The discovery of fission

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The Discovery of Fission

Initial formulations of nuclear fission are colored with the successes, failures and just plain bad luck of several scientists from different nations. The winning combination of good fortune and careful thought made this exciting concept a reality.

by Otto R. Frisch and John A. Wheeler

Otto R. Frisch, professor of natural philosophy (physics) at Cambridge University, England, did research in Berlin (1927–30), Hamburg (1930–33), London (1933–34), Copenhagen (1934–39) and Birmingham (1939–40). During the war he worked on the A-bomb at Los Alamos. He was first to observe energy liberated in the fission of a single uranium nucleus.

John A. Wheeler, one of the first American scientists to concentrate on nuclear fission, worked at the U. of Copenhagen in 1934 as a National Research Fellow with Niels Bohr. Wheeler received his PhD in physics at the Johns Hopkins University prior to his research in Copenhagen. In 1938 he joined Princeton's physics department, where he remains active.

How It All Began

by Otto R. Frisch

THE NEUTRON was discovered in 1932. Why, then, did it take seven years before nuclear fission was found? Fission is obviously a striking phenomenon; it results in a large amount of radioactivity of all kinds and produces fragments that have more than ten times the total ionization of anything previously known. So why did it take so long? The question might be answered best by reviewing the situation in Europe from an experimentalist's point of view.

Research in Europe

In Europe there were few laboratories in which nuclear-physics research was conducted, and I think the word "team" had not yet been introduced into scientific jargon. Science was still pursued by individual scientists who worked with only one or two students and assistants.

Paris harbored some of the most active research laboratories in Europe. It is the city in which radioactivity had been discovered and where Madame Curie was working until her death in 1934. She still dominated the situation: Techniques were quite similar to those used at the turn of the century; that is, ionization chambers and electrometers. This state of affairs is good enough for performing accurate measurements on natural radioactive elements, but it is not really adequate for much of the work on nuclear disintegration. Madame Curie had little respect for theory. Once, when one of her students suggested an experiment, adding that the theoretical physicists next door thought it hopeful, she replied, "Well, we might try it all the same." Their disregard of theory may have cost them the discovery of the neutron.

Cambridge is the second place worthy of discussion. Ernest Rutherford, whose towering personality dominated Cambridge research, had split atomic nuclei in 1919; since 1909 he had, in fact, been keenly concerned with the observation and counting of individual nuclear particles. He first introduced the scintillation method and stuck firmly to it. His great preference was for simple, unsophisticated methods, and he possessed a strong distrust of any complicated instrumentation. Even in 1932, when John Cockcroft and Ernest Walton first disintegrated nuclei by artificially-accelerated protons, they used scintillations to detect the process. By that time Rutherford had realized that electronic methods of particle counting must be developed. The reason was that the scintillation method clearly had its shortcomings. It did not work for very low or high counting rates and was not really reliable. This deficiency was highlighted by the results that came from the third laboratory I want to mention—Vienna.

Vienna is where I began my career and it was in those days a sort of *en-*

fant terrible of nuclear physics. Several physicists were claiming that not only nitrogen and one or two others of the light nuclei could be disintegrated by alpha particles but that practically all of them could and did give many more protons than anybody else could observe. I still do not know how they found these wrong results. Apparently they employed students to do the counting without telling them what to expect. On the face of it, that operation appears to be a very objective method because the student would have no bias; yet the students quickly developed a bias towards high numbers because they felt that they would be given approval if they found lots of particles. Quite likely this situation caused the wrong results along with a generally uncritical attitude and considerable enthusiasm over beating the English at their own game.

I still remember when I left Vienna at just about that time (after having escaped the duty of counting scintillations). My supervisor, Karl Przibram, told me with sadness in his voice, "You will tell the people in Berlin, won't you, that we are not quite as bad as they think?" I failed to persuade them.

Germany had nuclear-physics research in several places. The team of Otto Hahn and Lise Meitner, which had been one of the first groups to study radioactive elements, had at that time separated to carry out independent research. Hahn was working on various applications of radioactivity for the study of chemical reactions, structures of precipitates and similar subjects, whereas Lise Meitner was using radioactive materials chiefly to elucidate the processes of beta and gamma emission and the interaction of gamma rays with matter.

In addition, Hans Geiger was in Germany. He had been with Rutherford from 1909 onwards, in the early days before the nucleus was discovered. Rutherford felt uncertain about the scintillation method and asked Geiger to develop an electric counter to check on it. But as soon as Rutherford saw that the two gave the same results, Rutherford returned to the scintillation method, which appeared to be simpler and more reliable when used with proper precaution. Geiger went back to Germany and perfected his electric counters, and in 1928, together with a student named W. Müller, he developed an improved counter that could count beta rays. Earlier counters were inadequate for this purpose, and scintillation methods were also incapable of detecting beta rays. However the new counters were still very slow because the discharge between the central wire and the cylindrical envelope was quenched by a large resistor of many megohms placed in the circuit; consequently the counting rate was limited to numbers not much greater than with the scintillation method. Even at a few hundred particles a minute there were quite large corrections to be applied.

Walther Bothe was the first to use the coincidence method, both in an attempt to do something about cosmic rays and also for measuring the energy of gamma rays by the range of the secondary electrons they produced. This was really the first reliable method for measuring the energy of weak gamma radiations.

Until 1932, the only source of particles for doing atomic nuclear disintegration was natural alpha particles: either polonium, which was difficult to come by (in fact one practically had to go to Paris) or sources of one of the short-lived decay products of radium, which were very clean but were short-lived and usually had lots of gamma radiation.

The year of discovery

But in 1932, that *annus mirabilis*, not only the neutron was discovered but two other developments took place. In the US Ernest O. Lawrence made the first cyclotron that showed promise of being useful, and in England Cockcroft and Walton built the first accelerator for protons capable of producing nuclear disintegrations. I need not state that this was the beginning of an enormous development; most of nuclear physics as we know it would have never come about without at least one of those two instruments. But the interesting thing is that they played practically no role in that narrow thread that led to the discovery of nuclear fission.

I do not want to dwell on the discovery of the neutron very much because it was discussed in several interesting lectures in 1962 at the History of Science Congress held in Ithaca, New York. The published proceedings contained interesting contributions by Norman Feather and Sir James Chadwick, who showed that the neutron was discovered in Cambridge, not simply by chance with everybody else having done the groundwork, but because a search for the neutron had been going on in Cambridge (admittedly with wrong ideas). The people at Cambridge were keyed up for this discovery. They had made one observation that was important and that tends to be overlooked: H. C. Web-

$$(1)\ {}_{92}\mathrm{U} + n \rightarrow ({}_{92}\mathrm{U} + n) \xrightarrow[10\,\mathrm{Sek.}]{\beta} {}_{93}\mathrm{EkaRe} \xrightarrow[2{,}2\,\mathrm{Min.}]{\beta} {}_{94}\mathrm{EkaOs} \rightarrow$$
$$\xrightarrow[59\,\mathrm{Min.}]{\beta} {}_{95}\mathrm{EkaIr} \xrightarrow[66\,\mathrm{Std.}]{\beta} {}_{96}\mathrm{EkaPt} \rightarrow$$
$$\xrightarrow[2{,}5\,\mathrm{Std.}]{\beta} {}_{97}\mathrm{EkaAu}\ ?$$

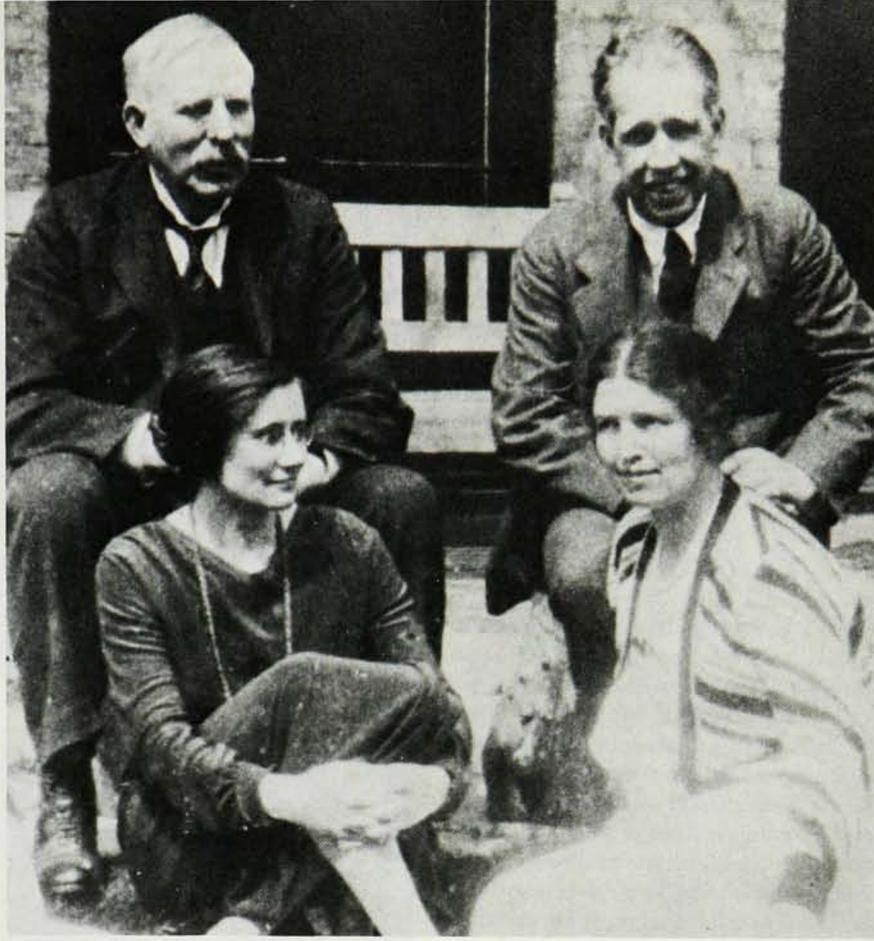
$$(2)\ {}_{92}\mathrm{U} + n \rightarrow ({}_{92}\mathrm{U} + n) \xrightarrow[40\,\mathrm{Sek.}]{\beta} {}_{93}\mathrm{EkaRe} \xrightarrow[16\,\mathrm{Min.}]{\beta} {}_{94}\mathrm{EkaOs} \rightarrow$$
$$\xrightarrow[5{,}7\,\mathrm{Std.}]{\beta} {}_{95}\mathrm{EkaIr}\ ?$$

$$(3)\ {}_{92}\mathrm{U} + n \rightarrow ({}_{92}\mathrm{U} + n) \xrightarrow[23\,\mathrm{Min.}]{\beta} {}_{93}\mathrm{EkaRe}\ ?$$

COMPUTATIONS, indicating chains of radioactive elements, were published in a 1938 *Die Naturwissenschaften* article by Hahn, Meitner and Strassmann. —FIG. 1

ster showed that those queer penetrating rays that beryllium emitted when alpha particles fell on it were more intense in the forward direction than in the backward direction. This result was quite incomprehensible if the radiation were gamma rays as everybody believed. Even the French physicists Curie and Joliot shared that belief in the teeth of all theoretical predictions. Then Chadwick's experiment showed clearly that the mysterious radiation consisted of particles having approximately the mass of the proton. There was a bit of confusion at the time because the word "neutron" had been used by Enrico Fermi and Wolfgang Pauli to indicate the particle that later came to be called the "neutrino."

After the neutron was discovered, there was of course a certain rush of activity, but nobody knew quite what to do. Neutrons were rather few in number. They were, after all, secondary products of nuclear disintegration. With only natural alpha sources available at first, neutron production was low.



GREAT AND GOOD FRIENDS. Lord and Lady Rutherford (left) with Niels and Margrethe Bohr in Rutherford's garden. The photograph was taken about 1930.

Moreover the main instrument for detection was essentially the cloud chamber. With cloud chambers only a limited number of tracks due to neutrons could be found. And it was slow work to make any sense out of the few detected tracks of recoil nuclei. Leo Szilard once joked that if a man suddenly does something unexpected there is usually a woman behind it, but if an atomic nucleus suddenly does something unexpected, there is probably a neutron behind it.

Electronic counting methods had only just been developed; largely as a reaction to the wrong results coming out of Vienna that nobody else could confirm, it had been decided that it really was necessary to build electronic amplifiers and counters. Actually the Viennese themselves started that kind of work but were not very successful. The work was also started in Switzerland with some success by Hermann Greinacher. Yet I think the main thread that led to the development of decent counters took place in England, where Charles Wynn-Williams used proper screening and tubes with low noise level etc. to produce electronic counters. Nevertheless those counters, although Chadwick had used them with good effect to pin down the neutron, were still too noisy to be of much use.

Artificial radioactivity

Things really got moving when, in 1934, artificial radioactivity was found by Curie and Joliot. I think they must have been very happy to have made up for their failure to spot the neutron two years previously. Almost to the day two years previously both discoveries came out in the middle of January. They had known for many months before that aluminum bombarded with alpha particles emits positrons, but it had never occurred to them that this might be a delayed process. They had only observed the positron during bombardment. Lawrence and his cyclotron people in California had made the same mistake. In fact they had noticed that the counters misbehaved after the cyclotron was switched off. I am told that they built in special gadgetry so that the counters were automatically switched off together with the cyclotron! Otherwise they would have found artificial radioactivity before the French.

It is astonishing that nobody appears to have thought beforehand that the result of a nuclear disintegration might be an unstable nucleus although the existence of unstable nuclei had, of course, been known for thirty years or more. I have been told that, after the discovery, Rutherford wrote to Joliot and congratulated him on his discovery saying that he himself had thought that some of the resulting nuclei might be unstable, but had always looked for alpha particles only because he was not really interested in beta particles.

As soon as this work became known in January 1934 a lot of people rushed to repeat and extend the experiment. But most of them rushed in a straight line indicated by Curie-Joliot, bombarding other elements with alpha particles. (So did I in Blackett's laboratory in London.)

But in Rome Fermi at that time had already decided that nuclear physics was an important and interesting line, and he had started to set up some instrumentation. So when this discovery came along, he began working quite fast to see whether neutrons would form radioactive nuclei.

I remember that my reaction and probably that of many others was that Fermi's was a silly experiment because neutrons were much fewer than alpha particles. What that simple argument overlooked of course was that they are very much more effective. Neutrons are not slowed down by electrons, and they are not repelled by the Coulomb field of nuclei. Indeed, within about four weeks of the discovery by Curie and Joliot, Fermi published the first results proving that various elements did become radioactive when bombarded with neutrons. Only another month later he announced that bombarding uranium produced some new radioactivity that he felt must be due to transuranic elements. Because both on theoretical grounds (Coulomb barrier and all that) and as far as the experiments confirmed it, all heavier elements were known to absorb neutrons without splitting anything off. And so it was felt that must also be the case with uranium.

This work was of course considerably interesting to radiochemists. Several took it up, but once again, oddly enough, one false result started things really moving—a note by Aristid von Grosse, a German-born chemist working in the US, who thought one of these elements behaved like protactinium. He had done some of the early work with Hahn on protactinium soon after it was discovered in 1917; so his suggestion put Hahn and Meitner on their mettle. They felt protactinium was their own baby and they were going to check it. Lise Meitner persuaded Hahn to join forces again. They soon showed that von Grosse was wrong: It was not protactinium. On the other hand there were so many odd things there that they were captured by this phenomenon and had to go on. The results were most peculiar.

Figure 1 shows one of the tabulations indicating the chains of radioactive elements that Hahn and Meitner had thought identified them. They did not give new names to the transuranic elements that they thought they had identified, but they used the prefix "eka" to indicate that they were higher homologues of rhenium, osmium, etc. up to ekagold. Obviously, Hahn was excited to have a whole new lot of chemical elements to play with and to study their properties. Today, of course, these elements after uranium are known as neptunium, plutonium, americium etc., and are known to be chemically quite different from those that Hahn was studying.

LINKS IN THE CHAIN. Cockcroft (top) and Walton contributed to the new ideas when they disintegrated nuclei by artificially-accelerated protons.

Parallel chains

The results were astonishing for two reasons. In the first place, it appeared that there were three parallel series. And from the yields obtained they must all derive from uranium 238 or possibly one of them from 235 (which is already much rarer). So it looked as if there were at least two parallel chains of isomeric elements. This isomeric property had to be propagated all along the chain of beta disintegrations.

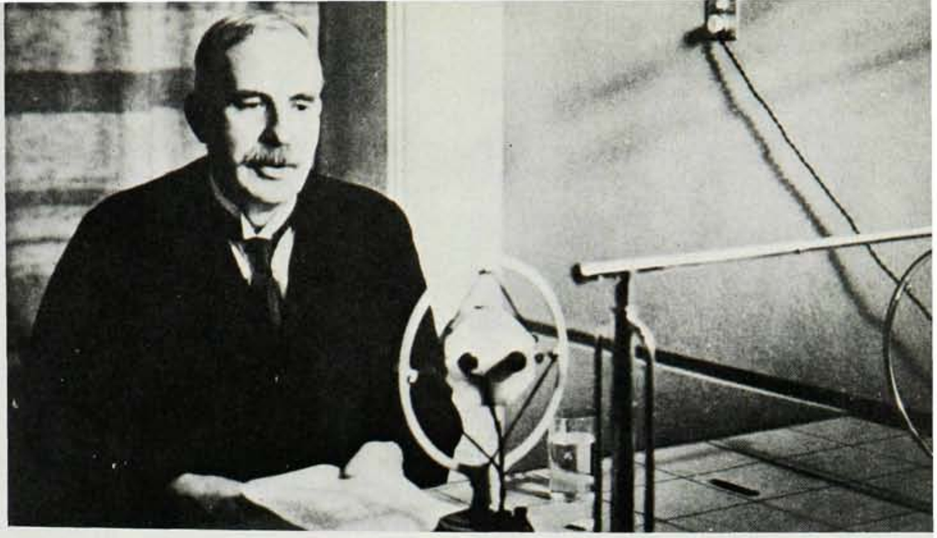
Nuclear isomerism was still fairly new in 1938, and its interpretation was not altogether clear. It had been suggested (as we now accept) that it was due to high angular momentum, but there were also proposals that it might be due to the existence of rigid structures inside nuclei. One could imagine that such a rigid structure might survive a beta decay and might influence the half-life of the subsequent product.

But then there was still the mystery of the great length of those chains. Uranium, after all, was not beta unstable itself. The other elements in that region never had more than two beta decays in succession; yet here four or five had been found. So Hahn the chemist was delighted by so many new elements, but Hahn the radiophysicist or radiochemist was rather worried about the mechanism that could account for them.

All this work was made difficult by the political situation in Germany. Hitler was in power and the institute had to play a delicate game of politics to prevent racial persecution from removing some of its personnel. In 1938, when Austria was occupied by the Nazis, Lise Meitner felt very insecure; rumors began to float around that she might lose her post and be prevented thereafter from leaving Germany because of her knowhow. A certain amount of panic resulted. Dutch colleagues offered to smuggle her to Holland without a visa. Thus she left Germany in the early summer of 1938, went from Holland for a brief stay in Denmark, and was offered hospitality by Manne Siegbahn at the Nobel Institute in Stockholm.

Near misses

After that, the team that had already brought Strassmann in with Hahn as a second chemist had to carry on without her. In the meantime some work had been started in Paris. It is interesting that they had a different angle. They were at first not so interested in



RUTHERFORD was the first to use scintillation methods to detect particles.

the transuranic elements; but they realized that if thorium is bombarded with neutrons, one ought to find the beginning of the new and missing radioactive chain with the atomic weight $4n + 1$. One realizes that the others, $4n$, $4n + 2$, $4n + 3$, are all represented by the natural radioactive series. But the $4n + 1$ was missing, and so Irene Curie, the daughter of Madame Curie, together with Hans von Halban, an Austrian, and Peter Preiswerk, a Swiss, set out to search for that series and published some work on it.

Later that team broke up because Halban came to Copenhagen and, for a time, worked with me on the study of slow neutrons. Irene Curie found a new collaborator in Pavel Savitch, a Yugoslav. They tried to disentangle the transuranic elements. Having realized that there was a great variety of different materials, Irene Curie had the good idea of selecting one of them simply by the high penetration of its beta rays. They covered their samples with a fairly thick sheet of brass and only studied the substance whose radiation penetrated. They did not realize that even that method might not select a single substance although the substance appeared to have a reasonably unique lifetime of 3.5 hours. From the chemical behavior they first thought it looked like thorium.

This work was checked by Hahn, who concluded that it was not thorium and wrote so to Paris. Curie and Savitch continued the work and in a later paper in the summer of 1938 acknowledged that the 3.5-hour substance was not thorium but behaved a bit more like actinium and even more like lanthanum. She had come very close indeed to the concept of nuclear fission but unfortunately did not state it clearly. She said that it was definitely not actinium and that it was quite similar to lanthanum, "from which it could be separated only by fractionation." But she did think it could be separated. The reason was probably that she still had a mixture of two substances; in that case of course one does effect a partial separation. Then this work was in turn checked by Hahn and Strassmann who discovered radioactive products that behaved partly like actinium, partly a bit like radium.

There was another near miss at about the same time: Gottfried von Droste, a physicist working with Lise Meitner, looked for long-range alpha rays from uranium during neutron bombardment. If he had supressed the ordinary alpha rays by applying a bias to the amplifier, he would not have failed to find fission. Unfortunately instead of using a bias he used a foil, and that foil was thick enough to stop not only uranium alpha rays but also the fission fragments; nor did he find any long-range alpha rays, which had to be there if radium or actinium isotopes were formed.

Then Hahn and Strassmann checked the chemical properties of this "radium" with care and found that they were identical with those of barium.

A propitious visit

This is where I came in because Lise Meitner was lonely in Sweden and, as her faithful nephew, I went to visit her at Christmas. There, in a small hotel in Kungälv near Göteborg I found her at breakfast brooding over a letter from Hahn. I was skeptical about the contents—that barium was formed from uranium by neutrons—but she kept on with it. We walked up and down in the snow, I on skis and she on foot (she said and proved that she could get along just as fast that way), and gradually the idea took shape that this was no chipping or cracking of the nucleus but rather a process to be explained by Bohr's idea that the nucleus was like a liquid drop; such a drop might elongate and divide itself. Then I worked out the way the electric charge of the nucleus would diminish the surface tension and found that it would be down to zero around $Z = 100$ and probably quite small for uranium. Lise Meitner worked out the energies that would be available from the mass defect in such a breakup. She had the mass defect curve pretty well in her head. It turned out that the electric repulsion of the fragments would give them about 200 MeV of energy and that the mass defect would indeed deliver that energy so the process could take place on a purely classical basis without having to invoke the crossing of a potential barrier, which of course could never have worked.

We only spent two or three days together that Christmas. Then I went back to Copenhagen and just managed to tell Bohr about the idea as he was catching his boat to the US. I remember how he struck his head after I had barely started to speak and said: "Oh, what fools we have been! We ought to have seen that before." But he had not—nobody had.

Lise Meitner and I composed a paper over the long-distance telephone between Copenhagen and Stockholm. I told the whole story to George Placzek, who was in Copenhagen, before it even occurred to me to do an experiment. At first Placzek did not believe the story that these heavy nuclei, already known to suffer from alpha instability, should also be suffering from this extra affliction. "It sounds a bit," he said, "like the man who is run over by a motor car and whose autopsy shows that he had a fatal tumor and would have died within a few days anyway." Then he

THE JOLIOT-CURIES discovered artificial radioactivity.

CENTRAL FIGURES in the discovery were Otto Hahn and Lise Meitner, here shown in front of the institute that bears their names

said, "Why don't you use a cloud chamber to test it?" I did not have a cloud chamber handy and thought it would be difficult anyway. But I used an ionization chamber and it was a very easy experiment to observe the large pulses caused by ion fragments.

I do not think chronology means very much and certainly cannot claim any particular intelligence or originality. I was just lucky to be with Lise Meitner when she received advance notice of Hahn's and Strassmann's discovery. Then I had to be nudged before I did the crucial experiment on 13 January. By that time our joint paper was nearly written. I held it back for another three days to write up the other paper, and then they were both sent to *Nature* on 16 January but published a week apart. In the first paper I used the word "fission" suggested to me by the American biologist, William A. Arnold, whom I asked what one calls the phenomenon of cell division.

The second paper also contained a suggestion from Lise Meitner that fission fragments emerging from a bombarded uranium layer could be collected on a surface and their activity measured. The same thought independently occurred to Joliot, and he successfully did this experiment on 26 January. About that same time the news reached the US; what happened then is discussed by Wheeler.

Serendipitous searches

To come back to my initial question: Why did it take so long before fission was recognized? Indeed, why wasn't the neutron found earlier? Rutherford thought about it and foretold some of its properties as early as his Bakerian lecture in 1920; but Joliot did not read it, expecting a public lecture to contain nothing new! When Curie and Joliot found that the "beryllium radiation" ejected protons from paraffin, they put it down to a kind of Compton effect of a very hard gamma radiation (some 50 MeV), ignoring the objections of theoretical physicists. The neutron was finally observed in Cambridge, where such a particle was expected and had been sought.

At the time the neutron was found in 1932 pulse amplifiers and ionization chambers were available for a facile detection of fission pulses. But that would have been too big a jump to expect. The liquid-drop model of the nucleus was born late; the compound-nucleus idea was conceived by Bohr only late in 1936. It would have been a stroke of genius to think of fission then, and nobody did.

The discovery of artificial radioactivity in 1934 was again a chance discovery; no one had looked for it except Rutherford, who looked in vain for alpha decay. And indeed the Berkeley team turned a blind eye when their counters "misbehaved." After the discovery there was a sheep-like rush to repeat the experiment with only the most obvious variation (I was one of the sheep). Only Fermi had the intelligence to strike out in a different and tremendously fruitful direction.

But then Fermi got on the wrong track: He felt sure that uranium, like other heavy nuclei, would obediently swallow any slow neutron that fell on it. He did make sure that the radioactive substances that were formed from it were different from any of the known elements near uranium. Ida Noddack, a German chemist, quite rightly pointed out that they might be lighter elements; but her comments (published in a journal not much read by chemists and hardly at all by physicists) were regarded as mere pedantry. She did not indicate how such light elements could be formed; her paper had probably no effect whatever on later work.

In the end it was good solid chemistry that got things on the right track. Irene Curie and Pavel Savitch came very close to it; only the presence of two substances with maliciously similar properties prevented them from establishing uranium fission before Hahn and Strassman finally accomplished it. □

Mechanism of Fission

by John A. Wheeler

In early January 1939 the Swedish-American liner, MS Drottningholm carried a short message across the stormy sea from Copenhagen to New York. This message symbolized the steady transfer of nuclear discoveries from Europe to the US that had been going on during the Hitler years.

Although these transfers were fateful for the US and the rest of the world, the act of relaying this particular message was simple: a few words spoken by Otto Frisch to Niels Bohr on the pier in Copenhagen and a few words spoken to Enrico Fermi and me by Bohr on the pier in New York. As a junior participator in the events that occurred then and in subsequent months, I shall relate the activities that led to the publication of a *Physical Review* paper by Bohr and me. In this paper we summarized the thoughts expressed in the message: the liquid-drop model that Frisch had applied to the mechanism of fission and the determinations of packing fraction that Lise Meitner considered when arriving at the first estimate of energy release in fission.

No one looking at such a novel process at that time could fail to call on everything he knew about nuclear physics to seek an interpretation. Fortunately the key ideas for unraveling the puzzle had already been developed. It may be appropriate to recall what had been learned about nuclear physics in the preceding half a dozen years.

Clues to the answer

1933 was a fruitful year for someone like me, who was just earning his doctor's degree. It was the year of the discovery of the neutron and Werner Heisenberg's great paper on the structure of nuclei built out of neutrons and protons. These discoveries made one feel that he might soon know as much about the nucleus as he already knew about the atom.

Encouraged by the vision that inspired so many young men, me included, at that time, I spent 1933–34 working with Gregory Breit, to whose insights I owe so much. He and the group of which I soon found myself a member accepted almost unconsciously the model of the nucleus of that day: neutrons and protons moving in a common self-consistent potential, closely analogous to the electric potential of the atom. "Unconscious" our acceptance of the model was, yes; but also shadowy. None of us took it too literally, especially not Breit, with his caution and insight. Thus he was always willing to consider alpha particles in the nucleus as well as neutrons and protons when that point of view made sense in considering a particular reaction. Breit also directed especial attention to areas of investigation as nearly free as possible of model-dependent issues. Thus much work was done on the penetration of charged particles into nuceli and how the cross section for a nuclear reaction depends on energy. The analysis of scattering processes in terms of phase shifts also received much attention.

With Breit's warm endorsement I spent the following year at Niels Bohr's institute in Copenhagen. Here I was initiated into the study of many new ideas, but nothing was more impressive in nuclear physics than the message that Møller brought back during the spring of 1935 from a short Easter visit to Rome: It told of Fermi's slow-neutron experiments and the astonishing resonances that he had discovered. Every estimate ever made before then indicated that a particle passing through a nucleus would have an extremely small probability of losing its energy by radiation and undergoing capture if the current nuclear model was credible. Yet, directly in opposition to the predictions of this model, Fermi's experiments displayed huge cross sections and resonances that were quite beyond explanation.

Of course a number of weeks went by before the most significant results of this discovery could be sorted out. Everyone was actively concerned, but no one more so than Bohr, who paced up and down in the colloquium and took a central part in discussions.

Liquid drops

The story of the development of the liquid-drop model and the compound-nucleus picture is a familiar one. What is not so clear and was certainly not evident at the time is the distinction between these ideas: (1) The compound-nucleus model shows, in essence, that the fate of a nucleus is independent of the mechanism by which it has been formed, and (2) the liquid-drop model is, so to speak, a special case of the compound-nucleus model, a particular way of making such a model of nuclear structure reasonable. Bohr proposed that the mean free path of nucleon is short in relation to nuclear dimensions instead of being long, as assumed in all previous estimates. This new idea made something like a liquid-drop model exceedingly attractive.

No one looking back on the situation from today's vantage point can fail to be amazed at "the great accident of nuclear physics"—the circumstance that the mean free path of particles in the nucleus is neither extremely short compared with nuclear dimensions (as assumed in the liquid-drop picture) nor extremely long (as assumed in the earlier model) but of an intermediate value. Moreover, all the marvelous detail of nuclear physics turns out to depend in such a critical way on the value of this parameter. As Aage Bohr and Ben Mottelson have taught us in recent years, no one could have predicted the precise one among many alternative regimes in which the phenomenology would actually lie from any advance estimate of the mean free path. Only observation could suffice! Knowing as little as one did in 1935 about the value of this decise parameter, still less about its cirticality, one had no option but to explore with all vigor the idea that the mean free path is very short.

The development of the liquid-drop model, which was applied to a variety of processes, took place in the hands of Fritz Kalckar and Niels Bohr in 1935–37. They applied it to a variety of processes. At the center of every such application stood the idealization of the compound nucleus, that is, the concept that a nuclear reaction occurs

STROLLING THINKERS, Fermi (left) and Bohr, are well known for their important applications and expansions of early ideas of nuclear fission.

in two well separated stages: First, the particle arrives in the nucleus and imparts an excitation; then in some way the nucleus uses that energy for radiation, neutron or alpha-particle emission or any other competing process.

Bohr brings the news

The message that Frisch gave Bohr as Bohr left Copenhagen opened up a new domain of application for this concept of the compound nucleus. By the time Bohr had arrived in New York he had already recognized that fission is one more process in competition with neutron reëmission and gamma-ray emission. Four days after his arrival he and Rosenfeld finished a paper summarizing this general picture of fission in terms of formation and breakup of the compound nucleus.

Rosenfeld had originally accompanied Bohr to Princeton for several months of work on the problem of measurement in quantum electrodynamics. During Rosenfeld's Princeton sojourn Bohr gave less than half a dozen lectures on that issue. Nevertheless, that and many other questions conspired to take much of his time. No one could go into his office without seeing the long list of duties and people he had to give time to. That list made it easy to appreciate the pleasure with which he came into my office to discuss the work that we had under way. We were trying to understand in detail the mechanism of fission and, not least, analyze the barrier against fission and the considerations that determine its height.

First of all, of course, we had to formulate the very idea of a threshold or barrier. How can there even be any barrier 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 out of the theory of the calculus of variations in the large, maxima and minima, and critical points. This subject we absorbed by osmosis from our environment, so thoroughly charged over the years by the ideas and results of Marston Morse. 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 activation energy for fission.

While we were estimating barrier heights and the energy release in various modes of fission, the time came for the fifth annual theoretical physics conference held in Washington on 26 Jan. Bohr felt a responsibility toward Frisch and Meitner and thought that word of their work-in-progress and their concepts should not be released until they had the proper opportunity to publish, as is the custom throughout science. Even though this was the situation, at the outset Rosenfeld did not appreciate all the complications and demands of Bohr's position. On the day of Bohr's arrival in the US Rosenfeld went down to Princeton on the train. (Bohr had an appointment later that day in New York.) Rosenfeld reported the new discovery at the journal club—the regular Monday night journal club—and of course everybody was very excited. Isidor I. Rabi, who was at the journal club, carried the news back to Columbia, where John Dunning started to plan an experiment.

Nevertheless, even on 26 Jan., Bohr was reluctant to speak about Frisch's and Meitner's findings until he received word that they had actually been published. Fortunately that afternoon an issue of *Die Naturwissenschaften*, which contained work by Hahn and Fritz Strassmann, was handed to him; thus he could tell about it. Of course everybody started his experiments. The first direct physical proof that fission takes place appeared in the newspapers of the twenty-ninth.

Shaping the theory

The analysis of fission led to the theory of a liquid drop and this in turn led back to a favorite love of Bohr, who, for his first student research work, experimented on the instability of a jet of water against breakup into smaller drops. He was quite familiar with the work of John W. Strutt, the third Lord 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 the third-order terms to see the turning down of the potential. They 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.

Here 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 the power-series development mentioned above one 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.

ROSENFELD, with Bohr, summarized the idea of fission.

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 28 years since that time many workers have done an enormous amount of computation on the topography of the deformation energy as depicted over configuration space as a "base" for the topographic plot. We are still far from completing the analysis. Beautiful work by Wladyslaw J. Swiatecki and his collaborators at Berkeley has taught us much more than we ever knew before about the structure of this fission barrier and has revealed many unsuspected features for values of x that are remote from the two simple, original limits.

From fission barrier we turned to fission rate. All of us have always recognized that nuclear physics consists of two parts: (a) the energy of a process and (b) the rate at which the process will go on. The compound-nucleus model told us that the rate should be measured by the partial width of the nuclear state in question for breakup by the specified process.

Toward a simpler theory

How could we estimate this width? Happily, in earlier days, several persons in the Princeton community—among them Henry Eyring and Eugene Wigner—had been occupied by the theory of the rates of chemical reactions. Also we derived some useful information from cosmic-ray physics. Who does not recall the many detailed calculations Størmer and his associates made on the orbits of cosmic-ray particles in the earth's magnetic field? Fortunately Manuel Sandoval Vallarta and later workers were able to spare themselves almost all of these details. They had only to employ Liouville's theorem. It said that the density of systems in phase space remains constant in time.

The same considerations of phase space were equally useful for evaluating the rate of fission. It turned out that we could express the probability of going over the barrier as the ratio of two numbers. One of these numbers is related to the amount of phase space available in the transition-state configuration as the nucleus goes over the top of the barrier. We were forced to think of all the degrees of freedom of the nucleus other than the particular one leading to fission. All these other degrees of freedom are summarized in effect in the internal excitations of the nucleus as it passes over the fission barrier. In classical terms this concept is well defined. It is a volume in phase space completely determined by the amount of energy.

The other quantity, appearing in the denominator of the rate-of-fission expression, is linked with the volume of phase space accessible to the compound system. In all the complex motion short of actual passage over the barrier the ensemble of systems under consideration remains confined to the narrow band of energies, ΔE, defined by the energy of the incident neutron. What counts is this energy interval multiplied with the rate of change of volume in phase space with energy for the undissociated nucleus. The beauty of this derivation is the fact that these classical ideas lend themselves to direct transcription into quantum-mechanical terms. Thus the Wentzel-Kramers-Brillouin approximation taught us that volume in phase space determines the number of energy levels. So we concluded that the width—the desired width measuring the probability for fission—is given by a ratio in which the numerator is the number of states accessible to the transition-state nucleus as it is going over the barrier, that is, the number of

PLACZEK was helpful in formulating theories of fission.

states of excitation other than motion in the direction of fission. In the denominator appears the spacing between nuclear energy levels, divided by 2π. Thus we had attained the most direct tie with experimentally interesting quantities. The formula that was obtained in this way for the reaction rate, or the level width, applied to a wide class of reactions as well as to fission, and was more general than any that had previously been available in reaction-rate theory. The new formula gave considerable insight into the rate of passage over the fission barrier.

At this particular point it is interesting to note the caution with which Bohr adopted the formula. He would come in every other day or so, and we would go at it for perhaps a half a day, trying out first this approach and then that approach. But his supreme caution was most evident when we wanted to interpret the number of levels accessible in the transition state. Today that number is called "the number of channels," and we use it as a formula to describe the channel-analysis theory of fission rate. Also we apply similar channel-analysis considerations to other nuclear reactions. But at that time the idea that each one of these individual channels has in principle a definite experimentally observable significance was, for us, of dubious certainty. Still less did we appreciate, until the later work of Aage Bohr, the possibility that each individual channel would have its individual angular distribution from which one could determine the K values of that channel. The cautious phrase that was used in reference to that channel number appears in the following quotation: "It should be remarked that the specific quantum-mechanical effects which set in at and below the critical fission energy may even show their influence to a certain extent above this energy and produce slight oscillations in the beginning of the yield curve, allowing, possibly, a direct determination of the number of channels." Of course we know how later on in the 1950's these variations were observed by Lamphere and Green and others and how they led to direct measurement of the channel number.

Bohr's epiphany

The most important part of this Princeton period happened when I was not in direct touch with Bohr. One snowy morning he was walking from the Nassau Club to his office in Fine Hall. As a consequence of a breakfast discussion with George Placzek, who was deeply skeptical of these fission ideas, Bohr began struggling with the problem of explaining the remarkable dependence of fission cross section on neutron energy. In the course of the walk he concluded that slow-neutron fission is caused by U^{235} and fast-neutron fission by U^{238}. By the time he had arrived at Fine Hall and he and I had gathered together with Placzek and Rosenfeld, he was ready to sketch out the whole idea on the blackboard. There he displayed the concept that U^{238} is not susceptible to division by neutrons of thermal energy, nor is it susceptible to neutrons of intermediate energy but only to neutrons with energies of a million electron volts or more. Further, the fission observed at lower energies occurs because U^{235} is present and has a $1/v$ cross section for capture. We already knew experimentally that neutrons of intermediate energy undergo resonance capture. And, with the help of simple considerations, we could show that the resonance reaction of neutrons with uranium could not be due to U^{235}. We concluded this because we knew that the resonance cross section would exceed the theoretical limit given by the square of the wavelength if U^{235} were responsible for the resonance effect. So the resonance had to be due to U^{238}, and the very fact that the resonance neutrons did not bring about fission proved that U^{238} was not susceptible to fission by neutrons of such low energy. Thus if it was not susceptible at that energy, it would certainly not be susceptible at lower energies; consequently low-energy fission must be due to U^{235}.

A few days later, on 16 April, Placzek, Wigner, Rosenfeld, Bohr, myself and others discussed whether one could ever hope to make a nuclear explosive. It was so preposterous then to think of separating U^{235} that I cannot forget the words that Bohr used in speaking about it: "It would take the entire efforts of a country to make a bomb." He did not foresee that, in truth, the efforts of thousands of workers drawn from three countries would be needed to achieve that goal.

The theory of fission made it possible to predict in general terms how the cross section for fission would depend upon energy. In Palmer Physical Laboratory Rudolf Ladenberg, James Kanner, Heinz H. Barschall and Van Voorhies, just at the time we were working on the theory, actually measured the cross section of uranium in the region from two million to three million volts—and also the cross section for thorium, all of which fitted in with predictions. The same considerations of course made it possible to predict that plutonium 239 would be fissile. For this application of the theory we are especially indebted to Louis A. Turner. One started on the way that ultimately led to the giant plutonium project having only this theoretical estimate to light and encourage the first steps.

Spontaneous fission offered a most attractive application of these ideas in conjunction with the concept of barrier penetration. Another application dealt with the difference between prompt neutrons and delayed neutrons. In conclusion, nuclear fission brought us a process distinguished from all the other processes with which we ever dealt before in nuclear physics, in that we have for the first time in fission a nuclear transformation inescapably *collective* in character. In this sense fission opened the door to the development of the collective model of the nucleus in the postwar years. □