

Introduction

Historical prelude

It has been known for many years that vast amounts of energy are stored in the nuclei of many atomic species and that their release is not in contradiction with the principle of the conservation of the energy, nor with any other of the accepted basic laws of physics. In spite of this recognized fact, it was the general opinion among physicists until recently that a large scale release of the nuclear energy would not be possible without the discovery of some new phenomenon. [Fermi (1946b)]

The discovery of the fission of uranium and the possibility to produce a chain reaction

Such a new phenomenon, as mentioned by Fermi in one of his reviews of 1946, was that observed by Otto Hahn and Fritz Strassmann in the Fall of 1938 at the Kaiser Wilhelm Institute in Berlin, when bombarding the uranium nucleus with neutrons from a radium-beryllium source. The correct explanation of the Hahn and Strassmann experiments was soon given by Lise Meitner and Otto R. Frisch who interpreted the observed phenomenon as due to the splitting of uranium, from which two elements formed, each of approximately half of its original mass. The mass which “disappeared” was assumed to be converted into energy, according to Einstein’s theory of relativity.

The news of the novel phenomenon reached the other side of the Atlantic Ocean just after Fermi and his family arrived in America, after receiving the Nobel Prize in Stockholm.

Niels Bohr, who had come for a stay at Princeton, was on his

way to attend a conference in Washington. [...] By the time he was ready to leave Princeton, Bohr had heard the results of Frisch's experiments. It was a most exciting development. [Anderson (1984)]

Willis Lamb was in Princeton at that time and, after heard from Bohr of this breaking news, he went to Columbia University and communicated it to Fermi [Segrè (1970)]. Quite independently, according to Anderson's recollections,

on his way to Washington, Bohr thought it would be a good idea to drop by and see Fermi to tell him about the exciting new physics. He came to the Pupin Physics Laboratory looking for Fermi. [...] He didn't find Fermi; he found me instead. I was the only person around. He hadn't see me before but that didn't stop him. He grabbed me by the shoulder and said, "Young man, let me tell you about fission." [...] I had heard enough to catch the excitement. [...] When Bohr left I felt I had something to tell Fermi. [...] "Professor Fermi, I've come to tell you that I have just seen Professor Bohr. He was looking for you and he told me some very interesting things." Fermi interrupted me. A smile broke out and he said, "Let ME tell you about fission." Then I heard again, but this time much more graphically, how the energy would appear when the uranium was split and the pieces flew apart by Coulomb repulsion. [Anderson (1984)]

After the news spread out, many physicists (including Fermi and collaborators) confirmed the results by Hahn and Strassmann and proved true the interpretation and suggestions by Frisch, working rapidly for a better understanding of the phenomenon.

In the spring of 1939 it was generally known that a fission that can be produced by the collision of a single neutron with a uranium atom was capable of producing more than one new neutron, probably something of the order of two or three. It was felt at that time by many physicists that a chain reaction based on the uranium fission was a possibility well worth investigating. [Fermi (1946b)]

The idea of a nuclear chain reaction able to liberate energy on a large scale came to Leo Szilard as early as in 1933-34, when it was believed that beryllium (instead of uranium) was unstable and that neutrons would split off when this element disintegrated. This proved soon incorrect, but the possibility to create a process that would emit more neutrons than were absorbed (or, in other words, with a multiplication or reproduction factor

greater than one) came back into the picture when the fission of uranium was discovered. This was promptly recognized by Szilard who, according to Anderson [Anderson (1984)], “was very anxious to work with Fermi, or at least to have discussion with him” in order to achieve effectively a chain reaction.

Natural uranium and graphite

In 1939 a number of experiments were performed to put the problem of fission on a quantitative basis. The first important fact to be realized was that the cross section for neutron fission was higher for low energy neutrons, while the second one was that the key isotope of uranium involved in the fission induced by slow neutrons was the rare one of mass 235, instead of the most abundant ^{238}U . The problem was, however, complicated by the fact that, besides producing fission, slow neutrons can also give rise to the production of the radioactive isotope ^{239}U by simple capture. In particular the capture of neutrons with thermal energies (thermal neutrons) was proved to be due to a strong resonance absorption at somewhat higher energies [Anderson (1939a)]. Such a process competes with fission in taking up the neutrons which are needed to sustain a chain reaction, so that a major problem in making the chain reaction to be effective was to avoid losses due to this absorption.

In any case, the first basic point to be cleared up was the choice of the fissile material to be used and, in this respect, two alternatives were opened at the end of 1939. The first one was the separation of ^{235}U from the natural uranium, thus eliminating the absorption by the most abundant isotope ^{238}U . Obviously, for this method to work, the major difficulty for that time was to produce large quantities of the isotope needed. The alternative choice was, instead, to use directly natural uranium, with the evident drawback caused by the undesirable absorption of neutrons by the most abundant isotope, which may lower significantly the multiplication factor for the self-sustaining reaction to be achieved. The problems with both the alternative methods were serious, and Fermi chose to work out the one where more physical effects should be understood and kept under control, i.e. he decided to study the possibility of a chain reaction with natural uranium. It is quite interesting to observe that Fermi was very confident that such a way was the right one:

“Herbert,” he said, “if you stick with me we’ll get the chain reaction first. The other guys will have to separate those isotopes

first, but we'll make it work with ordinary uranium." [Anderson (1984)]

Such an attitude, as usual for him, came from the appropriate quantitative results he and his collaborators obtained from an extensive experimental work. Here, as already mentioned, the discriminating factor was the slowing down of the incident neutrons, which makes more effective the cross section for fission with respect to that for absorption.

The problem of the slowing down of neutrons and its effect on the development of neutron-induced nuclear reactions (and, in particular, the production of radioactive elements) had been the subject of intense and fruitful researches by Fermi and his group in Rome as early as 1934 [De Gregorio (2006)], and lead to several important papers, collected in [Fermi (1962)]. A patent for the practical applications of the results obtained was as well issued; the interesting subsequent anecdotes related to this patent have been narrated in [Segrè (1970)]. It was recognized that the most efficient way to slow down neutrons was to pass them through hydrogen, the lightest chemical elements present in water, paraffin, etc., so that the obvious conclusion for getting a reproduction factor high enough for a chain reaction was to disseminate uranium powder in water. However, measurements revealed [Anderson (1939b)] that thermal neutron absorption by hydrogen was too large for water to make it a usable medium for slowing down neutrons in a chain reaction, since that absorption (leading to formation of deuterium) would lower substantially the multiplication factor. Thus, other light elements should be taken into consideration.

Out of Szilard's thinking came the idea of using graphite instead of water to slow down the neutron. [...] Fermi had also been thinking about graphite. [Anderson (1984)]

Measurements showed [Anderson (1940)] that the absorption of neutrons on graphite was small enough to make it the obvious choice for a material for slowing down the neutrons, so that Fermi set forth also the basic theoretical techniques for describing the behaviour of neutrons in such substances. It was also shown that, after the neutrons reached thermal energies, a second diffusion process began in which the neutrons continued to diffuse through the material until they either escaped or were absorbed. The advantages of graphite against water, as a moderator for neutrons, came out from experiments with a pile of graphite aimed at measuring the absorption of carbon [Anderson (1941b)]. In such a pile the neutrons were slowed down more slowly than in water, but once they reached thermal

energies the neutrons would diffuse longer and reach greater distances from the source. As a consequence, a physical separation of the thermal neutrons from higher energy ones could be obtained, and this property was later used by Fermi in many different ways.

Experimental piles

At this point of the story, the next step was to design a chain reacting pile that would work, and, to this end, a number of experimental piles were built, early at Columbia University in New York and then at Chicago, to study directly the properties of uranium and graphite (or other moderators) in a pile.

The key ingredient was, of course, to work with sufficiently pure materials; these were obtained from different factories (with quite different degree of purity), and always were tested by Fermi and his collaborators. A chemical method, involving ether separation, was used to purify uranium [Anderson (1941a)] while the absorption of neutrons by graphite was especially measured.

Graphite bricks were stacked into the so-called “sigma pile” (denoted with the Greek letter “sigma”), designed to measure the absorption cross section. A neutron source was placed near the bottom of the pile and indium foils were exposed at various points on the vertical axis above the source; from the radioactivity induced in these foils the absorption cross section of graphite was deduced. To this regard, standard procedures were introduced [Anderson (1941c)] by which indium (and rhodium) foils could be calibrated in order that the measurement of their radioactivity could be used to give either the slow neutron density or the slowing down density in absolute units. The graphite column erected at Columbia was also used as a source of thermal neutrons in the measurement of the absorption cross section of boron. This element, in fact, had importance in absolute neutron measurement, because of its high neutron absorption cross section and its dependence on the inverse of the velocity of neutrons [Anderson (1942a)].

For uranium, apart from its purification, an important problem was that of resonance absorption, as mentioned before. The idea then came out of using uranium in lumps, just to reduce the resonance absorption. Also, Fermi measured the resonance absorption for uranium oxide compressed into spheres and, in particular, when these spheres were embedded in graphite [Fermi (1941)]. Evidently, he was already thinking about experiments to test a “complete” uranium-graphite reactor.

Meanwhile, the fission of uranium induced by fast (rather than slow) neutrons was as well investigated to some extent, not only for the possibility of obtaining a fast neutron chain reaction, but also for measuring the contribution of fast neutron-induced reactions to the slow neutron chain reaction [Anderson (1941d)].

Fermi and Szilard had the very important idea of placing the uranium oxide in a lattice in the graphite, instead of spreading it out uniformly [Wattenberg (1984)]. Here the problem was “to ascertain whether a given lattice of uranium oxide lumps embedded in graphite could give a divergent chain reaction if its dimensions were made sufficiently large” [Anderson (1942b)], by exercising the greatest care in keeping under control possible losses of neutrons.

In order to test with a smaller structure whether a larger one would work, Fermi invented the “exponential experiment”. Uranium was placed among the graphite bricks in a cubic lattice array, with a radium-beryllium neutron source near the bottom and indium foils exposed at various distances from it on the vertical axis. The arrangement is, thus, similar to that of the sigma pile, but the exponential pile was much larger than the sigma pile. The exponential decrease in the neutron density along the axis is greater or less than that expected due to leakage according to whether the reproduction factor is less or greater than one.

Such exponential piles were developed at Columbia in Summer-Fall of 1941 [Fermi (1946b)]; they produced results indicating that even an infinite amount of material would not lead to a self-sustaining structure, this being due mainly to the impurities in the graphite. The situation changed when, during the following Spring (1942), some new graphite was available. The last two experiments performed at Columbia, before the move to Chicago, gave encouraging results [Fermi (1942c,d)], and definitively demonstrated an understanding of the physical effects being involved.

Achieving the first nuclear chain reaction

The National Academy of Science Committee, whose chairman was Arthur H. Compton of the University of Chicago, was charged to review the uranium projects of the United States and to judge their military importance. At the end of 1941 the Committee decided that the work made by the Fermi group using natural uranium was important and, one day before the Pearl Harbor attack on December 7, 1941, the Metallurgical Laboratory was established with Compton as its scientific head in Chicago. For people

working on a chain reaction using natural uranium, Chicago became the only game going and, finally, Fermi and his group at Columbia definitively moved to Chicago in April 1942.

Under Compton leadership a large number of people came too. Among them there was Szilard who worked hard getting the graphite free from neutron absorbing impurities, and Norman Hilberry, who did a marvellous job procuring what was needed. Soon large quantities of graphite began to appear for us to test. Equally strenuous efforts were expended getting uranium in forms sufficiently pure. First we worked with uranium oxide. Then various people worked to produce uranium metal. Outstanding among those was Frank Spedding from Iowa State University. [...] Spedding's uranium was an important component of the first chain reaction. [Anderson (1984)]

A number of engineers then came into the project to produce an appropriate and feasible design of a chain reacting system, so that a first practical problem was to “translate” the known physical achievements into a form suitable to them who had little knowledge in a field completely new. To this end, Fermi invented the notion of “danger coefficient” [Fermi (1942a)] for identifying the impurities which were dangerous for the realization of the chain reaction, due to their high neutron absorption cross section. The effect of such impurities was, then, taken into account directly on the evaluation of the multiplication factor through those danger coefficients. For example, it was determined the effect of gases in the interstices of the graphite, mainly concerning with the appreciable amount of nitrogen impurity in the porous graphite, or even the effect of the undesirable impurity of water in graphite or uranium [Wattenberg (1984)].

Another problem studied was the stability of the pile against temperature changes, since the heat production in the reactor would have altered the reactivity of the pile [Fermi (1942b)] [Christy (1942)].

The study of the uranium-graphite reactor was not the sole work carried out at the Metallurgical Laboratory; other possible systems were as well considered and some measurements made. This is the case, for example, of the so-called “water boiler”, that is a reactor system made of a central uranium core enriched with ^{235}U and water around it serving as a moderator [Breit (1942)]. Also, the multiplication factor of a uranium oxide system with a beryllium metal as neutron moderator was measured [Wattenberg (1984)].

Turning back to the study of the main uranium-graphite reactor, the first important result was obtained in August 1942, when very pure uranium oxide was delivered to the Laboratory, making the reproduction factor K greater than one for the first time [Fermi (1942e)]. The 4% excess available ($K = 1.04$) effectively opened the road to the building of the first self-sustaining pile, the Chicago Pile No. 1 (CP-1).

The major engineering problem with it was the choice of an adequate cooling system with sufficiently low neutron absorption, since the “official” motivation for the project was to produce plutonium, another fissile material (other than ^{235}U) to be used also for military purposes. Indeed, “a large effort was underway for planning the pilot and production reactors, on the assumption that CP-1 would succeed” [Wattenberg (1984)]. Alternative choices [Fermi (1942g)] were proposed to cool the system by gas (preferably helium), water, or even liquid bismuth, this ingenious proposal by Szilard being later set aside because of the lack of engineering experience with this material. The Chicago group definitively worked on the design for a helium cooled plant submitted by the engineers T.V. Moore and M. Leverett.

So it happened that on 15th of November [1942] we started to build the pile in the West Stands [of the Stag Field, in Chicago.] [...] Fermi wanted to build the pile with a shape as close to spherical as possible. This would minimize the surface/volume ratio and make the best use of the material which would become available. [...] A major change in design came when we had news that Spedding would be sending some of his high purity uranium metal. The best place for this was as close to the center as possible. As a result, the shape of the pile was changed as we went along. The spherical shape we started with got squashed somewhat as we went along because the purity of the material we were getting was better than we had anticipated. [Anderson (1984)]

The delivery of the Spedding’s metal avoided the use of another ingenious trick proposed by Anderson, i.e. to build the pile inside an envelope made of ballon cloth to remove the air (and replace it with Carbon dioxide), in order to minimize the absorption of neutrons by the nitrogen in the air within the pile, with a gain of about 1% in the reproduction factor [Fermi (1942f); Anderson (1984)].

To initiate the chain reaction, it was not necessary (as in experimental piles) to introduce in the pile a separate neutron source since, as already experimentally measured, the uranium also had a non-vanishing probability

for spontaneous fission, so that it emits a few neutrons of its own. However, when the pile was building, to keep it from becoming too reactive once it began to approach the critical size, some neutron absorber was needed to control the reactivity of the chain reaction. Control rods were, then, inserted within the pile, made simply of strips of cadmium, since such element was known to be a strong neutron absorber. The pile was controlled and prevented from burning itself to complete destruction just by these cadmium rods, which absorb neutrons and stop the bombardment process of uranium. Further safety arrangements were as well conceived and set up by Fermi for the first reactor (see [Anderson (1984)], for example), whose construction resulted to be completed about a week earlier than the director of the Metallurgical Laboratory had officially anticipated. In the afternoon of December 2, 1942, in fact, the Chicago Pile No. 1 finally got critical and a chain reaction successfully started for the first time.

We had built the pile, and Fermi had established that we could get a self-sustaining nuclear reaction that we could control in a very predictable manner. [Wattenberg (1984)]

Further studies on nuclear piles during the war years

The further development in the studies on nuclear pile, during the three years 1943-45 was of course focused on the main objective of producing weapons, so that it is natural to expect very few detailed information on these classified topics. Indeed, none of these appeared in the *Collected Papers* by Fermi [Fermi (1962)], and our source of information is only composed of eyewitnesses (see, for example, [Segrè (1970)]). However, quite fortunately, some reports exist that testify on part of Fermi's activity during these years, not strictly and directly related to military applications, though those reports had been classified for some time (see [Fermi (1962)]).

First of all, the pile was used as a suitable device for checking directly the purity of the uranium and for studying a number of features of the uranium-graphite lattice, unaccessible before [Fermi (1943a)] [Fermi (1943b)]. However, after about three months of operation, the original CP-1 pile was explored sufficiently to learn how to rebuild it with many improvements. A second pile, CP-2, was effectively built at the Argonne site, near Chicago, in March of 1943, and several studies started to be done. These were mainly aimed at designing an efficient pilot plant for producing plutonium or for isotope separation. Such plants were actually erected (at the end of 1943 and later on) at Oak Ridge, Tennessee (known as "Site

X”) and at Hanford, Washington (known as “Site W”). An example is the designing and test of a radiation shield for the production piles to be built at Hanford, mentioned in [Fermi (1943d)].

The pile was also used as a tool to measure neutron absorption cross sections by several elements. Samples of these elements were put in the pile, and the compensating changes in control rod position were determined [Anderson (1947)]. This method also became a routine tool for checking for neutron absorbing impurities in the materials used in reactors.

Some explicit “physics works” was, furthermore, carried out when the so-called “thermal column” was devised by Fermi and incorporated in experimental piles [Fermi (1943e)]. A graphite column was, in fact, set up on the top of a pile, where thermal neutrons could be found with substantial intensity and essentially free from those of higher energy. This led to the discovery of a novel phenomenon, that is the diffraction of thermal neutrons by graphite lattice [Anderson (1946)], which opened the road to investigate the wave properties of neutrons [Fermi (1944c)]. The increased neutron intensity available from a pile also allowed to obtain truly monochromatic beams of neutrons for different experiments (such as, for example, the measurement of the boron cross section at a well definite neutron velocity). This was made possible by a thermal neutron velocity selector designed by Fermi at the Los Alamos Laboratory (known as “Site Y”) [Fermi (1947)] and then built at Argonne.

The fission spectrum of uranium was also measured accurately by exploiting the slow neutrons provided by a pile, which were then absorbed by a layer of uranium. Other physical properties of ^{235}U and ^{239}Pu were as well determined [Fermi (1944b)], and these measurements, performed at Los Alamos in 1944 with the active collaboration of Chicago’s people, revealed somewhat unexpected properties of plutonium. In the same period some interesting work was also done on the theoretically possible phenomenon of “breeding” [Fermi (1944a)], namely of producing more fissionable material in a reactor than was consumed, clearly depending on the effective number of neutrons available in the chain reaction.

The increased production of heavy water in 1943 made possible to take seriously into account a proposal by H.C. Urey of April 1942 to use heavy water as neutron moderator. This led to the construction of an experimental reactor, known as P-9 and later becoming CP-3 pile, which would have much more power than CP-2, thus extending the experimental possibilities [Fermi (1943f)].

Finally, other effects were studied during 1944, ranging from the dissoci-

ation pressure of water due to fission [Anderson (1944)] to the measurement of the amount of nitrogen in the first production pile at Hanford. An unexpected problem with the Hanford pile was also studied, and independently solved by Fermi and J. Wheeler, on the xenon poisoning, which caused the full stop of the chain reaction [Fermi (1944d)].

Further works by Fermi until the end of the Second World War concerned mainly the realization of the atomic bomb at the Los Alamos Laboratory, so that the corresponding written reports were strictly classified and not available for the *Collected Papers*. A relevant exception are the lecture notes [Fermi (1946a)] for a course that Fermi gave at Los Alamos just after the end of the war, in the fall of 1945. Here he summarized the results achieved on neutron physics, with particular reference to nuclear piles. These lectures are an example of the didactic ability of Fermi rather than a source of information about his research work.

The set of lectures on neutron physics

Once the pile program of the Metallurgical Project in Chicago was sufficiently advanced not to need a continuous attention by Fermi, he definitively moved to Los Alamos (in September 1944) to join the Manhattan Project. Here Fermi began to give isolated lectures on many different subjects [Anderson (1984); Segrè (1970)], related to that project, for the benefit of the people who worked at Los Alamos, many of them being just students or graduated guys. Then, after the end of the war, in the Fall of 1945 he taught a regular course on neutron physics to about thirty students: this was the first time that such a complete course was given, ranging over more than ten years of important discoveries, and also the first occasion for the scientists who contributed in those achievements to pause and reason a bit more on the results obtained.

We know about the content of this course from the notes taken down in class by one of the attending students, I. Halpern, who assembled them into a (classified) typescript on February 5, 1946. A first part of the Fermi lectures at Los Alamos, containing neutron physics without reference to chain reactions, was declassified on September 5, 1946, while the remaining part has been declassified only in 1962. Both parts have been later published in the *Collected Papers* by Fermi [Fermi (1946a)]. Leaving aside the pregnant didactic style by Fermi, the main relevance of such notes is, as we have already mentioned, that they present for the first time a complete

and accurate treatment of neutron physics from its beginning, including a detailed study of the physics of the atomic piles. In this respect it is not surprising that especially the second part of the notes, dealing just with chain reactions and pile physics, was considered as “confidential” material by governmental offices.

However, one of us (S.E.) has recently recovered a *different* version of the Fermi lectures at Los Alamos, formerly belonged to James Chadwick and now deposited at the Churchill Archive Centre in Cambridge (U.K.). Two relevant (for the main topic of the present volume) folders exist in this archive. The first one (CHAD I 17/3) contains a letter from R.T. Batson of the Atomic Energy Commission (A.E.C.), a copy of the paper *Elementary Theory of the Pile* by Fermi¹ and a copy of only the *first part* of the Halpern notes of the Fermi lectures. The second folder (CHAD I 4/1) contains a version of the *complete* set of lectures made by A.P. French, dated June 23, 1947.

It is apparently not strange that the material of the first folder belonged to Chadwick, since he was the respected (also by Americans) leader of the British Mission in the United States. The biggest part of the British contingent was, in fact, at Los Alamos, and Chadwick himself was present at the world’s first nuclear test at Alamogordo on July 16, 1945. Several scientists of the British Mission were very young and, among the others, it was Anthony P. French who graduated in Physics at the Cambridge University just in 1942. In the same year he joined the atomic bomb project (“Tube Alloys”) at the Cavendish Laboratory, and was later sent to Los Alamos in October 1944 as a member of the British Mission. Here he worked with E. Bretscher, O.R. Frisch, J. Hughes, D.G. Marshall, P.B. Moon, M.J. Poole, J. Rotblat, E.W. Titterton, and J.L. Tuck in the field of experimental nuclear physics [Szasz (1992)]², and returned to the United Kingdom in 1946, working for two years at the just newly formed Atomic Energy Research Establishment (A.E.R.E.). The second folder of the Chadwick papers mentioned above contains just the notes of Fermi course on neutron physics taken by French on his own, when he was at Los Alamos, and later (1947) re-organized into a final version when he came back to England.

In the first part of the present volume we report, for the first time, just the complete set of the French notes.

¹This paper is reproduced in [Fermi (1962)]; in particular see page 538 of Volume II.

²The remaining part of the British Mission was composed by B. Davison, K. Fuchs, D.J. Littler, W.G. Marley, R.E. Peierls, W.G. Penney, G. Placzek, H. Sheard, and T.H.R. Skyrmes.

From these notes it comes out that our previous knowledge of the Fermi course was incomplete and, to some extent (limited to the Halpern notes), misleading. We have indeed performed a careful analysis of the mentioned documents, and the main results are summarized below.

First of all, our study has shown that the French notes do *not* depend on the Halpern ones, but French probably saw them (the organization of the introduction is similar). The topics covered are exactly the same, although to a certain (minor) extent the material is organized in a little different manner. Almost all the topics discussed were expounded by Fermi; according to French, when Fermi was absent, R.F. Christy and E. Segrè treated the scattering of neutrons and the albedo in the reflection of neutrons, respectively.

The text of the notes is different in the French and Halpern versions; in few cases, however, similar or even identical words or sentences are present in both versions, likely denoting quotes from an original wording by Fermi. In general, the French notes are much more detailed and accurate (as may be roughly deduced even looking at the table of contents), with a great number of shorter or larger peculiar additions³ (explanations, calculations, data or other, and 5 more exercises) not present in the Halpern notes. It is quite interesting that the greater detail already present in the French notes increases even more in quality (especially figures and data) in the last part, directly related to chain reactions and their applications, and, moreover, explicit references to bomb applications are made (see below). By limiting ourselves to significative scientific remarks or discussions, the French version of the Fermi lecture notes contains about 100 additions, 18 of them being quite relevant while the remaining part accounts for minor remarks, calculation details or figures. Instead the peculiar additions present in the Halpern version but not in the French one are only about 30 (and 3 more exercises), only one of them being relevant. Also, the French paper contains the six questions which were set as a final examination at the end of the lecture course.

In order not to distract reader's attention, we have preferred not to indicate all these differences throughout the volume, but we report in the

³The case is completely different, for example, from that of the revision of the (first part of the) Halpern notes made by J.G. Beckerley in 1951 (document AECD 2664 of the Atomic Energy Commission). Here the author *re-wrote* the Fermi lectures by including several additions from *other* sources, "where clarity demanded more information and where the addition of recent data made the text more complete." Contrarily to the present case (as it is evident from the text of the notes), Beckerley "was not privileged to attend the course" by Fermi.

following the most relevant additions (the references correspond to what can be found in the present volume):

- the entire section 2.3, *The Binding Energies of Nuclei*, where the definition of the binding energy and an example for calculating it in a specific case is reported;

- the introduction of Sect. 5.1: “In this section we consider the solution by wave mechanics of a simple problem in nuclear scattering. The nucleus is considered as a centre of force, the force being of short range, so that it ceases to exist beyond a certain distance r_0 from the origin. The actual shape of the nuclear potential then approximates to a square well, as shown in Fig. 5.1. The potential U is negative and constant over most of the nucleus. This corresponds to the fact, as far as we know them, of the interaction between a neutron and a nucleus. The depth of the nuclear potential well is equal to the binding energy, that is about 8 MeV.”;

- some details in Sect. 6.2 about the Bragg scattering of slow neutrons by an element with different isotopic composition, ending with the following remark: “The total scattered intensity is thus given by $I_{sc} = \text{const.} (\sigma_1 + \sigma_2 \pm 2\sqrt{\sigma_1\sigma_2})$, and may be seen to consist of coherent and incoherent contributions, the latter not being subject to interference”;

- a long discussion in Sect. 7.4 (see pages 55-57), with detailed calculations about the spatial distribution of slowed neutrons, aimed at calculating the source strength in neutrons per second both for a thermal detector and for a resonance detector (final explicit expressions are reported);

- calculation details on pages 63-64 about the neutron scattering in a medium (with the determination of the mean free path), ending with a prediction for the neutron-proton scattering cross section (in water) of $\sigma \simeq 20$ barns which “agrees very closely with the accepted value”;

- discussion in Sect. 9.6 (see pages 77-79) on calculation details aimed at solving the so-called (Fermi) age equation for the diffusion of neutrons from a Ra-Be source in a column of graphite of square section (with length of side a) and infinite length; the effective length of a side of the column, $a = a_{\text{geometrical}} + 2 \cdot (0.67\lambda)$ (λ being the mean free path), and the range of the neutrons, $r_0 = \sqrt{4\tau}$ (τ being the age parameter), are introduced; the numerical values of r_0 (instead of only τ as in the Halpern notes) for three (instead of two) typical neutron energies are given; the addition in the French notes ends with the peculiar observation that “we have the somewhat paradoxical result that the system can be made infinite for fast neutrons being slowed down, but not for the same neutrons when they have

become thermal”;

- after some calculations in Sect. 10.5 (see page 86), this ends with the observation that “a thermal neutron in these media [paraffin and water] makes about 100 collisions before being captured. The distance it travels, measured along the path, is about 80 cm on the average, and the time it takes to do this, which is its lifetime as a thermal neutron, is something less than a millisecond”;

- introductory remarks in Sect. 11.1 on the binding energy of a nucleus, with the theoretical expression for the measured mass of an atom in terms of A , Z and the said binding energy;

- several important additions on pages 89-92 related to the stability of nuclei (according to the even- and odd-ness of Z , A or both) and the accurate determination of the expression of the binding energy of nuclei in terms of Z and A , with several numerical data (and a graph);

- the inclusion in Sect. 12.5 of three graphs (Figs. 12.10-12.11) for the cross section of (n, γ) and $(n, \text{fission})$ processes on uranium as function of the incident neutron energy;

- relevant additions on pages 112-113 about homogeneous and lumped graphite piles: explicit calculations of the neutron absorption volume by uranium spheres of 3 cm radius (and of other quantities) lead to the conclusion that “no homogeneous pile of this type will work, and we must therefore devote our attention (if we are considering only the U-graphite combination) to heterogeneous piles”;

- small introduction of Sect. 14.1 (with key comments) to the design of a lumped pile, with figures (Fig. 14.1) of lattice structures with spherical lumps or rods of uranium;

- the two relevant figures 14.4 (and related discussion) about efficient cooling systems (by blast of air or water flowing) for piles;

- introductory remarks to Sect. 14.3 dealing with the reproduction factor and critical size of a pile, with a graph (Fig. 14.4) of the actual neutron density in a finite pile as a function of the distance from the center of the pile; explicit expressions and related comments on pages 121-122 on the reproduction factor K as a function of geometrical and other parameters of the pile;

- “We have discussed the mechanism of thermal neutron chain reactions. The question now arises how to produce a nuclear explosion” (see page 139); introductory remarks in Sect. 17.1 about fast reactors starting from the calculated expression for the growth of neutron density in a reactor;

- on pages 141-142, definition, calculations and related discussion on the

transport cross section and transport mean free path for neutrons in a fast reactor with a core of ^{235}U and a tamper (neutron reflector);

- discussion in Sect. 17.3 of equilibrium conditions (with explicit expressions) for a fast reactor and mathematical expressions for some quantities describing neutron losses (see pages 144-146);

- the end of Chap. 17 on fast reactors (and, then, of the lecture notes) is: “in this way one can calculate the e-folding time for a fast reactor, and its value thus found will be valid until mechanical effects set in – these having to be known before the efficiency etc. of the bomb can be estimated”.

The only relevant addition in the Halpern version is, instead the description of a fast neutron detector, based on the scattering of a neutron flux by a paraffin layer (see page 471 of Volume II of [Fermi (1962)]).

Fermi's patents

A less known part of the history of the development of the Manhattan Project, with its military and non-military applications, is the controversial issue of patenting the novel scientific and technological discoveries [Wellerstein (2008)]. The utilization of patents was, indeed, an *ad hoc* attempt at legal control of the nuclear energy by governmental administrators, and patents themselves played an important role in the thinking of project administrators concerned with meaningful postwar control of the atomic energy.

As a matter of fact, when the Los Alamos laboratory was formed, it included a patent office as part of its administration, starting its operation in July 1943; captain R.C. Smith was appointed as advisor on patent matters. That office collected records of the researchers completed work, with a view to covering them by patents (Smith commanded that the personnel keep workbooks with records of recent discoveries) but, of course, the patent applications on “sensitive” inventions were placed under a “secrecy order”, and the patents could not be issued until the applications were declassified. Smith's office worked on five hundred patent cases altogether; completed cases were then filed with the U.S. Patent Office.

Fermi himself became an “inventor”, and his “inventions” were described in several patents on nuclear reactor designs, processes, methods and instrumentation. All but two among these patents (accounting for a total of 15 papers) were filed between May 1944 and January 1946, while the

last one was a re-issue (with small corrections) of the fundamental patent on nuclear reactors filed by Fermi and Szilard on December 1944.

An intriguing, though particular, story is deserved for the first patent about the process for the production of radioactive substances, originally issued in Italy as early as in 1934 [Segrè (1970)]. Indeed, when in October 1934 Fermi and his group in Rome discovered the well-known effect of enhancing artificial radioactivity with slow neutron bombardment [Amaldi (1935)], the Director of the Institute of Physics, Orso Mario Corbino, recognized the potential practical importance of such effect and urged Fermi to file for a patent. This was effectively done, and on October 26, 1934 an Italian patent was granted (which was later extended to other countries, including U.S., Great Britain, Belgium, France, Germany and Canada) concerning a method of producing radioactive substances by neutron collisions and, in particular, covering the increase of efficiency obtainable by slowing the neutrons with multiple elastic collisions. After the discovery of fission, when nuclear energy development started in earnest, this patent became obviously fundamental to all industrial and military applications, and hence of considerable value. However, during the war it was neither possible nor desirable for Fermi and collaborators to raise questions about compensation for that patent. The question was pointed out only at the end of the war, but then the affair became even more complicated since, although the Atomic Energy Act sought to restore legality in the use of atomic patents by allowing their purchase, governmental managers were resistant to settling existing claims and eventually they used the new legislation to argue that no compensation should be paid. In addition, since Bruno Pontecorvo was among the inventors of this patent, his flight to Soviet Union in October 1950 further complicated matters. At the end, after much legal wrangling, the controversy was solved, and the government of the United States paid some compensation for the patent's rights [Turchetti (2006)].

The whole set of Fermi's patents were not included in the *Collected Papers* published in the 1960s, probably due to the fact that many of them were kept confidential at that time: all but two of them, indeed, deal with the technical and operative construction of nuclear reactors. This fact, unfortunately, has prevented us to know about the precise contributions of Fermi on pile physics and engineering, although the activity by himself on this subject was early well recognized from the accounts given by the living testimonies (see, for example, [Segrè (1970)]) and partially documented by several papers appeared in the Fermi's *Collected Papers*.

Nevertheless, from the direct reading of the text of the patents, a num-

ber of important scientific and technical points comes out, putting some new bright light on the Fermi's activity about the construction and functioning of nuclear reactors. In practice, what Fermi *effectively* did in this applicative field is here technically documented, and very clearly emerges from these papers. It is quite impressive the fact that, just from the accurate reading of the patents, anyone who has at his own disposal the necessary materials could effectively build a working reactor, with a number of possible alternatives.

Different topics are, instead, covered in the first and the eighth patents of the set, though they are strictly related to pile physics research. The first case refers to the Italian patent, dealing with the production of radioactive substances, already discussed above. Instead, in the second case, the corresponding patent presents a detailed description of the construction and operation of a velocity selector for neutrons [Fermi (1947)].

For any of these patents, reported in the present volume, a concise description of their content has been added as an introduction. We then refer the reader to these introductory texts for further information about the specific content of the given patents.

This volume

In the present book we reproduce, for the first time, the French notes of the Fermi's lectures on neutron physics and, in the second part, the complete set of the 15⁴ patents authored (or co-authored) by Fermi. All these documents do not appear in the Fermi's *Collected Papers* published in the 1960s [Fermi (1962)].

For the French notes, we have used the only available document (to the best of our knowledge) reporting them, i.e. the copy belonged to James Chadwick and now kept at the Churchill Archive Centre in Cambridge (U.K.). In particular, such a copy (dated June 23, 1947) is conserved in the folder CHAD I 4/1 of the Papers of Sir James Chadwick at the archive mentioned.

Instead, for the text of the Fermi's patents we have employed the material conserved at the United States Patent and Trademark Office. For several of these patents, however, this Office is not the only available source of documents, which may also be found in the Patent Offices of Italy (patent no. IT324458, the original one), Great Britain (patent no. GB465045),

⁴Note that two patents belonging to this set are almost identical; see below.

Belgium (patent no. BE411973A), France (patent no. FR796795A), Germany (patent no. DE681540C) and Canada (patents no.s CA407558A, CA407559A, CA552312A, CA552693A, CA619065A, CA620923A). In general, these alternative versions do not contain more information⁵ with respect to the U.S. patents, this motivating our choice.

The major effort we have made to carefully check and type all equations and tables was motivated by our desire to facilitate the reading of Fermi's unpublished papers as much as possible. However, we have tried to keep as much as possible the original editing, this holding, in particular, for sectioning and equations (especially in Part 2, however, for a better understanding we have introduced some punctuation marks). Additions or modifications are properly pointed out throughout the text; in particular, explicative non-original footnotes are marked with the symbol @. The figures present in the first part (lecture notes) have been redrawn electronically for a better display, without the use of photographic or scanning devices but are otherwise true in form to the original drawing. Figures appearing in the patents, instead, have been elaborated directly on the originals, in order not to lose important though tiny details. In both cases, the numbering of figures is different from that present in the original documents: here we have numbered them according to the sections where they are present, thus keeping traces of their relative position. Tables have been reproduced, again, as close as possible to the originals but, for editorial reasons, especially for Part 2 their position is not always the original one, the same being true for figures (in the original patents, all the figures are placed at the beginning of the patent itself). In few cases, whenever required by pagination, very long tables have been broken into two or more parts. For units, we have adopted the common modern notation instead of that present in the original documents (for example, "g" rather than "gr" for gram, "s" rather than "sec" for second, etc.).

Finally, for any of the patents reported, we have added (in italics) a brief introduction about their content, in order to present shortly the key points of what described in the given patent.

The 3rd and 15th patents (that is U.S. patents no. 2708656 and 2798847) refer to the same work, the text being practically the same. The second version was probably prepared by the same authors (Fermi and Szilard) in order to correct several misprints in the previous version. For these reasons, we have reported only the "definitive" second version replacing the

⁵With slight exceptions for Canada patents CA407558A and CA407559A with respect to the first U.S. patent 2206634, where two more claims and two tables appear.

text of the 3rd patent.

Appropriate bibliography, not present in the original documents, have been added at the end of the volume, before the index. Far from being exhaustive, it provides only some references about what touched upon in this introduction or in the specialized introductions preceding each patent.

An introductory note by Prof. A.P. French follows this Preface.

Acknowledgements

We are indebted to Prof. A.P. French for a very helpful exchange of information, his willingness to write the following historical note and, more in general, for his kind encouragement. We are as well very thankful to Dr. A. De Gregorio and Prof. E. Recami for valuable discussions, and to Prof. G. Miele for his kind support and encouragement.

The active and valuable cooperation of the staff of the Churchill Archive Centre, Cambridge (U.K.), the staff of the U.S. Patent and Trademark Office and of the Information Resource Center of the U.S. Embassy in Rome is here gratefully acknowledged.

S. Esposito

O. Pisanti

Naples, March 2010

Some Background to my Notes on Fermi's Neutron Physics Lectures (Los Alamos, 1945)

Introduction

In October, 1939 I entered Cambridge University as a freshman. World War II had recently begun, but the admission of students was not yet affected. I began attending lectures in physics at the Cavendish Laboratory as part of a program of studies called the Natural Sciences Tripos. My subjects besides Physics were Mathematics, Chemistry and Mineralogy. These four subjects (or some other combination of introductory subjects in science) were typically attended during the first two undergraduate years, after which the aim during the third year (Part II of the Tripos) was to concentrate on one subject, in my case physics, and complete a B.A. degree.

As the war proceeded, large numbers of students were recruited into the Armed Forces and into special projects, postponing the completion of their university studies until a later time. My own fate – and that of many others – was somewhat different. I completed my degree program but went into war work later. In my case I was directed to report to my physics tutor Egon Bretscher at the Cavendish Laboratory two weeks after receiving the B.A. He had given an exciting course of lectures about nuclear physics, but of course I had known nothing about the atomic bomb project and his involvement in it.

Tube Alloys

As the world learned later, the discovery of nuclear fission in 1938 was followed by intensive research in a number of laboratories on both sides of the conflict. In Britain the leading figures were Otto Frisch (co-discoverer of fission) and Rudolf Peierls (a senior theoretical physicist at the University of

Birmingham). Together they produced, in 1940, a seminal memorandum on the possibility of making a nuclear fission bomb. Using the available nuclear data, they made a quantitative estimate that a few kg of U^{235} should be sufficient. To separate this from the much more abundant isotope U^{238} would be a monstrous task. But the possibilities were judged to be sufficiently good to justify setting up a British atomic bomb project. It was given the code name “Tube Alloys” and was set up with headquarters in London. The staff for the project included most of the nuclear scientists working at universities in Britain and Canada, plus some major industrial participants. The chief academic partner was the Cavendish Laboratory, where James Chadwick discovered the neutron, and where Cockcroft and Walton had achieved the first artificial nuclear reactions in 1932.

The project had two main groups. One of them (in which my former tutor Egon Bretscher was a leader) was concerned with atomic bomb design, by producing fission in U^{235} by fast neutrons with bombarding energies of 1 MeV or so. The other group (slow neutron physics) was concerned with designing a nuclear reactor using a “pile” of U^{238} to generate power and neutrons in a large mass of ordinary uranium. Fermi led the team that succeeded in making the world’s first chain-reacting pile in December 1942 at the University of Chicago.

The Manhattan Project

Much of the preceding section is familiar history, but it gained special significance later in the war. Britain had undoubtedly taken the lead initially, but the United States, with its size and immense resources, was ready and able to join the effort. It should be remembered, however, that the USA did not actually join the war until after the attack on Pearl Harbor in December, 1941. There had been intense consultation between Britain and the USA well before this, but the USA was unready or unable to become an actual combatant. Once the USA had joined the struggle, a formal partnership became possible. It was not all plain sailing, however. Specific national interests were involved. Valuable meetings between British and American scientists took place, but the going became rough at times. A crucial meeting between Roosevelt and Churchill took place in Quebec in 1943. It was recognized that it was impossible, in the conditions of enemy bombing, food rationing and a general shortage of supplies, to continue with the development of the atomic bomb in Britain. A new project – the Manhattan Project – was launched in the USA. This became an immense

venture, including all the industrial-scale plants needed for separation of U^{235} and plutonium. But even the basic research in nuclear physics was terminated in Britain. The work in slow-neutron physics toward the design of nuclear reactors was transferred to Canada, and the basic research on fast-neutron fission (for the design of the atomic bomb itself) went to a newly created laboratory at Los Alamos, New Mexico.

Once the Manhattan Project had been established, it outstripped anything that could have been achieved in Britain. It became the most powerful (and expensive!) organization for research and development in atomic science that had ever been created. Only in the circumstances of the world war could it have been done. It benefited from the participation of some of the best scientists in the world who had left Europe as refugees from Nazi and Fascist oppression. One of these was Enrico Fermi, who fled from Italy with his family after receiving the Nobel Prize in physics in 1938.

The British Mission

A special British mission to Los Alamos was created under the leadership of James (later Sir James) Chadwick. This mission contained a little over 20 members, who were attached to specific parts of the atomic bomb project, including explosives. The most prestigious member of the British Mission was Niels Bohr, but there were half a dozen junior assistants such as myself. Thus it was that I found myself traveling to Los Alamos in October, 1944 to join Bretscher, who had preceded me by a few months. (To the best of my knowledge I am the only surviving member of that group.) I found that Bretscher himself was no longer doing research related to the bomb; he had joined forces with Edward Teller, who since the beginning of the Manhattan Project had pushed for working on the theory of thermonuclear reactions of the lightest nuclei, with the aim of creating a bomb called "the super". But Teller and his associates were all theorists, so it was appropriate for Bretscher to form a very small experimental research group, which he did, using two of us (M.J. Poole and A.P. French, junior staff from the British Mission) together with two American assistants. We built a miniature accelerator to provide up to 100 kV or so to study nuclear collisions of hydrogen isotopes, suitable at energies for thermonuclear reactions. It was reminiscent of the apparatus that Cockcroft and Walton had constructed and used in their pioneering researches in 1932.

The Special Engineering Detachment

An important component of the scientific work force at Los Alamos was the Special Engineering Detachment (S.E.D.) made up of young American university students with scientific and technical skills aspiring to attend graduate school after the war. Their total number, I believe, was of the order of 1000. They were attached to research groups in all branches of the project. The two assistants in Bretscher's group were S.E.D.s. Many of S.E.D.s became successful scientists and engineers in their own right after the war. One such assistant (Val Fitch) won a Nobel Prize in Physics later in his career.

"Los Alamos University"

The dropping of the two atomic bombs on Japan marked the end of the short-term goals of the Manhattan Project, but there were many ongoing projects worth continuing. Although many of the senior Los Alamos research staff were eager to return to their home institutions, there were many others glad to continue their research projects at Los Alamos. There was another significant factor. The large numbers of S.E.D.s were subject to military discipline and were not necessarily free to leave Los Alamos at will. It was natural to take advantage of this situation by setting up lecture courses in which the junior people could receive education from older experts. This opportunity was informally called "Los Alamos University". Students could sign up for two courses and have the possibility of receiving academic credit from a chosen college for doing so. I believe that about a dozen different courses were offered, taught by people some of whom were world experts.

The existence of this scheme was too good to miss, and I sought permission to enroll although I was not specifically a target of the program. The courses I chose were Electromagnetism, taught by Hans Bethe, and Neutron Physics, taught by Enrico Fermi. Both men were outstanding as physicists, but there was no doubt concerning my own preference as to subject matter.

Concluding Remarks

I left Los Alamos at the end of August, 1945. Before doing so I mailed a package of my personal Los Alamos books and records addressed to myself at the Cavendish Laboratory. However, I did not return directly to Cam-

bridge until 1948. Like several other people who had been at Los Alamos, I went to the newly established Atomic Energy Research Establishment (A.E.R.E.) at Harwell, England, where Egon Bretscher became head of the Physics research division and remained until his retirement. I do not think that I wrote up a final copy of my notes on Fermi's lectures until after my return to the U.K. I may have sent a set of the notes to the London office of Tube Alloys, but I have no recollection of sending a copy of them directly to Chadwick (although I may have done so). It would have been natural for Chadwick to receive the notes from the Tube Alloys headquarters, as he had served as head of the British mission, and it is no surprise that the notes were found in Chadwick's papers many years later by Dr. Esposito. My own copy of the notes slept in my personal files until I was reminded of its existence when Dr. Esposito first wrote to me in 2008.

Anthony P. French