability) for one's failure to find very heavy nuclei in nature.

The stability, examined so far for small distortions, had at this point to be analyzed for larger deformations. For this purpose it was necessary to go beyond Rayleigh's terms of the second order. The terms of the third and fourth orders in the distortion were calculated. The energy first rises with deformation and then falls. The location of the maximum defined a barrier against fission. The nucleus has to be provided with this critical amount of energy, it was concluded, if it is to undergo fission without having to tunnel quantum mechanically through the barrier. Once over the barrier, the nucleus elongates more and more rapidly, and finally tears apart into two fragments.

Whether capture of a slow neutron brings about fission with significant probability thus became the question of whether the energy of condensation set free by addition of the neutron exceeds the fission barrier. For nuclei of sufficiently high charge this will always be the case, and for lighter normal nuclei it will never be the case. For uranium, clearly one was near the critical limit—but how near?

Bohr was staying at the Nassau Club, as was Rosenfeld, who had come with him and was closely occupied with him on the deep problem of measuring the electromagnetic field quantities. One morning, early in February, George Placzek joined them for breakfast.[15] The conversation naturally turned to the progress that had been made in understanding the mechanism of fission. Placzek protested that there were observations which the theory could not explain. The cross section of uranium for the capture of a neutron showed a resonance at roughly 10 eV, whereas the fission cross section—substantial at thermal energy—showed no resonance at 10 eV. In thorium, there was again a capture resonance at low energy but the fission cross section, according to the observations of Ladenburg, did not become significant until the neutron energy reached about 1.5 MeV. Bohr became restless, got up from the table, and, deep in thought, walked with Rosenfeld over to Fine Hall, where without a word he proceeded to sketch on the board the complete explanation in terms of the theory.

For thorium, as we had already recognized, the barrier against fission must be about 1.5 MeV higher than the energy set free by intake of a neutron. Thus, only fast neutrons have an appreciable probability of bringing about fission. Therefore, a slow-neutron resonance must lead to capture, not to fission.

In uranium, the presence of a similar resonance leading exclusively to capture argued similarly that the fission barrier must be high. Therefore, here too, only fast neutrons ought to be able to bring about fission. How, then, is it that thermal neutrons also induce a modest amount of fission in natural uranium? The only evident way out was to say that this modest amount of slow-neutron fission occurs in the rare isotope U^{235}. The capture of a slow neutron in this odd isotope produces a compound nucleus with an excitation much higher than that produced in U^{238}. The energy levels, therefore, are much closer together. They are so close that they overlap. Thus there is no resonance in the fission produced by slow neutrons in natural uranium—produced by reason of the content of one part in 139 of U^{235} in this natural mixture.

From the theory as it had already been developed up to this time it was thus possible to conclude that U^{235}, if separated, would be highly fissile to slow neutrons. Among those of us aware of this new conclusion, with all that it implied for using uranium as a source of energy, only Placzek doubted. A wonderful person, a man of the highest integrity, he was often a delightful and thorough-going skeptic about new ideas, and skeptic at this time in particular about the idea that U^{235} is responsible for the fissility of natural uranium. Placzek and I made a bet on this issue, $18.46 on my part to $0.01 on his, the odds based on the proton-electron mass ratio. More than a year later, on the 16th of April 1940, immediately following the experimental verification that U^{235} *is* responsible for the low-energy fission, I received from Placzek a money order telegram for $0.01 with the one-word message, "Congratulations".

Deeper than the question of fission thresholds—as we developed the general theory further—was the issue of fission rates. The mechanism of passage over the barrier is much like the monomolecular transformation by which a complex molecule goes through the so-called "transition state". There one knows the temperature and wants to know the reaction rate. In the nuclear case one knows the energy and wants to know the level width. To evaluate this quantity, it was necessary to compare the phase space available for the nucleus before it passed over the fission barrier with the number of cells of phase space available at the summit of the fission barrier itself. In this connection one was led directly to a count of what at that time was called N_f, "the number of states of excitation accessible to the fission form at the saddle point". Today we use the shorter term, "number of fission channels". The statistical analysis led to an extremely simple expression for the ratio of level width to level spacing, namely the number of fission channels divided by 2π:

$$\Gamma_f/D = N_f/2\pi.$$

When the available excitation exceeds the fission barrier by only a small amount, it followed from this formula that the fission width is a small fraction of the level spacing. On the other hand, at high excitation the number of fission channels which are accessible increases almost exponentially with energy. From this circumstance it was possible to explain why the cross sections for the fission of U^{238} and Th^{232} rise almost exponentially with neutron energy in the beginning and then reach what is almost a plateau.

In the region of steepest rise of the cross section, new channels are coming in one by one. In those days it was supposed that quantum mechanical effects would wash out the individuality of these channels. Today we know from the precision experiments of Lamphere and Green and the theoretical considerations of Aage Bohr that each new channel produces its own characteristic step in the curve for cross section as a function of energy. Thus one can make a direct count of the number of states in the transition-state nucleus up to any excitation and give that number N_f a well-defined experimental significance.

Many other issues arose in the analysis. I will mention only a few of them to illustrate Bohr's deep appreciation of the complexity of the fission process. Will one slow-neutron resonance be greatly different from another in its half life with respect to fission? This question was often discussed, and Bohr inclined to the view that the differences would *not* be great. However, the channel concept had not at that time been verified experimentally nor developed in sufficient theoretical detail for one to take a definitive position on such variations.

Spontaneous fission presented its problems too. From the observed half life of unexcited uranium against fission it was possible to get an "experimental" value for the Gamow penetration integral associated with the fission barrier. The value agreed in order of magnitude with what one calculated from the liquid-drop model. In that model, one analyzed successive configurations of the droplet on the way through the barrier to the lowest relevant two orders of perturbation from the spherical form. However, in detail, the value of the penetration integral derived in this way fell short by a factor of the order of two from accounting for the observations. On this point, I have a troubled comment from Bohr in a letter of July 1939, after a joint paper on the fission process had been sent in for publication, and before proof had been returned. Today we know, from the years of investigation that Swiatecki and his collaborators have devoted to the liquid-drop model with the assistance of a high-speed electronic computer, that the idealized fission barrier does not fall off quickly beyond the summit as one anticipated from the first three terms of the power series, but on the contrary keeps a substantial height, and shows a complex structure, out to relatively enormous deformations.

Another subject of deep concern as more experimental information came in was the distribution in mass of the fission fragments, the eternal puzzle: how does it come about that one fission fragment typically is quite substantially larger than the other? We now have so much new observational evidence bearing on this important, complex question that it would be out of place to try to discuss it here. Bohr's interest in the topic was reflected in a brief draft manuscript that I received from him late in 1939, which he suggested that we might develop further. The central idea was a pair of saddle points in the surface for energy as a function of deformation, each saddle point or barrier summit corresponding to an asymmetric fission form. However, the further we looked into this picture, the more doubtful did it appear that asymmetry in fission has any simple relation to the properties of the saddle-point configuration itself. Today one is forced to recognize that one is dealing here with a rather deep aspect of the dynamics of fission.

With the war came an enormous application of fission physics. However, this is not the occasion to touch on nuclear energy and its manifold consequences, including Bohr's intense concern with the related postwar political issues.[16] Instead, it is appropriate to ask about Bohr's postwar position on nuclear structure and nuclear reactions.

By 1949, a wealth of evidence had come to light indicating that a nucleus is not in all respects comparable to a liquid drop. Instead (and in more ways than one had previously known) it has points of similarity to an atom. In a certain approximation, one was forced to recognize, the individual nucleons are assigned to individual states, each with its own set of quantum numbers. It is the miracle of nuclear physics that the mean free path of nucleons through nuclear matter is so nearly comparable to nuclear dimensions that on the one hand the liquid-drop model proves useful, being based on the idealization that the mean free path is short, and on the other hand the individual particle model, resting on the idealization that the mean free path is large compared to nuclear dimensions, has even more predictive power. The presence of two such different views of nuclear constitution, with their conflicting predictions, created an important issue: how to reconcile these views and draw them together into a unified picture of nuclear constitution.

A letter from Bohr in December 1948 suggested that it would be fortunate if we could have further discussions of these issues. It was possible to get together again in the last quarter of 1949 and the first month of 1950, before the demands of hydrogen-bomb defense supervened. There were numerous discussions. The central idea was the concept of an interaction between the individual nucleon and the nuclear surface, involving a union of individual-particle behavior and collective motion. Evidently what was required was a further and still more sophisticated development of the ideas that Bohr had put forward in 1936 that nuclear "excitation will correspond to some quantized collective type of motion of all the nuclear particles". Thinking over Bohr's vivid concept of a coupling between a nucleon and the surface (while on the train to Paris after a stimulating discussion in Copenhagen), I could not help thinking of an explanation along these lines for the mysteriously large quadrupole moment of certain atomic nuclei in the ground state.

The liquid drop, with its spherical equilibrium form, could of course give no account of these moments. Neither could an explanation be found in terms of the electric quadrupole moments of individual particles moving in a spherically symmetric field of force. However, when one allowed for the nonspherically symmetric pressure of nucleons against the nuclear surface, and the resulting deformation, as calculated on the liquid-drop model, one found quadrupole moments of the right order of magnitude. This step fell in with Bohr's idea of a unified picture of collective and individual particle motions. On December 24, 1949, after discussing the question with Lindhard, he wrote, "What you tell about your considerations of the quadrupole moment of a nucleus with one particle in an otherwise empty shell seems to us very beautiful and convincing and, as we understand, the point is that the deformation of the nucleus arising from the presence of this particle will imply a comparatively large quadrupole moment of the particles in the closed shells."

Independently, James Rainwater came to the same conclusion, which he published in a beautiful and important paper.[17]

Bohr went on to comment on the problem of the dynamics of deformations, a point also of great concern to him. "As regards the problem of the treatment of the oscillations of an excited nucleus, starting from an individual particle picture, we are, however, not certain that we fully understand your considerations. The attack is surely of a very direct kind, but it seems not beforehand quite clear to me how one can analyze the effect of nuclear deformations and their time derivations so generally.

"It would seem that the actual physical problem is rather to examine the semiadiabatic changes of the individual-particle binding accompanying the oscillatory deformations of the whole nucleus, and that the justification for the customary treatment of the problem should be the appearance of additional terms in the whole nuclear energy of a type corresponding to those of capillary oscillations of a liquid drop."

We had already done something towards writing an account of the issues of nuclear constitution and nuclear reactions as an attempt to unify the individual-particle and liquid-drop points of view, and with the aim of making further headway, Bohr added, "It might therefore be the very best if you could come in the week from January 14 to 21st."

Carlsberg was the center for living and working during that visit as it has been for others of Bohr's family of collaborators before and since. There was a little quiet time at breakfast with Mrs. Bohr when two newspapers were perused and the latest political developments were discussed. After breakfast until an hour in the evening anywhere from ten to midnight the discussion then went on, day after day, sometimes in the atrium, sometimes in the workroom attached to it. The atrium, with its U-shape, its domed-glass ceiling, and its colonnaded walk, had been copied from Pompeii by Jacobsen, the builder and donor of this beautiful home. It was an ideal place for the perambulatory style of discussion to which Bohr was so given. Sometimes it was necessary to repair to the blackboard in the workroom to develop the details of a point. Again this little room was put to use each time when the discussion had led to a conclusion. Either I would write it up or Bohr would dictate it.

This place satisfied Bohr's definition of a workroom, "a place where nobody can keep you from working". One of its most delightful features was a set of drawers, about 25 in number. Each was perhaps an inch thick and contained a draft manuscript having to do with one or another issue of physics. Each topic ripened from draft to draft—sometimes over many years—until the point was reached where in Bohr's judgment publication was at last appropriate. Among the manuscripts for which the basic idea reached far back into the past, one of the most celebrated dealt with angular momentum and its exchange in atomic and nuclear transformation processes. It never reached the point of publication. However, it, like other drafts in this collection, defined conclusions and stated issues, and it furnished the starting point for the development of new ideas.

In the fall of 1949 I had received from Bohr just such a draft, called "Tentative Comments on Atomic and Nuclear Constitution". It had furnished a starting point for what we had been doing since. This intensive week at Copenhagen in January brought us further in formulating on paper a collective model of the nucleus, incorporating both individual-particle motions and collective vibrations, and in understanding fission phenomena along this line, particularly the variation from one fission act to another in the number of protons which come off in a fragment of a given mass number.

Several breakfast discussions during this late January period touched on the Soviet nuclear tests of the fall of 1949 and on the debate going on across the Atlantic as to whether the United States should undertake work on thermonuclear devices. Whatever were the factors in such more immediate decisions, Bohr had stressed his belief in the long-term ideal of *The Open World*.[16] At the same time —aware as always of political realities—he emphasized again what he had often said before, "How could Western Europe possibly have remained free and at peace after World War II if America had not had the atomic bomb?"

Only a few days after these discussions, the demands of Western thermonuclear defense unexpectedly broke in and removed any further opportunity to collaborate with Bohr for the period from February 1950 to March 1953. Our analysis, as far as we had gone, together with additional considerations by David Hill and myself on fission and on the coupling between individual nucleons and the nuclear surface, were embodied in a manuscript which went to Bohr in 1952. It was obvious from his reply that he felt that there was more that remained to be understood, and that a still further period of collaboration would be desirable. That this was impracticable was, however, as clear to the one as to the other. In view of the circumstances, he advised that the report on the collective model be published by Hill and me as it stood, despite the fact that a substantial fraction of it was his own work, and in this form it appeared.[18]

Happily for the development of the collective or unified model of the nucleus, Aage Bohr—then at Columbia—had quite independently been drawn into the subject from an entirely different side. He had realized that rotational states of atomic nuclei are to be understood in terms of the interaction between the quantum states of individual nucleons and the rotating potential well associated with a deformed and rotating nucleus. He found himself forced to differentiate between the collective motion associated with rotation and the extrinsic excitation connected with the motion of individual nucleons. The importance of this work to the whole subsequent development of nuclear physics is too well known to require mention here. The increase in our understanding of nuclear constitution and nuclear transformations achieved by those in Copenhagen, not least Aage Bohr and Ben Mottelson, gives one faith that the spirit of Niels Bohr lives on in the deeds of those who have felt his influence.

On a visit to Copenhagen in a later year, in the course of a quiet Sunday morning talk, Bohr commented on how all the great religious leaders—among them Jesus, Lao-Tse, Confucius, and Buddha —had won their influence through their power to console those who had suffered great sorrow. He recalled the old story of the woman who had lost her only child, a fine and promising boy. She was almost out of her mind with grief. Month after month went by without improvement in her condition. Finally her husband and her friends won Buddha's agreement to bring her back into the course of human life. "I will cure you on one condition," he said, "that you bring me six grains of mustard seed—grains given by someone who has never experienced a sorrow." Eagerly she approached the first villager she came upon, and as courteously he gave her six grains of mustard seed. Going off with them, she suddenly thought of the rest of the condition, and turned back to him, "Oh, but have you ever had a grief?" With his answer she had to give the seeds back, and sadly turn away. After many another experience of this kind she came back to Buddha, cured. Bohr's own deep understanding of human problems, and his powerful influence over those around him, make one certain that men like Jesus, Lao-Tse, Confucius, and Buddha really lived.

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