

THE DISCOVERY OF NUCLEAR FISSION

Fermi's group bombarded uranium with neutrons in 1934, but it was almost five years before Hahn and Strassmann realized what these neutrons were actually doing. It required superb chemists to bring the comedy of errors to a close.

Emilio G. Segrè

Few modern discoveries have influenced mankind so rapidly and so profoundly as has nuclear fission, and few have had such an intricate history. Thus it is natural that the discovery of fission by Otto Hahn and Fritz Strassmann in December 1938 is remembered and commemorated in many places on its 50th anniversary. I participated in the early experiments in Rome, and later in the US, and I knew most of the principals well, except for Strassmann. I will try to present a brief outline of the discovery and its antecedents.

Transmutation by neutrons

I begin the story in 1934 with the first neutron bombardment of uranium.¹ Following the discovery of artificial radioactivity by Irène Curie and her husband Frédéric Joliot in Paris at the beginning of 1934, Enrico Fermi in Rome had started using neutrons from radon-beryllium sources, in lieu of alpha particles, to activate many common elements. Between March and April, with the help of Edoardo Amaldi, Oscar D'Agostino, Franco Rasetti and myself, Fermi had established² the reactions (n,p) , (n,α) and (n,γ) or $(n,2n)$. (The notation (A,B) means that a nucleus has been transformed by incident particle A, with the emission of particle B.) Our sources emitted about 10^7 neutrons per second.

Bombarding uranium with neutrons presented an especially interesting case, because we could expect to form element 93, the first transuranic element, by an (n,γ) reaction followed by subsequent beta decay. Indeed this does happen—but much more was in store. I remember that Rasetti was particularly eager to bombard uranium and thorium. The first communication on our results was dated 10 May 1934, about two months after the first neutron bombardment. For the sake of brevity I will omit most of the work on Th, which paralleled and often supplemented that on U.

The radioactivity produced by our neutron sources was not much greater than the natural radioactivity of uranium off the shelf. This caused severe technical problems. We could chemically remove some of the uranium's beta-active decay products before irradiating it. But they grew anew after just a few hours. They formed a large, confusing background of counts that had nothing to do with the neutron irradiation. The same trouble

Emilio Segrè was emeritus professor of physics at the University of California, Berkeley. He died suddenly on 22 April at the age of 84, three months after he presented the talk on which this article is based at the January meeting of The American Physical Society in San Francisco.



Enrico Fermi's group in Rome, 1934. Left to right are Oscar D'Agostino, Emilio Segrè, Edoardo Amaldi, Franco Rasetti and Fermi.

affected all the European investigators and played a part in the errors we all made.

In Rome we immediately found that irradiated uranium showed a complex radioactivity with a mixture of several decay periods. We expected to find in U only the previously observed backgrounds, and so we started looking for an isotope of element 93 produced by an (n, γ) reaction on U^{238} followed by beta decay.³

A mistake in the chemistry

Here we made a mistake that may seem strange today. We anticipated that element 93 would resemble rhenium (element 75)—that it would be, in the language of Mendeleev, an eka-rhenium. Products of the bombardment are indeed similar to Re, but for a totally different and then unimagined reason: Some of the abundant fission products are isotopes of technetium, element 43. This element (so named because it was the first of the artificially produced elements) was discovered by Carlo Perrier and me three years later, in 1937, in molybdenum (element 42) bombarded with deuterons. It proved to be chemically very similar to Re. Thus it actually resembled what we erroneously expected in the 1934 uranium experiment.

Hahn and Lise Meitner, and Irène Curie, made the same error, presuming that 93 was an eka-Re. (Element 93, like all the now known transuranics, has a chemistry akin to that of the rare earths.) Even stranger is the fact that Niels Bohr did not object. Ten years earlier he had considered the filling of the 5f electron orbits and the formation of a new family of rare earths, although he did

not start it at exactly the right place. On the other hand, Aristid von Grosse at the University of Chicago pointed out in 1934 the possible analogy of element 93 to a rare earth.

Aluminum foil hides the prize

In Rome we also considered the possibility of the formation of short-lived alpha emitters in neutron-bombarded U. To test this hypothesis, we placed a uranium foil in front of an ionization chamber and irradiated it with slow neutrons. We thought that if alpha particles came from a short-lived substance created by the neutron bombardment, they would have a significantly longer range than the uranium background alphas. We therefore covered the uranium sample with a thin aluminum foil that would stop the U alpha particles. The results were negative, and the aluminum layer prevented us from seeing the big ionization pulses produced by the fission fragments! We did not publish this result, but it is in Amaldi's notebooks of the period.⁵ I cannot say, however, that if we had seen the big pulses we would have understood their cause. A similar experiment was performed by Paul Scherrer and coworkers in Zurich and by Gottfried von Droste in Berlin. I've been told that the Swiss saw the big pulses but attributed them to a fault in the detector.

Another error was in not paying enough attention to a 1934 article by Ida Noddack in Berlin,⁶ who criticized our chemistry and pointed to the possibility of fission. Much has been said of her prescience. Her article was certainly known to us in Rome, to Hahn and Meitner in Berlin and to Joliot and Curie in Paris. If any of us had really grasped its importance, it would have been easy to discover fission

in 1935. It is equally astounding that Noddack did not try any experiment herself to check her ideas. It would have been quite easy for her too. Be that as it may, for more than three years all the investigators in the field considered only nuclear reactions that would lead to elements with atomic numbers near 92.

In Rome, we started by convincing ourselves that the radioactivity observed in bombarded uranium was not due to isotopes of elements between Pb and U, which turned out to be correct. We tried to establish some properties of substances extracted from the radioactive complex, and we found an ingredient that we thought behaved like an eka-Re. Its chief reaction was a precipitation with MnO_2 . A few years ago, Franco Baroncelli in Italy repeated our old procedure and found that he could separate some isotopes of technetium (unknown in 1934) that simulate our results. Of course Tc behaves very much like Re, and it is a fission product! We should, however, have been suspicious of many things, and especially of the fact that our supposed element 93 could account for only a small fraction of the activity generated by the neutron bombardment.

Spurious transuranics

By June of 1934, the end of the academic year, we were confident enough of our imagined success at forming transuranic elements to publish this result. We also had a feeling, however, that the work was incomplete. We therefore refrained from naming the transuranic elements we thought we had found. Fermi was particularly upset when the press gave this work a lot of publicity.⁷

After the 1934 summer vacation, Bruno Pontecorvo joined our group. In October we suddenly discovered how to produce slow neutrons. This was a major find, and our group, depleted of its chemist by the departure of D'Agostino, suspended the work on transuranics and concentrated on studying slow neutrons. We did, however, look at the effect of slow neutrons on the U activity, to find out whether it involved neutron capture, and also to learn more about the substances produced in neutron bombardment. The work on transuranics in Rome stopped in the summer of 1935. At the end of the year Rasetti came to the US, Pontecorvo went to Paris and I became director of the Physics Institute in Palermo. Amaldi and Fermi, in Rome, concentrated their efforts on developing slow-neutron physics.

At this point Hahn and Meitner,⁸ and Irène Curie,⁹ entered the uranium fray. Both groups had a past history of great achievements in nuclear physics and chemistry.

In Berlin, Hahn and Meitner were the senior members of the group at the Kaiser Wilhelm Institute in Dahlem. Hahn was a renowned radiochemist; he had worked with Ernest Rutherford at Montreal 30 years earlier. At the beginning of his career he had discovered several new radioactive substances. Later, together with Meitner, he discovered the phenomenon of nuclear isomerism.

Meitner, an Austrian citizen born in Vienna, had been an assistant to Max Planck in Berlin; later she became the steady colleague of Hahn and worked in the same lab with him. She was a distinguished physicist and something of a rival to the Curies, both Marie and Irène. Meitner was of Jewish descent; Hahn was strongly anti-Nazi. The political situation made working conditions at Dahlem difficult and caused continuous anxiety.¹⁰ Strassmann was the last to join the Dahlem group. He was a superior analytical chemist, and also resolutely anti-Nazi, a fact that hampered his career.

Irène Curie was the daughter of Marie, the discoverer



Frédéric Joliot and Irène Curie in their Paris laboratory, around 1934.

of radium, and she had learned radiochemistry and chemistry from her great mother. She was steeped in Marie's tradition, methods and techniques. She had married Joliot, and together they had performed important experiments on the positron and the neutron, though they had missed discovering these new particles. This was, however, soon compensated for by their memorable discovery of artificial radioactivity.

Berlin

Hahn and Meitner, working with a neutron source about as strong as the ones used in Rome and Paris, started by confirming our Rome results. This is somewhat surprising because they applied quite different chemistry. Their early papers are a mixture of error and truth as complicated as the mixture of fission products resulting from the bombardments. Such confusion was to remain for a long time a characteristic of much of the work on uranium. A firm and important result obtained by Hahn, Meitner and Strassmann was the identification of U^{239} as a beta emitter with a half-life of 23 minutes. For the rest, there is not much reason to follow the details. Their numerous papers, published mostly in *Naturwissenschaften*, record the ups and downs of these investigations that preceded the realization that fission was happening. At first the authors are Hahn and Meitner. Strassmann appears initially as collaborator and then, from July 1938, as a full-fledged coauthor. In 1937 the three published a longer summarizing paper in *Zeitschrift für Physik*¹¹ and a parallel paper in *Chemische Berichte*.¹² They mention 12 new radioactive isotopes attributed to elements of atomic number between 92 and 95, and a

series of double isomeric states. (In fact, no transuranic element was properly discovered before 1940.) The supposed double isomers were particularly surprising. Double isomeric states were unknown then, and even today we know of less than a handful.

My own feeling at the time was that there was a mystery in uranium. In Palermo I could not work on it because I had no neutron sources. In the summer of 1936, when I visited Berkeley for the first time (and gratefully obtained radioactive material from Ernest Lawrence), I spoke at length with Philip Abelson, who was a student at that time, and pointed out to him the great uranium puzzle. I emphasized that the powerful neutron source offered by the cyclotron would make it easier to solve. A start could be made by irradiating U with fast or slow neutrons. Abelson made a few runs and gave me some of the resulting decay curves.

Paris

Irène Curie started working on neutron-bombarded thorium in collaboration with Hans von Halban and Preiswerk. As early as May 1935 they confirmed a 22-minute decay period we had found in Rome. This Th isotope is important as a precursor of U^{233} . More important, they soon found a 3.5-hour radioactive component, which chemically resembled lanthanum.¹³ They did not realize, of course, that it was indeed La^{141} , a fission product! Unable to pin it down, Curie and her colleagues thought it an isotope of actinium.

In 1937 and 1938 Curie and Pavel Savitch concentrated their combined efforts on the study of the 3.5-hour substance. By July 1938 they arrived at the conclusion that the substance was not actinium, and that "all in all, the properties of [the 3.5-hour component] are those of La, from which up to now it can be separated only by fractional crystallization."¹⁴ Had they been able to identify it as La, they would likely have discovered fission, as Hahn and Strassmann did a few months later by nailing down barium. Possibly their initial precipitate contained not only La, but also the chemically similar yttrium, also produced by fission, and it was these two substances that the fractional crystallization was separating.

In mid-May 1938, Hahn and Joliot met in Rome at the 10th International Chemistry Conference and discussed the Paris results. Hahn was convinced that there was something wrong in Curie's chemistry, and he decided to repeat some of her experiments.

Darkness and dawn

Two months earlier Hitler had annexed Austria to the Reich, and thus Meitner had lost the relative protection of her Austrian citizenship. She was now in imminent danger of arrest. In mid-July she fled the country in secrecy and haste, helped by Hahn and the Dutch physicist Dirk Coster. Hahn was greatly relieved when he heard that she had safely crossed into Holland. From there she proceeded to Copenhagen and then to Sweden. Hahn and Strassmann continued their work at Dahlem, and Hahn kept Meitner posted on their progress by frequent letters. He also made a point of disclosing to her the results he and Strassmann were obtaining, before publishing them or even mentioning them to anybody else, including his Berlin colleagues.

Hahn and Strassmann concentrated on what they thought were isotopes of radium and on the 3.5-hour product described by Curie and Savitch. They concluded that from the neutron bombardment of U^{238} they could

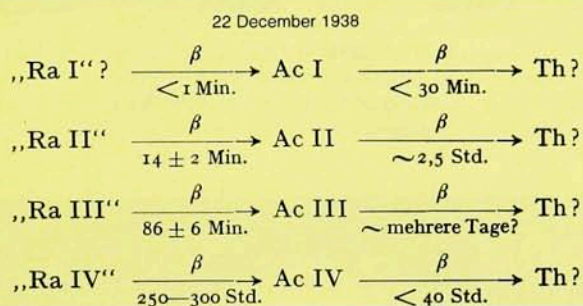
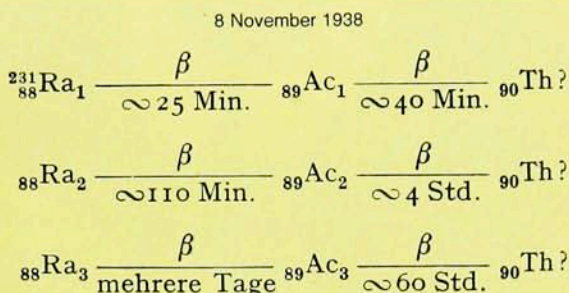
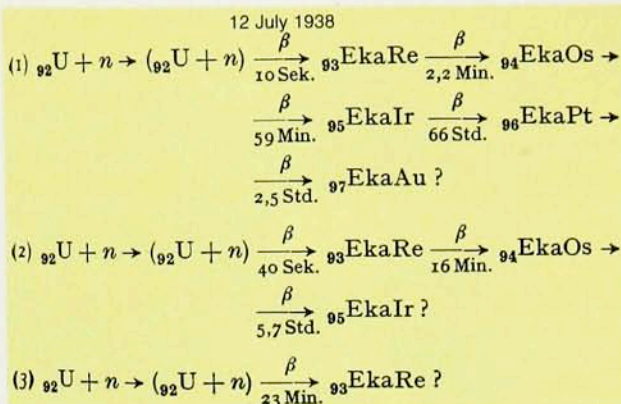


Lise Meitner in 1937, a year before she had to flee Berlin, where she had been collaborating with Otto Hahn for three decades.



Otto Hahn in Göttingen after World War II.

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obtain 16 nuclear species with atomic numbers ranging from 88 to 96, including a number of isomers.¹⁵ The confusion was approaching its maximum. It was, however, the darkness before dawn, because the solution was not far off.

Early in December 1938 they thought they had established some decay chains in which the genetic relations appeared to be solidly known. Supposedly four isotopes of radium were decaying to Ac and then to Th. These putative chains were modifications of chains given in a previous paper (see the figure above). To make doubly sure, Hahn and Strassmann decided to identify the radium isotopes beyond any doubt. They submitted them to several stringent chemical tests using barium as a carrier and radium as a tracer. These superb experiments forced Hahn and Strassmann to conclude reluctantly that the hypothetical radium isotopes were in fact barium! In their historic 22 December paper¹⁶ for *Naturwissenschaften* they wrote: “As chemists, in consequence of the experiments just described, we should change the schema given above and introduce the symbols of Ba, La, Ce in place of Ra, Ac, Th. As ‘nuclear chemists,’ working very close to the field of physics, we cannot yet bring ourselves to take such a drastic step, which goes against all previous experiences of nuclear physics.” A few lines earlier, however, the authors had noted “that the sum of the mass numbers of Ba + Ma [technetium], for instance 138 + 101,

Some of the last steps in the discovery of fission, reproduced from 1938 papers of Hahn and coworkers.

Top: In reference 15, from July 1938, Hahn, Meitner and Strassmann were still assuming that bombarding uranium with neutrons produced mostly transuranics, labelled “eka-gold,” “eka-platinum” and so on, after the lighter elements they were presumed to resemble chemically. Hahn and his colleagues were forced to invoke three different imagined isomers of “eka-rhenium.” Reaction 3 turns out to be correct, except that element 93 (now called neptunium) does not resemble rhenium at all. **Middle:** From Hahn and Strassmann, *Naturwissenschaften* 26, 755 (1938), dated 8 November. Much of the activity observed after neutron bombardment was attributed to supposed isotopes of radium. **Bottom:** Finally, in reference 16, dated 22 December, Hahn and Strassmann have recognized fission. They have chemically identified “Ra” (now in quotes) as *barium*, which could only be a fission product.

gives 239”—a clear sign that they were thinking of fission. This is the moment of the discovery of nuclear fission.

‘What idiots we all have been!’

Hahn sent letters with these results, prior to their publication, to Meitner in Sweden. She showed them to her physicist nephew Otto Frisch, who was visiting from Copenhagen over the Christmas holidays.^{10,17} Frisch and Meitner soon arrived at the idea of fission. A few days later Frisch returned to his lab in Copenhagen. In his words: “I was keen to submit our speculations—it wasn’t really more at the time—to Bohr, who was just about to leave for the USA. He had only a few minutes for me, but I had hardly begun to tell him when he smote his forehead with his hand and exclaimed: ‘Oh what idiots we all have been! Oh but this is wonderful! This is just as it must be! Have you and Lise Meitner written a paper about it?’”¹⁷ Two weeks later their paper was received by *Nature*.¹⁸

The discovery of fission started a flood of investigations. The first and most obvious was the verification of the presence of fission fragments. One could calculate from the mass defects, or from the Coulomb repulsion of the fission fragments, that they had to be nuclei with kinetic energies of about 200 MeV; they would therefore be heavily ionizing. Such fragments were promptly observed by Frisch¹⁹ and almost simultaneously by many others.

The chemical identification of the substances produced by neutron bombardment now took on a new aspect. Irène Curie, in 1938, had said, “It seems that uranium bombarded with neutrons gives an activity composed of almost every element.” She was right, and now many rushed to disentangle the fission products.

While fission was being discovered, Fermi was in Stockholm collecting the Nobel Prize for “his demonstration of the existence of new radioactive elements produced by neutron irradiation, and for his related discovery of nuclear reactions brought about by slow neutrons.” This citation has been variously interpreted, as far as the words “new radioactive elements” are concerned. If the word “isotopes” had been used instead of “elements,” it would be clearer. Fermi emigrated directly from Sweden to the US, where he first heard the news about fission.

The discovery of fission was a sensation. The reaction in America can be seen by the spate of papers on fission that immediately appeared in the *Physical Review*. Luis Alvarez has vividly described the reaction at Berkeley.²⁰

Verifying the transuranics

The transuranics still had some surprises in store. Joliot, trying to demonstrate fission, exposed a thin layer of

Fritz Strassmann (left) with Meitner and Hahn in Mainz, 1956.



uranium to neutrons and put next to it a sheet of bakelite in which he collected the fission fragments.²¹ Edwin McMillan, independently, did the same experiment at Berkeley.²² They both found a nonrecoiling activity remaining in the U layer. There was a 23-minute activity due to U^{239} formed by an (n,γ) reaction on U^{238} , already demonstrated by Hahn and Meitner. But there was an additional two-day activity.

Having come to Lawrence's Radiation Laboratory in 1938, I took advantage of the powers of the cyclotron as a neutron source to investigate this two-day component. I studied its chemical properties and concluded that they were those of a rare earth. I had expected that the two-day activity was due to a beta-decay product of U with atomic number 93 (as indeed it is), but I was expecting element 93 to have the chemistry of an eka-Re! My attempt to find a genetic relationship between the 23-minute and two-day activities failed because the beta rays from element 93 are uncommonly soft and led me astray. Thus another erroneous paper was added to the long list of blunders produced by irradiated uranium. Like many of the other blunders, however, it had some elements of truth. It showed that element 93 is similar to the rare earths.²³

Finally, a few months later, McMillan and Abelson²⁴ chemically separated and recognized element 93 (neptunium), the daughter of U^{239} .

In the meantime Joliot, Fermi and many others had noted that the two fission fragments were particularly rich in neutrons. Most of the excess neutrons transformed into protons by beta decay, but it was conceivable that some neutrons were being set free. This opened the possibility of a chain reaction.

It was the beginning of 1939; war was threatening in Europe. Nuclear fission was becoming more than a scientific curiosity. I will not go into the subsequent story. It has been told many times.

The discovery of fission has an uncommonly complicated history; many errors beset it. Nature had, however, truly complicated the problem. One had to contend with the radioactivity of natural uranium and the presence of two long-lived isotopes— U^{235} and U^{238} . The heavier isotope, as is well known, does not undergo fission when bombarded by slow neutrons. The lighter isotope, which makes up only 0.7% of natural uranium, is responsible for all slow-neutron fission. This is a tricky setup. Above all, it seems to me that the human mind sees only what it expects.

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