*THE DISCOVERY OF SLOW NEUTRONS: SOME RECOLLECTIONS**

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After having graduated from the University of Rome I was appointed assistent of the department of physics. The subject of my studies was selected by Fermi and Segre. It was a work in classical spectroscopy. In 1934 practically nobody at the Institute of Physics had anything to do with spectroscopy; everyone was feverishly busy investigating radioactivity induced by neutrons, and the seminars of the institute were dominated by "nuclear" reports. All these circumstances resulted in my heart being much closer to the neutron studies carried out by Fermi and his colleagues than to my spectroscopic work, which I completed by the summer of 1934. Therefore I was very glad when, upon coming back to Rome from my holidays, I was asked to help in the neutron experiments.

Amaldi and I were to perform quantitative measurements of the relative activities induced by neutrons in various substances. Similar measurements made earlier by Fermi and his colleagues were only qualitative. At the same time (who knows why) the scattering effect was ignored, and only primary neutrons were considered to have any influence; in this case the activity induced in a sample should be inversely proportional to the square distance R from the source, if this distance is much greater than the dimensions of the source and of the sample; but in the case of such distances only a negligible activity is induced in the sample. Thus, our task consisted in choosing a convenient geometry of the experiments for irradiating various elements in comparable conditions at a small distance from the neutron source. But it turned out to be that even in the simple case, when only the activities of a sole sample (a standard silver cylinder) were measured, it was difficult to reproduce the results.

Subsequently it became clear that this was related to the influence of scattering and slowing down of neutrons by the surrounding objects. But at the beginning, on the basis of the implicitly formulated dogma claiming there to be no neutrons "besides the primary neutrons", we could not find any explanation for the irregularities of the induced activity. The first step towards resolving this mystery consisted in measuring the activity of our standard cylinder, when it was at a distance of about 20 cm from the source, and both the cylinder and the source were placed inside a little shed made of

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lead bricks for protecting the experimenters from the radiation. The $1/R^2$ law would have resulted in a negligible activity, which was checked in a special experiment in the absence of the shed. But inside the shed the $1/R^2$ law seemed not to be valid. The activity in the shed at a distance of 20 cm was quite noticeable. Amaldi and I were absolutely sure about the influence of the lead being real, and we termed it the "casteletto" effect, which means the "effect of the lead castle" (later it became clear that we had to do with inelastic scattering of neutrons on the lead). The reaction of Fermi and Rasetti was interesting. Rasetti, being a sceptic, just didn't believe our result. Fermi said the experiment should be continued, but seemed not to be particularly interested. Later we learned that our impression was wrong.

Several days later Fermi himself became involved in resolving the "lead mystery". He proposed measuring the activity induced in the silver cylinder, when a thin lead wedge several centimetres thick was inserted between the cylinder and the source. The wedge was already made, but ... neither Fermi nor anyone else performed measurements with it. Without saying anything to anyone, in the morning of October 22, 1934 Fermi decided to measure the radioactivity of the silver cylinder making the neutrons from the source "pass", instead of the lead wedge, through a paraffin wedge of the same dimensions, which he himself had rapidly made. The result was quite clear: the paraffin "absorber" did not reduce the activity, but definitely (although not significantly) increased it. Fermi summoned us all and said: "This probably occurs because of the hydrogen in the paraffin; if a small amount of paraffin yields a noticeable effect, let us see what a large quantity will do". The experiment was immediately performed, first, with paraffin and, then, with water. The results were staggering: the activity of the silver was hundreds of times greater than the one we had dealt with previously. Fermi stopped the noise and excitement of his collaborators with the famous phrase, which he was said to have repeated eight years later when launching the first reactor: "Let's go and have lunch".

Thus was the Fermi effect (the slowing down of neutrons) discovered, which opened a new chapter of nuclear physics and, also, a new field in engineering, presently termed nuclear engineering.

I have spoked about the discovery of slow neutrons in such detail, because here both chance circumstances and the profoundness and intuition of a great mind happened to be essential. When we asked Fermi why he used a paraffin wedge, instead of a lead one, he smiled and mockingly pronounced : "C. I. F." (Con Intuito Fenomenale).

It would be wrong, if this CIF bravado gave the reader the impression that Fermi was indiscreet. He was an ingenuous, very simple and modest person, but he was just sure of himself. By the way, when he came back to the institute that famous day and gave us such a clear explanation of the effect of paraffin by first introducing the concept of the slowing down of neutrons, he quite sincerely said: "How stupid of us to have discovered the phenomenon by chance and of not having been able to predict it". Fermi immediately guessed that the neutrons, when losing energy in collisions with hydrogen, are slowed down to the energy of thermal motion and that precisely slow neutrons are most effectively capable of inducing radioactivity in our detector. However, he stressed with the scientific prudence characteristic of him that the idea of "thermal" neutrons remained a hypothesis for the time being and that it could be tested only by revealing the influence of the temperature of paraffin or by direct measurement of the neutron velocities. By the way, the first attempt at observing the temperature influence on the induced radioactivity was made by Fermi very soon, but the experiment which consisted in searching for the difference between the activations of the detector corresponding to the cold and the hot paraffin, respectively, gave no positive result, and only after several months were Moon and Tillman in England, as well as Fermi together with his collaborators, successful in observing this phenomenon.

Already that very day, immediately after lunch, was a series of experiments carried out that revealed the effect of paraffin (and of water) to be mainly related to hydrogen and not to other elements and to be caused by neutrons, but not by γ rays from the Rn + Be source.

Moreover, the rule was found in accordance with which the sensitivity to hydrogen-containing substances is exhibited not by all kinds of activity, but only by those that correspond to the production of a radioactive isotope of the target element. This was a very important result, and the connection of the (n, γ) with the hydrogen effect explained a number of strange results observed earlier.

That same night of October 22 we all gathered in the house of Fermi (or Amaldi, I don't quite remember). Here, in a clear telegraphic manner was the first work on the slowing down of neutrons written, which was to become the starting-point of a new series of letters to "Ricerca Scientifica" entitled "The Influence of Hydrogen-Containing Substances on the Radioactivity Induced by Neutrons".

Fermi reported the surprising properties of slow neutrons to the director of the institute, Professor Corbino. Corbino showed lively interest in what Fermi told him and said: "You must really get a patent for your method of obtaining slow neutrons". I can never forget how Fermi laughed sincerely and heartily, just like a child, when Corbino hinted that the results discussed could be of practical importance. Corbino reacted to the general merriment of Fermi and his collaborators quite drily and noted: "You are young and don't understand anything!".

Naturally, Corbino was right: this patent, first obtained in Italy and then in other countries turned out to be very useful to the inventors after 1950, when slow neutrons started to be widely applied.

The discovery of the decelerating effect of hydrogen gave rise to the necessity of resolving a number of other problems, and soon, besides the investigation of radioelements produced by neutrons, an important part in the work was played by studies of the properties of the neutrons themselves (their slowing down, scattering, absorption, etc.).

A quite detailed exposition of the results of studies performed during the first three months after the discovery of the slowing down of neutrons is given in an article presented by Rutherford in the "Proceedings of the Royal Society". This article contains practically all the principal ideas of the physics of slow neutrons, with the exception, naturally, of the most important issue of "groups of neutrons" and of neutron resonances, which, by the way, Fermi was able to fully substantiate and clarify a year later. As one can see, the rate of our work was extraordinary! At the same time no hurry was ever felt in the laboratory, and Fermi was always calm.

After the effect of slowing down was discovered, irradiation of uranium (and thorium) was carried out systematically with and without paraffin. However, the mystery of the multiplicity of the sorts of activity was not resolved by Fermi; for this to happen the discovery of fission by Hahn and Strassman (1939) had to be made. Now it seems a miracle to me that Fermi with his profundity and intuition had not been able to predict the fission process theoretically.

As to the experiments with uranium, the group of Fermi just had no luck at the time: fission could have been discovered experimentally in Rome in January 1935, had there not occurred certain circumstances. A few words are due on this point. According to Fermi, among the numerous sorts of activity induced by neutrons in uranium and thorium there could also be α-activities. Such α-active radioelements with a period exceeding several seconds were sought for, but were not found. Then, the possibility was considered of the bombardment of uranium and thorium with slow neutrons resulting in α-particles being emitted instantaneously, like what we found in bohr. The equipment used for observing the α-particles was the same as the one used in experiments with bohr. Thus, a sample of uranium oxide surrounded by paraffin was bombarded by slow neutrons; the sample was placed in front of a small pulsed ionization chamber connected with a linear amplifier capable of detecting ionization pulses from the α-particles. Since the α-particles sought were considered to have longer path ranges than the natural α-particles from uranium, for reducing the background of the latter an aluminium foil of thickness equivalent to 5 cm of air was put in front of the uranium sample. It was precisely this foil that prevented us from observing the large pulses of ionization due to fission fragments! Frequently in 1939 and later, also, Fermi's collaborators discussed the episode with the "nasty" aluminium foil and asked themselves the question: "Suppose we had observed large pulses of ionization from uranium in 1935; could have Fermi understood the phenomenon, i.e. would he have discovered fission?"