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FISSION IN 1939: The Puzzle and the Promise

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Never more actively, responsibly, and productively than today do historians of science study the evolution of modern physics. Their enterprise founds itself on written records, historical training, and scholarship. As they build, however, the living lamps of memory one by one go out. Therefore, the present account—by one early participant in fission physics—will perhaps be more useful if it is conceived, not as history, but as memories, impressions, atmosphere.

I stood in the winter cold at Pier 97, North River, New York on Monday, January 16, 1939 to welcome Niels Bohr (Figure 1), about to debark from the Swedish-American liner *Drottningholm*. In my wait Enrico and Laura Fermi (Figure 2) joined me. Fermi had been at Columbia University for less than a month since his Rome-to-Stockholm trip. As Bohr cleared customs we greeted him, his son Erik, and his long-time colleague Léon Rosenfeld. Their upcoming three-month stay at the Institute for Advanced Study in Princeton had for Bohr an overriding purpose: Clarify the quantum. To that end pursue the long-continued dialogue (1) with Einstein (Figure 3). Do everything possible, man to man, to reach agreement with him.

January 3rd, however, had opened to Bohr a second vista. That day, just before the *Drottningholm* sailed, Otto Robert Frisch—a friend of mine since my 1934–1935 year in Copenhagen—fresh back from Sweden, reported to him the conclusions to which he and his aunt, Lise Meitner, had been forced (2) by the not yet published findings of Otto Hahn and Fritz Strassmann (3). "I had hardly begun to tell him," Frisch writes (4), "when he struck his forehead with his hand and exclaimed, 'Oh what

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idiots we have all been! Oh but this is wonderful! This is just as it must be.'"

Show that fission as then known really does proceed just as it must: This goal held itself out ever more invitingly to Bohr with each new day of pacing up and down the deck with the one shipboard companion or the other. Nothing of this new goal or of fission itself did I or anyone in America know when I greeted Bohr at the pier. How could I have anticipated that he would invite me to join him in this enterprise? Or that we would extend the work (5): **predict** as yet undiscovered features of fission?

Bohr felt obligated not to let out word of fission until Frisch with an ionization chamber or otherwise could demonstrate splitting and send in his findings for publication (6). Niels and Erik went into Manhattan with Enrico and Laura to visit old friends, father and son spending a night or two there before coming down to Princeton. Rosenfeld and I, however, took the next train. He was unaware of Bohr's self-imposed commitment to silence on fission. He revealed the exciting news on the one-hour trip. I got him to report the great discovery that very evening at the regular Monday 7–9 p.m. Journal Club (Figure 4).

Bohr arrived a day or two later, discovered that the cat was out of the bag, told me more and we got to work. The aim was straightforward. The burgeoning world of experimental findings: bring them into order within the framework of Bohr's compound-nucleus model of nuclear reactions. This model he had first enunciated in 1935, during my time in Copenhagen.

Figure 1 The Old World reached across to the New World—at Pier 97 North River, New York, Monday January 16, 1939—with the word of fission [Detail from Michelangelo's *The Creation of Adam*, Sistine Chapel.]

Figure 2 The Fermis shortly after their arrival in the United States. Courtesy of Wide World Photos, Inc.

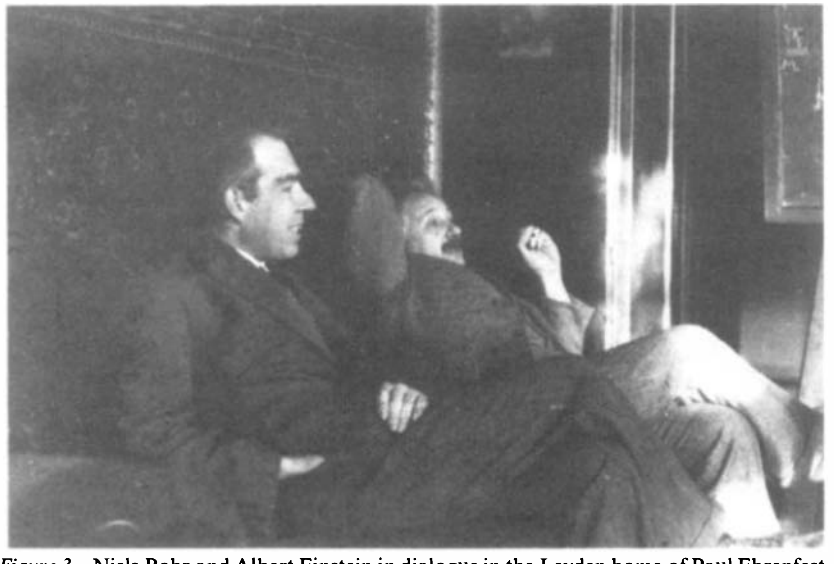


Figure 3 Niels Bohr and Albert Einstein in dialogue in the Leyden home of Paul Ehrenfest in 1933. The restoration of the negative of Ehrenfest's photograph and the production of the print were done by William R. Whipple. Courtesy of the American Institute of Physics Niels Bohr Library.

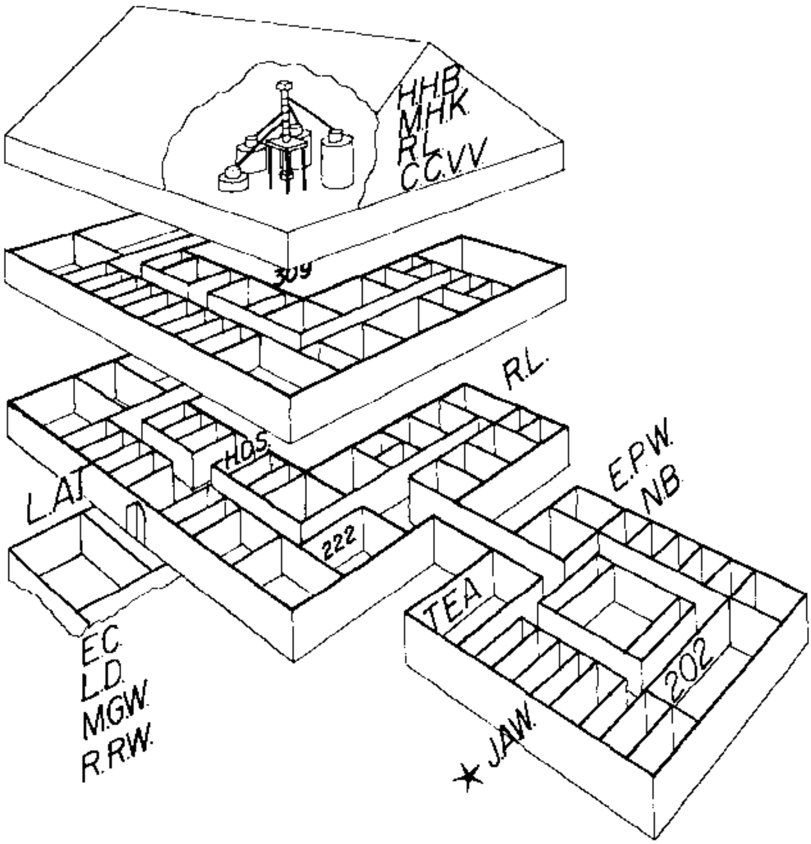


Figure 4 *Star*: location of the Fine Hall (today Jones Hall) ground floor seminar room where Rosenfeld made known the discovery of fission. Subsequent Princeton work on fission was divided between the main floor of Fine Hall (Eugene P. Wigner, Bohr, and the writer) and the adjacent Palmer Physical Laboratory. Cyclotron: Edward Creutz, Luis A. Delsasso, Milton G. White, and Robert R. Wilson. Main floor: Louis A. Turner, Henry D. Smyth, and Rudolf Ladenburg. Attic: Van de Graaff generator of neutrons: Henry H. Barschall, Morton H. Kanner, Ladenburg, and Cletus C. Van Voorhis. TEA: "where we explain to each other what we don't understand." Room 222: graduate courses in physics. By January 1939 Einstein had vacated his former Fine Hall office (E.P.W.) for new quarters at the Institute for Advanced Study, then abuilding a mile away, but nearby, over the fireplace in the Professors' Room (# 202), are engraved his famous words, "Raffiniert ist der Herr Gott, aber boshaft ist Er nicht." Of physics colloquia in Room 309 he attended occasional ones, including the one on the mechanism of fission; and there he gave his last talk (7) before his death.

Since then he and I had been working on the development and application of this model (Figure 5).

As we proceeded with our work, we found we had to introduce concepts new to nuclear theory: **fissility parameter**, nuclear **potential-energy** surface, **saddle-point energy** as threshold for fission, **channel** open for over-the-barrier fission, **channel count** as determiner of the contribution of fission to level width, and **spontaneous fission** via barrier penetration. The analysis culminated in a 25-page paper, "The Mechanism of Nuclear Fission." As if omen of a new world of weapons, it appeared in the issue of the *Physical Review* dated the first of September, 1939, the very day World War II began. What was the background for the collaboration of the American junior partner in this work?

No better symbol do I know than Michelangelo's great Sistine Chapel painting for the lightning stroke of January 16th, 1939 that brought the word of fission from Europe to America. No fitter image, either, can I offer for the electricity of learning that had flowed from the outstretched

Figure 5 A few of the participants in the Third Washington Conference on Theoretical Physics of February 18, 1937, where the compound-nucleus model of nuclear reactions received considerable attention. Niels Bohr, front; I. I. Rabi and George Gamow, second row; Fritz Kalckar and John Wheeler, third row; and Gregory Breit, directly behind Wheeler.

finger of Europe to the outheld finger of America for many decades before 1939.

In 1876 the new Johns Hopkins University in Baltimore, under the leadership of Daniel Coit Gilman, became the first great institution of higher learning in America explicitly to dedicate itself to the Europe-inspired research ideal. Henry A. Rowland brought to it preeminence in physics as other men brought to it a like spirit of exploration and discovery in other fields. What we would today call graduate-level training for research took first place in those days. Relative to it, any undergraduate education provided was regarded as preparatory, secondary, gap filling. Education was not a teacher facing a class. Education was colleagues, young and old, facing an issue.

Seminar courses provided the most stimulating source of education (8) even for one who had entered Hopkins so late as 1927, especially seminars focused on such books as Born and Jordan's *Quantenmechanik*, conducted by Gerhard Dieke, Maria Mayer, and Karl Herzfeld—relatively new arrivals from The Netherlands, Germany, and Austria; Compton and Allison's *X-Rays in Theory and Experiment*, guided by Joyce Bearden; Wintner's *Spektraltheorie der unendlichen Matrizen—Einführung in den analytischen Apparat der Quantenmechanik*, inspired by Wintner himself—from Leipzig; Ames and Murnaghan's *Theoretical Mechanics*, converted into tools for the future by Murnaghan; assorted Rayleigh works on optics, seminars given coherence by A. H. Pfund; and the then hot Rutherford, Chadwick, and Ellis *Radiations from Radioactive Substances*, illuminated by Norman Feather, in his early twenties, whom R. W. Wood had just recruited from Rutherford's own Cavendish Laboratory group. Nuclear physics was clearly the wave of the future. So, in the traditional Hopkins spirit of hands-on involvement in experimental research, after some small work in x rays with Bearden and in spectroscopy with Dieke, I learned from Feather how to determine for myself the 3.5-day half-life of radon. The most important requirement was simple. Sit in the dark for half an hour. Then the fully dilated pupil easily picks up the flash that the alpha particle makes when it hits the zinc sulfide screen. Come back every few hours and repeat the measurement of counting rate. Voilà—the decay curve.

How prophetic it was of the future that I should be asked to give a seminar report on the 1930 paper (9) of W. Bothe and H. Becker on the artificial excitation of nuclear "gamma radiation." How did the excitation get from A to B? A mystery, a puzzle, an enigma! This paper was a doorway to the discovery of the neutron as the neutron was a doorway to the discovery of fission.

A penetrating radiation with mysterious properties! What a challenge to try to understand it. We interested students, especially Robert T. K. Murray and I, followed the subsequent efforts to unravel the mystery, not least among them the attempt (10), the failed 1932 attempt, of Irene Curie and Frederic Joliot, "Émission de protons de grande vitesse par les substances hydrogénées sous l'influence des rayons γ très pénétrants." How brief the time was from the finding of Bothe and Becker to Chadwick's discovery (11) of the neutron! Minds were better prepared for the new particle at the Cavendish Laboratory than anywhere in the world because Rutherford—"Ce jeune homme dévine tout" in the words of Becquerel—had been arguing as early as 1920 that such a particle should exist.[1]

We graduate students raised with each other question after question about the neutron, and followed with excitement each week's new findings. Can neutrons be bottled? Are free neutrons present in the atmosphere? What will a neutron do to a nucleus?

Clearly nuclear physics was becoming wide open territory. So my first year of postdoctoral research found me in New York in the fall of 1933 with Gregory Breit, working on questions of nuclear barrier penetration, resonant and nonresonant nuclear reactions, and the production of electron-positron pairs out of the emptiness of the vacuum.

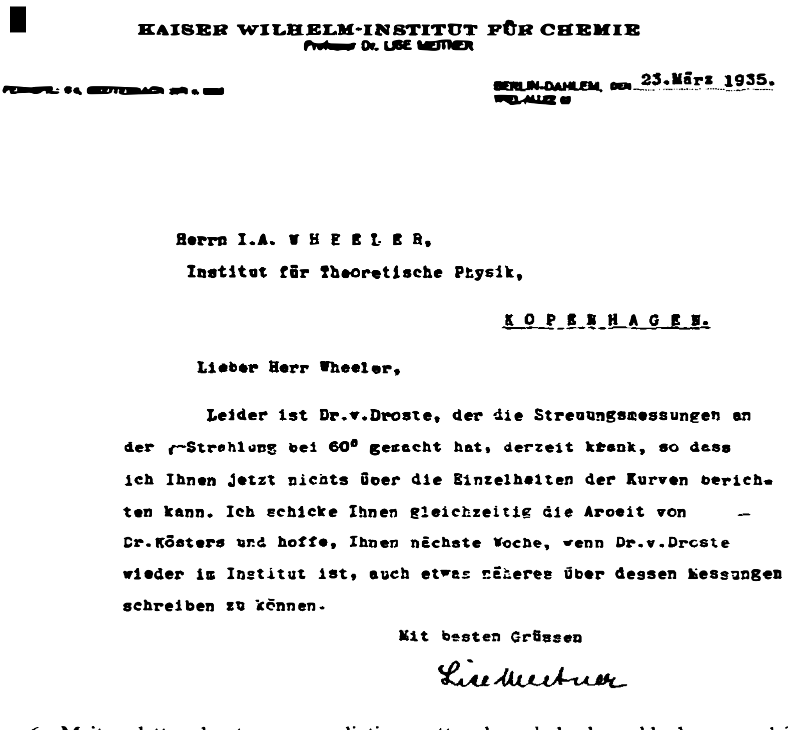
By the spring of 1935, with Breit's support, I was applying to go to Copenhagen for a second year of a National Research Council fellowship, to work with Niels Bohr, because—I wrote—"he sees further ahead than any man alive."

No one starting a year with Bohr could have had a more marvelous introduction to the great men and the great open issues of physics than the International Conference on Physics held at London and Cambridge in the fall of 1934: J. J. Thomson, seventy-seven, frail, and white haired, host at a reception at Trinity College. Ernest Rutherford, head thrown back, in impressive discourse, with a circle of delegates, a lordly presence at an evening reception in the rooms of the Royal Society. Max Born, deprived of his position in Göttingen and newly arrived in the United Kingdom, writing in huge letters on the blackboard, "NUCLEAR PHYSICS," then with eraser and chalk—to laughter—altering the title to read "UNCLEAR PHYSICS." Thirty-three-year-old Enrico Fermi reporting radioactivities produced in many an element by neutron irradiation. Less than a month later (11 a.m., October 27, 1934) came the Rome group's great discovery that hydrogenous substances moderate neutrons and this moderation of the neutrons "increases the activation intensity by a factor which, depending on the geometry used, ranges from a few tens to a few hundreds."

[1] Rutherford in his 1920 Bakerian Lecture to the Royal Society of London had predicted the neutron's likely properties.

There was not one of the many papers (8) in solid-state physics or nuclear physics which I did not find truly significant. One, by Gray and Tarrant (13)—on the anomalous back-scattering of gamma rays—incited me to prove the effect to be, not back-scattering at all, but a minishower. This work later in the Copenhagen year brought me into meeting with Lise Meitner (Figure 6), initiator of experiments of this kind (14) and a guiding spirit of the Hahn-Strassmann uranium work.

Bohr's institute, smaller than many a house, his small group, and the man (Figure 7) and his way of work I have described elsewhere (8, 15). Formulator of complementarity, he was also the personal embodiment of complementarity. Who could switch so totally—as occasion warranted—from one mode of operation to another? From boldness to caution? From breadth to concentration? From one who never makes an advance except when in solitary thought to one who never makes an advance except in discussion with another?



KAISER WILHELM-INSTITUT FÜR CHEMIE
Professor Dr. LISE MEITNER

BERLIN-DAHLEM, den 23.März 1935.

Herrn I.A. W H E E L E R,
Institut für Theoretische Physik,
K O P E N H A G E N.

Lieber Herr Wheeler,

Leider ist Dr.v.Droste, der die Streuungsmessungen an der γ-Strahlung bei 60° gemacht hat, derzeit krank, so dass ich Ihnen jetzt nichts über die Einzelheiten der Kurven berichten kann. Ich schicke Ihnen gleichzeitig die Arbeit von Dr.Kösters und hoffe, Ihnen nächste Woche, wenn Dr.v.Droste wieder im Institut ist, auch etwas näheres über dessen Messungen schreiben zu können.

Mit besten Grüssen

Lise Meitner

Figure 6 Meitner letter about gamma radiation scattered nearly backward by heavy nuclei.

Figure 7 Niels and Margrethe Bohr on motorcycle (Courtesy of Aage Bohr).

Eastertime 1935, Christian Møller returned from a visit to Fermi's group in Rome. Bohr called a seminar to hear and discuss the new findings, most impressive among them being the high cross section of many nuclei for the interception of a neutron. Møller had not gotten half an hour down the road when Bohr interrupted him and took his place. Head lowered, pacing back and forth, he murmured over and over, "Now it comes. Now it comes. Now it comes...". Suddenly it did come. Then and there Bohr sketched out the compound-nucleus model of nuclear reactions.[2] It stood totally at variance with the earlier conception of the nucleus as an open planetary system. The incoming particle, in the new view, has in nuclear matter a mean free path that, compared to nuclear dimensions, is not long but short. The new nucleus, the compound nucleus, retains no memory of how it was formed. How—and how quickly—it breaks up or deexcites depends only on its energy and its angular momentum, not on its history.

[2] Reference (16) contains a carefully documented chronology of the birth and elaboration of the compound-nucleus model. I was not present in Copenhagen for the developments that Bohr, and Bohr and Kalckar, made in this model subsequent to June 1, 1935, but I remember vividly the Eastertime seminar at which the idea arose, as well as the Bohr-Kalckar manuscript of some months later and their report on this work in Washington in February of 1937 (Figure 5).

By the time of the Third Washington Conference on Theoretical Physics of February 18, 1937, Bohr with Fritz Kalckar was well on the way to developing a comprehensive account of nuclear energy levels and nuclear reaction probabilities on the foundation of this compound-nucleus model. To think of the mean free path of nucleons inside the nucleus as short compared to the nuclear diameter was to have justification, they pointed out (17), for adopting a liquid-drop model for the nucleus. This droplet model had been advanced by George Gamow some years earlier [(18); after Bohr's compound-nucleus model (18a)]. However, no one had ever really pushed it until the compound-nucleus model converted it from vision to tool.

The liquid-drop model gave an approximate way to estimate the quantities that totally define the compound-nucleus model of nuclear reactions: (*a*) the energy levels of the nucleus, and (*b*) the probabilities—per second—that the state in question will send out this, that, or the other particle, or a photon, with this, that, or the other energy.

A less global approach to nuclear reaction processes I had imbibed from my 1933–1934 year with Breit: Let scattering be the key to knowledge! Deduce law of force from phase shifts in scattering. Deduce these phase shifts from the variation of scattering cross section with angle (19). Already while I was with him I had started working on the scattering of alpha particles in helium. I was inspired to go on in my Copenhagen time and at Chapel Hill (1935–1938) by new results on alpha-alpha scattering reported at the October 1934 London-Cambridge conference. It was obvious that neither helium nucleus is a simple particle. Therefore new methods had to be developed to treat the interaction between nuclei (20): the scattering matrix to give a precise description of the phenomenology of scattering, and the method of resonating group structure to define an effective interaction potential.

Despite the rich subsequent development of this clustering model by many workers, I—like others—soon found it quicker to make progress in understanding the broad features of nuclear reactions by applying the compound-nucleus model. That model I described and extended in work with graduate students at Chapel Hill and in lectures I gave at Princeton over a period of some weeks in 1937, as well as in the fall of 1938 after I moved to Princeton. Thus somehow fate had put me with the right ideas in the right place at the right time and with the right man when Bohr arrived on January 16, 1939 with the news of fission.

Once at work together, Bohr and I undertook a detailed analysis of fission regarded as an exciting new application of a compound-nucleus-plus-liquid-drop model. This work took not only the three months of Bohr's stay in Princeton but two additional months of finishing up until I could send it in for publication (June 28, 1939). The topics that had to be taken up are seen in this quotation from the paper (21):

> [We] estimate quantitatively in Section I by means of the available evidence the energy which can be released by the division of a heavy nucleus in various ways, and in particular examine not only the energy released in the fission process itself, but also the energy required for subsequent neutron escape from the fragments and the energy available for beta-ray emission from these fragments.
>
> In Section II the problem of the nuclear deformation is studied more closely from the point of view of the comparison between the nucleus and a liquid droplet in order to make an estimate of the energy required for different nuclei to realize the critical deformation necessary for fission.
>
> In Section III the statistical mechanics of the fission process is considered in more detail, and an approximate estimate made of the fission probability. This is compared with the probability of radiation and of neutron escape. A discussion is then given on the basis of the theory for the variation with energy of the fission cross section.
>
> In Section IV the preceding considerations are applied to an analysis of the observations of the cross sections for the fission of uranium and thorium by neutrons of various velocities. In particular it is shown how the comparison with the theory developed in Section III leads to values for the critical energies of fission for thorium and the various isotopes of uranium which are in good accord with the considerations of Section II.
>
> In Section V the problem of the statistical distribution in size of the nuclear fragments arising from fission is considered, and also the questions of the excitation of these fragments and the origin of the secondary neutrons.
>
> Finally, we consider in Section VI the fission effects to be expected for other elements than thorium and uranium at sufficiently high neutron velocities as well as the effect to be anticipated in thorium and uranium under deuteron and proton impact and radiative excitation.

A new feature of capillarity entered in the case of fission, the concept of fission barrier. The very idea was new and strange. More than one distinguished colleague objected that no such quantity could even make sense, let alone be defined. According to the liquid-drop picture, is not an ideal fluid infinitely subdivisible? And therefore cannot the activation energy required to go from the original configuration to a pair of fragments be made as small as one pleases? We obtained guidance on this question from the theory of the calculus of variations in the large, maxima and minima, and critical points. This subject I had absorbed over the years by osmosis from the Princeton environment, so thoroughly charged by the ideas and results of Marston Morse (22). It became clear that we could find a configuration space to describe the deformation of the nucleus. In this deformation space we could find a variety of paths leading from the normal, nearly spherical configuration over a barrier to a separated configuration. On each path the energy of deformation reaches a highest value. This peak value differs from one path to another. Among all these maxima, the minimum measures the height of the saddle point or fission threshold or the activation energy for fission. The fission barrier *was* a well-defined quantity!

Bohr knew from his own student research on water jets that a work of Lord Rayleigh would have something to say about the capillary oscillations of a liquid drop. We rushed up to the library on the next floor of Fine Hall and looked it up in the *Scientific Papers* of Rayleigh. This work furnished a starting point for our analysis. However, we had to go to terms of higher order than Rayleigh's favorite second-order calculations to pass beyond the purely parabolic part of the nuclear potential, that is, the part of the potential that increases quadratically with deformation. We determined—as soon also did Feenberg, von Weizsäcker, Frenkel, and others—the third-order terms to see the turning down of the potential. These terms enabled us to evaluate the height of the barrier, or at least the height of the barrier for a nucleus whose charge was sufficiently close to the critical limit for immediate breakup.

We found that we could reduce the whole problem to finding a function f of a single dimensionless variable x. This "fissility parameter" measures the ratio of the square of the charge to the nuclear mass. This parameter has the value 1 for a nucleus that is already unstable against fission in its spherical form. For values of x close to 1, by a power series development we could estimate the height of the barrier and actually give quite a detailed calculation of the first two terms in the power series for barrier height, or f, in powers of $(1-x)$. The opposite limiting case also lent itself to analysis. In this limit the nucleus has such a small charge that the barrier is governed almost entirely by surface tension. The Coulomb forces give almost negligible assistance in pushing the material apart.

Between this case (the power series about $x = 0$) and the other case (the power series about $x = 1$) there was an enormous gap. We saw that it would take a great amount of work to calculate the properties of the fission barrier at points in between. Consequently we limited ourselves to interpolation between these points. In the decades since that time many workers (among them Wladyslaw J. Swiatecki at Berkeley, Vladimir Strutinski and his colleagues in the USSR, and Ray Nix and his colleagues at Los Alamos) have revealed many previously unsuspected features of the fission barrier. This is not the place to go into the deeper theoretical considerations on prompt neutrons, delayed neutrons, the physics of fission product decay, and many another topic that came up, nor to detail the many impressive experimental results that were obtained on these and other topics week by week.

Two other issues of comparable or greater challenge came up in doing our paper: (*a*) figuring the rate of decay associated with spontaneous fission, and (*b*) determining the probability for fission of a nucleus with excitation in excess of the barrier summit. The first of these forced us to introduce a measure for the inertia associated with a deformation. The second led us to the concept of channels for fission associated with the transition state. Both introduced lines of thought still under active exploration and development today.

For our immediate needs, however, our simple "poor man's" interpolation was adequate. With it, knowing—or estimating from observation—the fission barrier for one nucleus, we could estimate the fission barrier for all the other heavy nuclei, among them plutonium 239. Thanks to the questioning of Louis A. Turner (Figure 8), soon to write his great and timely review of nuclear fission (23), we came to recognize that this substance, which up to then one had never seen except through its radioactivity [McMillan & Abelson (24), June 15, 1940[3]] would be fissile. This conclusion was soon to lead to a preposterous dream: by means of a neutron reactor such as never before existed, one could manufacture kilograms of an element never before seen on Earth. By the fall of 1944 the E. I. duPont de Nemours Company had converted this dream into the reality of a double alchemy. In total silence neutrons flow from place to place, splitting some uranium nuclei, converting others (the U^{238}) to U^{239}. Then nature itself took hold to complete the alchemy. Over the ensuing hours and days it let the U^{239} spontaneously transform—through two beta transformations—to Pu^{239}. By this double miracle, duPont at Hanford, Washington, was able, week by week, to supply ponderable masses of a strange and totally new element, plutonium, to its Los Alamos, New Mexico customer.

The barrier height of a compound nucleus against fission was not the only factor relevant for fission. Equally important in governing the probability of this process was the excitation, or "heat of condensation," delivered by the uptake of a neutron to form the compound nucleus in the first place. On this point an important development occurred on a snowy morning when I was occupied with classes and not with Bohr. He, having breakfast at the Nassau Club with Rosenfeld and with an arrival of the night before, George Placzek, faced Placzek's continuing skepticism about the very existence of fission. Placzek asked, how can it possibly make sense that slow neutrons and fast neutrons cause uranium to split but not neutrons of intermediate energy? Bohr stopped but said not a word, left with Rosenfeld, crossed the campus to Fine Hall still without a word and there, when Placzek and I joined them, explained the great idea (25) that had just come to him: that the slow neutron fission takes place in the rare

[3] According to Irving (24a), J. Schintlmeister and F. Hernegger identified neptunium and plutonium in June 1940 but reported their findings only at the end of 1940.

Figure 8 Louis A. Turner, who pointed the way to the manufacture of plutonium. Courtesy of Argonne National Laboratory Photographic Archives.

isotope U^{235} and the fast neutron fission in the abundant isotope U^{238}. Thus an incoming neutron delivers up a high heat of condensation when it enters into a nucleus with 143 neutrons, because it can form a new neutron pair. This excitation puts the compound nucleus U^{236} over the barrier summit. Therefore, the U^{236} must split when it is formed by slow neutron capture. Moreover, the cross section for fission of the rare U^{235}, like the cross section for the "fission" of boron,

$$n^1 + B^{10} \rightarrow He^4 + Li^7,$$

must exceed by far the geometrical cross section of the nucleus for sufficiently slow neutrons. That circumstance makes it understandable why an isotope present to only one part in 139 imparts to natural uranium the observed substantial fission cross section. However, the cross section must fall off inversely as the velocity of the neutron for U^{235} as for B^{10}; hence the negligible fission cross section of natural uranium for neutrons of intermediate energy. In contrast, when a slow neutron enters U^{238} to form the compound nucleus U^{239}, no new neutron pair is formed. The heat of condensation delivered up is not enough to exceed the fission barrier. Only neutrons of a substantial kinetic energy striking U^{238} can produce U^{239} with enough energy to surmount the barrier. Hence, the existence of fast neutron fission in natural uranium.

Was it reasonable to expect so great a difference between U^{235} and U^{238} from the estimated odd-even difference in neutron binding? Could not the fission barrier differ equally drastically from the one nucleus to the other? Might not this difference be the dominant factor? How could one be sure that the proposed attribution of slow fission to U^{235} and fast fission to U^{238} really made sense until one was clear about these energies? Fortunately Bohr and I had just been through the systematics of nuclear energies in the course of calculating the release of energy in various actual and potential fission processes. Therefore, we could estimate the difference between the excitation developed by neutron capture in the two uranium isotopes as almost a million volts, in favor of fission of U^{235}. From our interpolation for fission barriers we estimated on the other hand a barrier almost 1 MeV lower for U^{235} than for U^{238}. Thus we concluded there was about a 2-MeV margin in favor of the fission of the rare isotope. In later years, after the development of the collective model it became clear that individual particle effects can modify significantly barrier heights and barrier shapes from the predictions of the simple liquid-drop model. However, the qualitative conclusions are not affected; U^{235} is the fissile nucleus.

Placzek, wonderful person that he was, a man of the highest integrity, often a thoroughgoing skeptic about new ideas, said to me over and over in those early spring days of 1939 that he could not believe that the small amount of U^{235} could be the cause of the slow neutron effects in natural uranium. I therefore bet him a proton to an electron, \$18.36 to a penny, that Bohr's diagnosis was correct. A year later Alfred Nier at Minnesota had separated enough U^{238} to make possible a test and sent it to John Dunning at Columbia to measure its fission cross section (26). On April 16, 1940, I received a Western Union money order telegram for one cent with the one-word message "Congratulations!" signed Placzek (27).

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