RIGHT AND WRONG ROADS TO THE DISCOVERY OF NUCLEAR ENERGY

by
Lise Meitner

Twenty years ago, on 2 December 1942, Enrico Fermi succeeded in making the world's first reactor "critical", i. e. in bringing it into operation. It was no accident that Fermi was the first man to solve what was then an extremely complicated problem, although a simple one in principle. In both the experimental and theoretical fields, he was one of the most gifted physicists of our time, always ready and able to approach new and difficult problems with the simplest of conceptions and, if the available facilities were not adequate, to develop or devise experimental methods (again in the simplest manner) with an amazing power of analysis of the task in hand.

The basis for Fermi's achievement in constructing the first reactor was of course the discovery, by Otto Hahn and Fritz Strassmann, of uranium fission through neutron bombardment of ordinary uranium. Viewed in the light of our present knowledge, the road to that discovery was astonishingly long and to a certain extent the wrong one, yet here also, in following this devious path which led at last to the true explanation of events, Fermi was the pioneer.

Very soon after the discovery of the neutron by Chadwick and of artificial radioactivity by I. Curie and F. Joliot, Fermi recognized how suitable neutrons must be, due to the absence of an electric charge, for penetrating heavier, i. e. highly-charged, atomic nuclei and bringing about reactions in them. He and a group of young collaborators, some of whom were trained by him, bombarded every element they could with neutrons and thereby obtained a series of new radioactive isotopes, including representatives of the heavier elements. The most interesting results seemed to accrue from bombarding the then heaviest element, uranium: Fermi thought that this led to higher elements with atomic numbers 93 and 94, i. e. to transuranic elements.

I found these experiments so fascinating that, immediately after the reports on them appeared in Nuovo Cimento and Nature, I persuaded Otto Hahn to renew our direct collaboration, which had been interrupted for several years, with a view to investigating these problems.

So it was that in 1934, after an interval of more than 12 years, we started working again together, with the especially valuable collaboration, after a short time, of Fritz Strassmann.

Lise Meitner (Photo USIS)

We were of course not entirely uninfluenced by Fermi's assumption that in the case of uranium only higher elements were being formed, and the behaviour of thorium strengthened our confidence in this assumption: when we bombarded thorium-232 with decelerated neutrons, we found not only β-emitting thorium-233 of 26 minutes' half-life, which had already been observed by Fermi, but also unmistakably a β-emitting protactinium-233 with a half-life of about 25 days, whose correct chemical identification we had no reason to doubt. Nevertheless, I found it very disturbing to discover, with uranium, such a long chain of successive β-disintegrations, i. e. continually increasing nuclear charges with unchanged masses.

One outcome of my concern was our precise examination of uranium under slow neutron bombardment. We were able to demonstrate chemically, beyond all doubt, the formation of β-emitting uranium-239 with a half-life of about 23 minutes. We found this to be a resonance process with an energy of 25 $\pm$ 10 V. Proof of β-emission constituted proof of formation of element 93 - which we called ekarhenium and which was later named neptunium - but our preparations were far too weak to permit investigation of such things as its chemical properties or half-life. Our great difficulty lay in the fact that in this attempt we had to examine the entire quantity of bombarded uranium, from which uranium X had previously been meticulously removed, while the re-formation of uranium X was very rapidly covering the activity of the 23-minute uranium-239.

Our precipitations after fast neutron bombardment were always carried out in such a manner as to ensure that U, Pa and Th remained in the filtrate, as a result of which we believed that we were obtaining some confirmation of the transuranic nature of the precipitated elements. For this reason - and here was our mistake - we at first never examined the filtrates of our precipitations, even in experiments with slow neutrons. We did this only after Curie and Savitch declared in their first report on the subject that they had found a new thorium isotope in the course of their experiments. Unfortunately, we repeated the experiments of the French workers only to the extent that we looked for a thorium isotope in our filtrate; we were definitely able to establish that there was none.

We wrote to Irene Curie about our negative results, and a note to the next report published by Curie and Savitch, in which appeared a description of their remarkable 3.5 hour product, contained confirmation of our findings. The French workers deduced from their results, although with considerable hesitation, that the 3.5 hour product was a transuranic element which, however, to some extent behaved very much like the rare earth element lanthanum. We know today that this 3.5 hour product was apparently a mixture of barium and lanthanum. It may be interesting to mention that I learnt from von Hevesy that Irene Curie once told him in 1938 that she sometimes thought she had all the chemical elements in her bombarded uranium.

By the time the work on the 3.5 hour product was published, I had left Germany (in July 1938) and after a short stay in Holland had moved to Stockholm, where work premises in the new Institute were put at my disposal by Manne Siegbahn.

Hahn and Strassmann, who rightly regarded the French results as significant and inviting confirmation, repeated the experiments in order to obtain the 3.5 hour product and identify it chemically. Their careful experiments led to the conclusion that it was not a chemically homogeneous substance, but a mixture of β-active radium isotopes and the likewise β-emitting actinium isotopes resulting therefrom.

Separation of the radium isotopes was accomplished by precipitation of added barium. However, when Hahn and Strassmann then attempted to separate these "radium" isotopes from the barium carrier, they found, to their great astonishment, that this was impossible, although the known radium isotopes thorium X and mesothorium I could be separated from barium by the same methods, even, as they could see for themselves, in the minutest quantities. There could only be one conclusion: the "radium" isotopes were in fact barium isotopes. I should like to stress that, in view of the extremely low intensity of the preparation to be identified, the establishment of this proof was indeed a masterpiece of radiochemistry, which at that time could hardly have been achieved by any persons other than Hahn and Strassmann.

Hahn wrote to me at Christmas 1938 describing the results of their latest experiments, which had astonished both Strassmann and himself. I was at that time at Kungälv on the west coast of Sweden, spending a few days' Christmas holiday with O. R. Frisch, who had come over from Copenhagen. Quite naturally Hahn's letter betrayed great excitement, and in it he asked me what I, as a physicist, thought of the results. On reading the letter I myself was thoroughly excited and amazed, and also - to tell the truth - uneasy. I knew the extraordinary chemical knowledge and ability of Hahn and Strassmann too well to doubt for one second the correctness of their unexpected results. These results, I realized, had opened up an entirely new scientific path - and I also realized how far we had gone astray in our earlier work!

When I tried to tell Frisch about this vital news, I first had to lead the conversation away from discussion of his plans for a large magnet, which he was intent on describing to me. Finally, however, we both became absorbed in my problem and were convinced that we were faced with a completely different process than the splitting-off of a nucleon or α-particle.

The new process gradually became comprehensible in the light of Bohr's liquid-drop nuclear model, according to which the surface tension stabilizes the nucleus vis-à-vis small deformations. In the course of our discussion we evolved the following picture: if, in the highly-charged uranium nucleus - in which the surface tension is greatly reduced owing to the mutual repulsion of the protons - the collective motion of the nucleus is rendered violent enough by the captured neutron, the nucleus may become drawn out lengthwise, forming a sort of "waist" and finally splitting into two more or less equal-sized, lighter nuclei which, because of their mutual repulsion, then fly apart with great force. Using this image we were also able to estimate the liberated energy at about 200 MeV. In view of the similarity of this process to cell division, we called it (at Frisch's suggestion) "fission" and stressed its novelty by using in the title of our report the phrase "A New Type of Nuclear Reaction".

This report appeared under somewhat unusual conditions, viz. as a result of telephone conversations. Frisch had returned to Copenhagen and I myself to Stockholm before we could decide on the final terms of our communication. We also agreed by telephone on the demonstrability of the great energy released in the fission process, either by measurement of the ionization produced by the high-energy fission particles - proposed by Frisch and then forthwith carried out by him - or by using my suggestion of collecting the fission products through their recoil, as was done shortly afterwards by Joliot.

On 16 January 1939 we sent two letters to Nature, containing our explanation of the fission process and Frisch's experimental proof of the great energy of the lighter atoms formed hereby. As we did not ask for rapid publication, these only appeared on 11 and 18 February respectively.

Meanwhile, several unexpected things had happened. Bohr had gone to America and on 26 January had reported to the American Physical Society in Washington on Hahn and Strassmann's work, which had been published in the meantime, and on our explanation for the process, which Frisch had communicated to Bohr after his return from Kungälv. (It may be mentioned that Bohr immediately expressed surprise that the theorists had not foreseen the process.) Some American experimenters left the meeting immediately, even before Bohr had finished speaking, in order to go and confirm the ionization energy of the fission products to be expected from our picture, and they immediately published their findings in a daily newspaper, even before Bohr knew that this confirmation had already been obtained by Frisch. Bohr only learned this later in a letter from his son and then, in discussion with American journalists, energetically maintained that Frisch should be given credit for having been the first to establish proof. Apparently in the course of this exchange the startling assertion was made that Frisch was Bohr's son-in-law - startling if only because Bohr never had a daughter and Frisch at that time was unmarried.

The rest of the story is well known.

I should not like to end this account without stating how much I hoped that the newly-discovered source of energy would be used only for peaceful purposes. During the war, I used to say to my Stockholm friend, Oskar Klein: "I hope they will not succeed in making an atomic bomb, but I fear they will."

My fears were justified, and look at the state of the world today! However, I still have hopes that the Pugwash conference at present being held in Cambridge and similar efforts will finally lead to a solution of the highly complex problems at issue, hopes which Fermi would certainly have shared.