The first of the additional laws is known as the "conservation of heaviness"; it requires the appearance of a mass of nucleonic order for each one that disappears. Such a law is needed for understanding many elementary particle interactions besides the interfermionic ones.

The second new conservation law which seems to be needed is[19]: in interfermionic processes, *a pair of fermions must be created for every pair destroyed.* Of course, the appearance of an antiparticle is to be understood as the disappearance of a normal one, from a negative energy state. With the help of this, one can prevent the occurrence of all the conceivable processes which are not observed. It is only necessary to presume the positive muon to be the normal particle, the negative muon the antiparticle.

Without the hypotheses here described it would be difficult to understand the nonoccurrence of (a) $P+\mu^-\rightarrow P+\epsilon^-$ when both (b) $P+\mu^-\rightarrow N+\nu$ and (c) $P+\epsilon^-\rightarrow N+\nu$ do occur. When the hypotheses are adopted, (b) must be understood as $P+\mu^-\rightarrow N+\nu'$ ($\nu'\equiv$antineutrino) and (a) would require the appearance of three normal fermions from the absorption of one, in violation of the last conservation law. The hypotheses also establish the unambiguous 'order' for the μ-decay implied by $\mu^+ + \epsilon^-\rightarrow\nu+\nu$; the emitted neutrinos must be identical, one cannot be an antineutrino.

The considerations introduced here are highly speculative and probably do not warrant the attention given them here. However, they well illustrate how the ideas created by Enrico Fermi continue to ramify. They emphasize how much physicists owe to him.

Dr. Bethe:

Thank you very much Dr. Konopinski.

The two lectures that you have heard so far have illustrated how two rather small papers of Fermi's have had tremendous influence on the progress of physics. The three papers which you are going to hear now are of a somewhat different character in that they tell directly about the work of Fermi himself, in the last twenty-odd years, by some of the close collaborators in this work. The paper by Fermi that Dr. Konopinski talked about, the theory of beta disintegration, was the last paper which Fermi wrote as a pure theoretical physicist. Around that time in the early 1930's it was clear to most nuclear theorists that no progress could be made in nuclear theory without very many more experimental data. Most of us just left it at that and left to the experimental physicists the difficult task of providing the experimental data. Fermi came to the logical conclusion, that he should become an experimental physicist himself, and thus he provided the data on which the theory could be built. The next talk that you will hear will tell of the work of the nuclear physics group in Rome in the middle 1930's by one of Fermi's closest collaborators, Professor Segrè.

REVIEWS OF MODERN PHYSICS VOLUME 27, NUMBER 3 JULY, 1955

Fermi and Neutron Physics

EMILIO SEGRÈ

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FELLOW members of the Physical Society, Ladies and Gentlemen.

I have been requested to speak to you about the early work on neutrons performed in Rome in 1934 and 1935. There is obviously not very much that I could tell about it which would be of scientific interest, and even the official history of that period has in a certain way been written by Fermi himself in the Nobel lecture of 1938.[1] What I will try to do is to recapture, as much as possible, some of the atmosphere and the spirit that prevailed at that time among the people involved in the work. For this reason these recollections will be somewhat personal and I beg to be excused for it.

The main activity of Fermi until 1934 had been theoretical, and it had been very successful having already accomplished, among other things, the discovery of the statistical laws governing the antisymmetrical particles, that is, the famous Fermi statistics, and the beta-ray theory. But, in spite of his theoretical activity, Fermi liked to make experiments, once in a while, and in this he had been associated mainly with Rasetti. The principal topics of experimental research were spectroscopic and optical because these were the fields which were at the forefront of the attention of physicists in the late 1920's and early 1930's.

After the formation of the school in Rome, and in view of the fact that several of us were interested, not only in theory, but prevalently in experimental physics,

[1] E. Fermi, *Les prix Nobel* (1938).

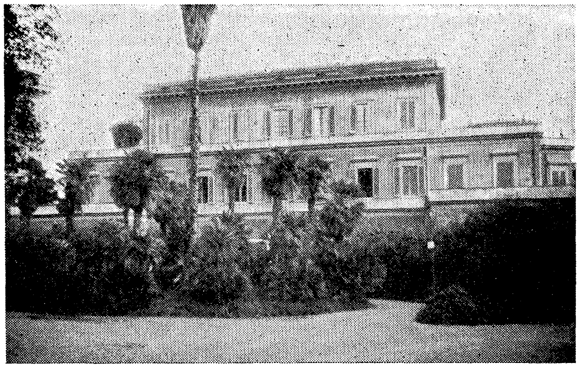

FIG. 1. Physics building in Rome where the neutron work was done.

we often had discussions on what experimental fields looked most promising. Although we had good spectroscopic equipment and a good knowledge of spectroscopic techniques, it had for several years been the feeling, mainly of Fermi, that we should branch out and go into nuclear physics because that was the field which promised to become most interesting in the future. (See Fig. 1.)

However, not being quite sure about this, we went to several laboratories to learn techniques. Among the laboratories visited at length were those of Millikan in Pasadena, Stern in Hamburg, Debye in Leipzig, and Lise Meitner in Berlin. Several of us spent a year or more in these laboratories and finally we all went home importing techniques and discussing what we had seen. After a somewhat long and heated debate in which different opinions were maintained (I remember vividly a scene in a locker room of a tennis court) with plenty of vigor, it was decided, mainly under the impulse of Fermi, that we should really branch into the nuclear field. For this Rasetti, who had learned techniques at Dahlem from Lise Meitner, started to prepare quite a few pieces of apparatus such as a cloud chamber and a gamma-ray spectrograph. However, while the equipment was being prepared and we were trying to decide what type of work to actually start, Amaldi and I continued spectroscopic work.

The real decision occured in 1934 when we read in the *Comptes Rendus*[2] and in *Nature* of the discovery of artificial radioactivity by Curie and Joliot. It became apparent to Fermi that this was just the occasion for which we had been waiting and immediately he saw the possibility of expanding the work tremendously by using neutrons as projectiles. The start of the experiments was facilitated by the very lucky circumstance that Professor Trabacchi had on hand a gram of radium and a plant with which it was possible to extract emanation and prepare radon-beryllium sources. We did not know at that time how dangerous it was to work with beryllium and we have been fortunate that up to now none of us has shown serious signs of berylliosis, although at that time we probably gave the disease many chances to develop. It was in the spring of 1934, during Easter vacation, that Fermi decided to put his idea to the experimental test immediately; he built with his own hands some primitive Geiger-Müeller counters of aluminum (see Fig. 2) which looked very ugly but worked adequately for the purpose and then started to bombard with radon plus beryllium neutrons (50 millicuries of radon was all that he had at the beginning) all the substances he could get. Being a systematic man, as most of you probably know, he tried in order hydrogen (helium was not available), lithium, beryllium, boron, carbon, nitrogen, oxygen, all with negative results; but since he *knew for sure* that the phenomenon was going to happen, he kept on trying and finally fluorine gave the expected result.

This was on March 25, 1934 and a letter announcing this result was promptly sent to the *Ricerca Scientifica*.[3] The *Ricerca Scientifica* is a general science journal similar to *Nature* or *Science* and the editors of it would give us reprints of the articles within a very few days after receiving the manuscript. These reprints were the bulletins of our work which were sent at that time to a mailing list comprised of most of the nuclear laboratories in Europe and America. The first paper was signed by Fermi alone and contained the initial discovery.

Having seen that he had struck gold, Fermi, who wanted to proceed as fast as possible and was quite unselfish with respect to the work, asked Amaldi and me to help him carry out the experiments. Rasetti was in Morocco getting decorated by the Sultan. D'Agostino was not originally in our group; he was a chemist in the laboratory of the Sanità and at the time had a fellowship in Paris at the Laboratory of Madame Curie. We wired Rasetti, asking him to come back and in the meantime Fermi, Amaldi, and I tried to push the work as fast as we could. We organized our activities in this way: Fermi would do a good part of the experiments and the calculations, Amaldi would take care of what we

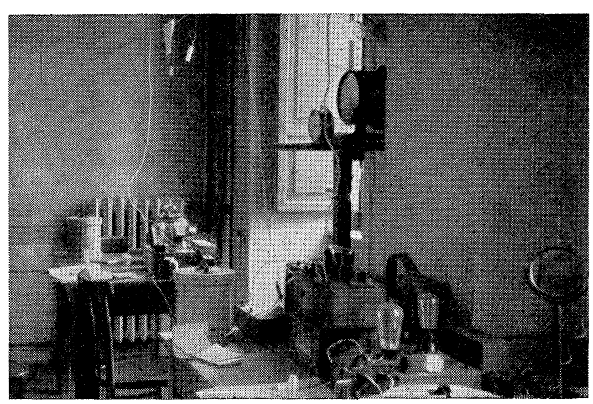

FIG. 2. The Geiger-Müller counter and lead houses used in the first year of the neutron work.

[2] I. Curie and F. Joliot, Compt. rend. **198**, 254, 561 (1934).

[3] E. Fermi, Ricerca sci. **5** (1) 283 (1934).

would now call the electronics, and I would secure the substances to be irradiated, the sources, etc. Now, of course, this division of labor was by no means rigid and we all participated in all phases of the work, but we had a certain division of responsibility along these lines and we proceeded at great speed. We needed all the help we could get and we even enlisted the help of a younger brother of one of our students (probably 12 years old) persuading him that it was most interesting and important that he should prepare some neat paper cylinders in which we irradiated our stuff. It was also apparent very soon that chemistry was an essential tool for the work and we were debating on how to get the help of a professional chemist when the door opened and Dr. D'Agostino entered. We barely knew him at the time, but soon we became excellent friends. He had a return ticket which was extended three times and finally expired (European railroad tickets are not reimbursable) and he did not return to finish his fellowship in Paris.

The first thing we decided to do was the obvious: irradiate all the substances we could lay our hands on. For this we needed a little money. A phone call by Fermi to the National Research Council got us about \$1000 with no strings attached (of any kind). I became the cashier without any obligation of keeping books. With some of this money in my pocket and a basket I went as a procuring agent to an old chemical shop in Rome which I knew. I found there a gentleman by the name of Troccoli who would speak Latin because he had been educated in a seminary for priests in the province of Rome and I explained the situation to him. I was very pleased and almost moved to see how enthusiastically he collaborated to the extent of even giving to me some of the chemicals which he had kept on his shelves for twenty or thirty years. He commented that nobody had ever asked for them and he was glad to see them used. Another firm which was selling precious metals would give us quite freely on loan any amount of gold (once 10 Kg) or rhodium or platinum that they could supply, and with a display of confidence and real enthusiasm for this scientific work which had started in the University of Rome which we deeply appreciated.

The irradiations went on quite regularly and soon we identified what in present-day language are called (n,p) (n,α) reactions. We also found that in many cases a neutron would produce a radioactive isotope of the target and there was great doubt among us whether this would be the result of an $(n,2n)$ or (n,γ) reaction. It must be remembered that at that time we still had the thought that the more energetic the neutrons the more effective they would be in producing reactions. At the beginning of May, while all this was going on, Rasetti had come back from Morocco. He joined us and he proposed to irradiate uranium in order to see what would happen. This was accomplished with results that puzzled us very much at first and which remained unexplained until the discovery of fission. Our main results were that irradiated uranium produced elements which were of an atomic number not found between that of lead and that of uranium. In this we were perfectly correct and we had tested our experiments carefully with isotopic tracers. Our further conclusion was that the substances thus produced were transuranic. In this we were only to a small extent correct because we were completely blind to the possibility of fission, although, remarkably this was called specifically to our attention by Ida Noddack who sent to us an article in which she clearly predicted the possibility of fission.[4]

With all this work the sholastic year 1934 came to an end. I had been keeping the first isotope chart of my life (a much simpler job then than now) and could proudly point to about 40 red spots—our new radioactive isotopes. Senator Corbino, the director of the physics laboratory and also our close friend and adviser, took care that the results be properly advertised at the Italian Accademia dei Lincei in the solemn royal session.

I have reread that speech[5] because it is a good reflection of our thoughts at the time, expressed in a beautiful literary style.

After having discussed the transmutation reactions he recalls a previous speech of his given to an International Nuclear Conference in Rome in 1931 in which he pointed out that in the stars, matter which is old and as it were, dead on our planet, is instead still young and in rapid evolution. He then considered, in 1931, the possibility of "rejuvenating" matter on the earth, as he put it.

In 1934 he sees this dream realized although on a microscopic scale, but he underrates the technical possibilities of the future. He says, "The scale of the transmutation is too insignificant yet to have any practical application." (This was in 1934, in 1935 he had already changed his mind, as you shall see.)

And finally he closes on a somber note: "It is perhaps not in vain that providence has limited these phenomena. Mankind is not yet worthy of having such terrific sources of power and destruction" and he continues "whoever has any humanitarian senses cannot think with indifference as to the character that war would acquire, if nuclear weapons became possible."

The Royal Meeting of the Accademia dei Lincei was the official end of the season in Rome. As you know the summer there is hot and at the end of July we all left for a summer vacation. Fermi and his wife went to South America, Amaldi and I went to Cambridge, England, but I do not remember where Rasetti and D'Agostino went (see Fig. 3). To Cambridge we brought the manuscript of a paper summarizing all our results which we wanted to have published in the *Proceedings of the Royal Society*,[6] and I delivered it to the hands of Lord Rutherford in person. He had already shown great

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[4] I. Noddack, Angew. Chem. **47**, 653 (1934).
[5] O. M. Corbino, Ricerca sci. **5** (1) 609 (1934).
[6] Fermi, Amaldi, D'Agostino, Rasetti, and Segrè, Proc. Roy. Soc. (London) **146**, 483 (1934).

interest in our results, and he corrected our English. When I expressed our wish to have a speedy publication he said, "What do you think, I am President of the Royal Society for?" and indeed, the paper came out within about six weeks.

In Cambridge during that summer Bjerge and Westcott,[7] with our help, tackled the question of the (n,γ) *versus* $(n,2n)$ reaction and found an example which we thought was very clear cut of an (n,γ) reaction. (Indeed the result is correct, but it is based on an erroneous assignment of the period of Na^{22}.)

At the end of the summer, Amaldi and I went back to Rome and Fermi, on his way back from South America, went to England to a conference on nuclear physics that was held at that time.[8] While Fermi was at the conference Amaldi and I, always trying to settle this question of (n,γ) *versus* $(n,2n)$ reactions, irradiated other substances besides the one used by Bjerge and Westcott in order to find more examples of the (n,γ) reaction and we thought we had found a nice example in aluminum which we duly communicated to Fermi. He referred to this at the meeting in England. Soon thereafter I caught a cold and could not go to the laboratory for a few days. Amaldi tried to verify our results and found a different decay period for irradiated aluminum which showed that our so-called (n,γ) reaction did not occur. This was hurriedly communicated to Fermi and the result was that we got a real strong reprimand for having induced him to say things that were not true at the famous London conference. This whole business was in the process of making serious trouble because I was sure of my results, so was Amaldi but we could hardly reproduce them consistently. Fermi thought that none of us were reliable. At that time we were joined by our fresh Ph.D., B. Pontecorvo. It was now the beginning of the scholastic seasons 1934–1935 and we again started our systematic irradiations. Doing things a little more quantitatively it became quite apparent that the intensity of the radioactivity that we could obtain from various substances depended in a strange way on the conditions of irradiation and that there were unexplainable irregularities in the intensity. In particular there was a table near a spectroscope which had miraculous properties in as much as silver (there were still silver coins in Italy at the time) irradiated on that table gained more activity than when irradiated in other parts of the same room. The explanation was that the table was of wood and that the source on the wood gave slow neutrons whereas in other parts of the room the tables were of marble and the source on marble did not give slow neutrons. But all this was then unknown to us and these puzzling results baffled us for several days, or perhaps a week or two, until we decided to try to filter the radiation that produced the artificial radioactivity. By that time we were not even quite sure they were neutrons anymore. In filtering this radiation we decided to filter with light elements or heavy elements and the first filter tried was paraffin. I remember that I was giving an examination and all of a sudden everybody from the group came rushing into the room telling me to run upstairs to the counters to see what had happened. I entered the room and my first reaction was that a counter had gone bad, as frequently happened at that time (that is, it was discharging continously) and that there was nothing to be very excited about. But, I soon changed my mind when it was explained to me that the counter was not bad but simply was acted on by some substance that had been irradiated with filtered neutrons. This was around noon and we tried a few more substances and saw that the filtration, the powerful filtration, occurred only with paraffin. We went to lunch extremely puzzled by this fact and came back after a siesta as usual around three o'clock to find that Fermi, in the meantime, had had a hunch that what possibly could make this strange behavior of the neutrons was the fact that they could be slowed down by collisions and become more effective. If this were the case . . . we all started to shout with our loud Italian voices listing possible consequences and how to test them by experiments. I jumped to my old favorite, the (n,γ) *versus* $(n,2n)$ reaction, because I was still burned up by the alleged wrongness of our results and immediately tried to see whether by filtering the neutrons with paraffin the reactions (n,γ) or (n,p) or (n,α) could be effected differently. In about half an hour we had the explanation of the disagreement between Amaldi and myself. Both were vindicated and by now we even knew the explanation of our previous results and could produce at will short or long periods by bombarding aluminum. Furthermore, we had no need to correct the minutes of the London conference where Fermi had said certainly more than half of the truth and no lies.

We also tested that the radiation was emitted by radium plus beryllium and not by radium alone, and that the effect of the paraffin was characteristic of hydrogen and not of carbon. Finally, we went all the way to think that the neutrons could really be thermalized and instituted in the same day an experiment (unsuccessfull at the time) to demonstrate this fact by slowing the neutrons in hot water instead of cold water. All this happened on October 22 of 1934 and by the evening of that day a short letter to the *Ricerca Scientifica* telling all of these miraculous effects was on its way. Actually in order to write this letter we had to break our habits and write it after dinner. This was done at Amaldi's house.[9] This discovery obviously opened a host of problems and the first thing that we did was to measure for many substances what we called the "coefficient of acquaticity," namely, how much the immersion in water would increase the activity. This gave us a confirmation that the (n,γ) reactions were the

[7] T. Bjerge and H. C. Westcott, Nature **134**, 286 (1934).

[8] *Papers and Discussions at the International Conference in Physics* (Cambridge University Press, London, 1934), Vol. I, p. 75.

[9] Fermi, Amaldi, Pontecorvo, Rasetti, and Segrè, Ricerca sci. **5** (2) 282 (1934).

only ones sensitive to hydrogenated substances and by November 7 we were fairly convinced of the correctness of the explanation.[10] Attention turned then more to the study on the slow neutrons themselves and we tried again to see whether slowing down in a hot or cold medium would change the properties of the neutrons. We had strong suspicions that the neutrons were effectively thermalized and although we could not, at first, show any positive effect (this was shown for the first time by Moon and Tillman[11] in England), we kept trying this theme. We also found very soon that some substances, for instance, cadmium, absorbed slow neutrons very strongly and we measured crude cross sections for this effect. We detected the capture gamma rays and we also started crude measurements of the density of the slow neutrons in a hydrogenous medium as a function of the distance from the source. Finally, we tried to slow neutrons down by collisions with substances different from hydrogen and found some slight effects of inelastic collisions. All this work was initiated and had given significant results by December 6 of 1934, that is about six weeks after the discovery of the slow neutrons themselves. Approximately at this time, Corbino came to us with the suggestion that discoveries or inventions of the type we had made by now might have important practical applications and that we should take a patent. This we did, and it is this patent that two years ago was acquired by the A.E.C., after a long and complicated story.

The boron reaction was studied immediately afterwards and interpreted correctly as (n,α). More systematic work followed in line with the previous problems and with considerable emphasis on the uranium irradiation, on which, however, we did not make great progress. The main results of this work were summarized in a paper for the Royal Society[12] written in February, 1935. In it there are the seeds of most of the important ideas and facts of neutron physics. One serious difficulty was the apparent lack of correlation between the scattering and capture cross section in cases as e.g. Cd. What we did miss was Bohr's idea of the compound nucleus. The concept of scattering length and the possibility of resonances were present in Fermi's mind. Actually the scattering length argument with its typical diagram was stock in trade for him since 1933 having invented it in order to explain some spectroscopic facts (pressure shift of spectral lines) which Amaldi and I had discovered experimentally. At this time, Fermi also made a number of calculations on the behavior of neutrons in hydrogenous media by what we would call today the Monte Carlo Method; he did not publish them and I learned of it only much later, after the war.

[10] Fermi, Pontecorvo, and Rasetti, Ricerca sci. **5** (2) 380 (1934); Amaldi, D'Agostino, and Segrè, Ricerca sci. **5** (2) 381 (1934).
[11] P. B. Moon and J. R. Tillman, Nature **135**, 904 (1935); Proc. Roy. Soc. (London) **A153**, 476 (1936).
[12] Amaldi, D'Agostino, Fermi, Pontecorvo, Rasetti, and Segrè, Proc. Roy. Soc. (London) **149**, 522 (1935).

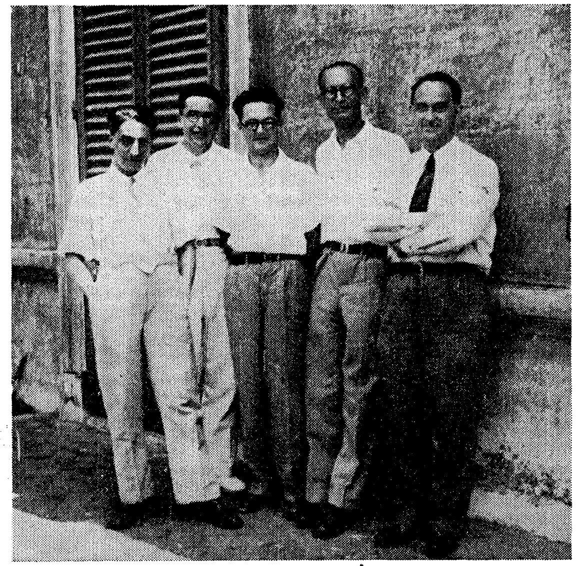

FIG. 3. A group taken in early summer 1934. From left to right, D'Agostino, Segrè, Amaldi, Rasetti, Fermi.

Finally, in the spring of 1935 we devised a mechanical experiment in which one could compare the velocity attained by the neutrons with the mechanical velocity of a wheel, and soon after the experiments of Moon and Tillman showing the influence of the temperature, we could not only confirm their result but also measure by a mechanical experiment some data relevant to the velocity of the neutrons. With this we had reached the summer of 1935 and as usual we stopped the work and scattered pretty much all over the world. (See Fig. 4.) I do not remember exactly where Fermi went at that time. I came to the United States and spent some time at Columbia University perfecting the velocity selector and some more mechanical experiments together with the staff of Columbia and Rasetti. However, the political developments of the time, namely the Ethiopian War and the very grave deterioration of the situation in Europe had practically affected very seriously our work even in the last months of 1935. There was a famous atlas in the library of the physics department in Rome which automatically opened on the page of Ethiopia because everybody was poring over it and the worries connected with that unhappy campaign and with the growth of Nazism affected the peace of mind even of the imperturbable Fermi.

When the fall of 1935 came and we should have gone back to Rome, Rasetti did not want to re-enter Italy. I had been appointed director of the physics laboratory at Palermo, Pontecorvo had left Italy for France, D'Agostino had taken employment in another scientific institute, and only Amaldi and Fermi were left in Rome with neutron sources. From Palermo I went quite often to Rome in 1936 to see my old friends and work a little there. The spirit of the previous years had really some-

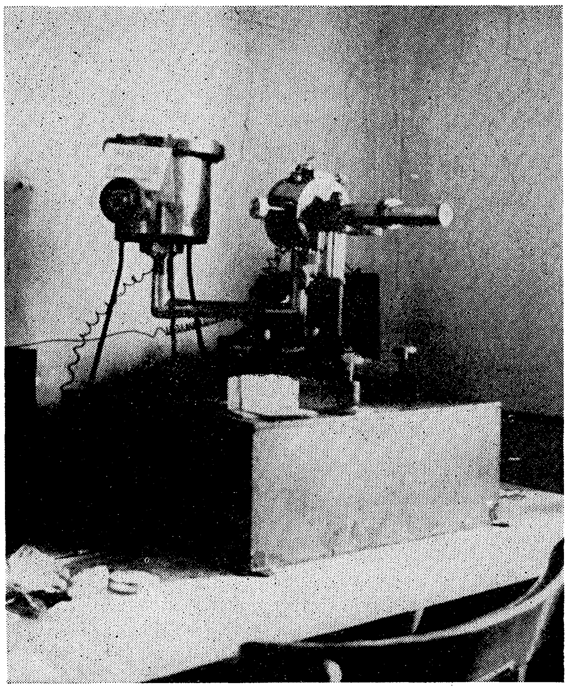

FIG. 4. Ionization chamber and electroscope used in Rome in the second year of the neutron work when the hydrogen effect made stronger activities available. Several chambers similar to this were then built in U.S.A. laboratories which we visited at the time and were jocosely called "The Roman Sign."

what disappeared because the discoveries were not so frequent anymore and moreover the physical conditions of the work were very bad. I remember Amaldi and Fermi locked in rooms from which no light should filter out because of the anti-air-raid regulations of the time. They were working very hard on what has become the famous study of the motion of the neutrons in hydrogenated substances.[13,14] The so-called groups of neutrons were found and a beautiful piece of theoretical work was developed by Fermi, probably leaning on the Monte Carlo calculations mentioned previously, to find his way to the correct approximations. This laid the foundation of the slowing-down theory and the subsequent development of slow neutron diffusion theory. I think it was at this time that Fermi acquired, by the combined means of empirical experience, Monte Carlo calculation, and more formal theory, that extraordinary feeling for the behavior of slow neutrons which marked him for the development of the pile. He told me that he did this theoretical work at home from four o'clock to eight o'clock A.M. before coming to the laboratory.

I have had occasion to reread these papers and also some of the wartime reports preceding the pile. It is still stunning how he could always find the correct and simple way to everything, guessing the essential points. At the end he obviously had a knowledge of the behavior of slow neutrons which has probably not been equalled.

It is difficult at such a distance of time to recapture the exact spirit prevailing. We were quite sure that the discoveries we had made were of great importance and I remember also that we discussed repeatedly, in a half-serious vein, the possibility of a nuclear explosion. In the case of uranium, in particular, we thought that there might be enough of an $(n,2n)$ reaction to start a chain. For reasons which are not very clear even today, we did not consider seriously the possibility of fission. Fermi, with whom I occasionally discussed this subject, always maintained that he had wrong ideas about the mass defects of the nuclei. We followed very closely the work of Meitner and Hahn and we were well aware of the existence of a very important problem in the uranium irradiations. On the other hand we had wrong ideas on the chemical properties of elements 93 and 94.

Among the experiments performed in Rome I might mention one in which we looked for the "alpha particles" emitted by uranium under neutron bombardment. However we covered our uranium with enough aluminum to stop its natural alpha particles and hence also missed the fission fragments. But who can say whether even Fermi would have recognized them for what they were if he had seen them?

The proportions that nuclear transmutations were to attain in a few years were completely out of our dreams. When we mentioned the possibility of having one curie of an artificial radioactive substance it was only in a jocose mode.

However when the large neutrons' sources furnished by the pile came along, Fermi was ready to use them for the furtherance of pure physics, and his interest in and knowledge of the solid state became very handy. The scattering length measurements, the neutron-electron interaction, the specular reflection, and other features of the diffraction of the neutrons were post-war applications of neutron physics. The speed of these developments staggers the imagination and it seems incredible that only eight years should have elapsed between the discovery of the slow neutrons and the criticality of the pile.

But already at Los Alamos Fermi had the feeling that his next phase of activity would not be in neutrons but in something new and he reminded me that just as in 1934 he discarded all his investments in spectroscopy to go to nuclear work, so now he would leave the slow neutrons in order to proceed to new conquests in the field of high-energy physics. In a half-joking mode he quoted Mussolini: "Rinnovarsi o perire"—"to renew oneself or to perish."

[13] E. Fermi, Ricerca sci. **7** (2) 13 (1936).
[14] E. Fermi, Ricerca sci. **7** (2) 13 (1936); E. Amaldi and E. Fermi, Phys. Rev. **50**, 899 (1936).

Dr. Bethe:

Thank you very much Dr. Segrè. I think I can see from the applause that you all enjoyed the personal flavor of this talk as much as I did.

Segrè has mentioned the puzzle that was posed by the activities induced in uranium by neutrons, and you all know that this puzzle found its solution in the discovery of fission by Hahn and Strassmann in late 1938. You also know that the political situation which Segrè mentioned and which looked bad in 1935 became increasingly bad in the ensuing years; Italy came under the domination of Nazi Germany and Fermi, like Segrè before him, decided to leave Italy for a more hospitable country. You know that Fermi received the Nobel prize of 1938 for the research in neutron physics which you have just heard, and you know that having received the Nobel prize in Sweden, he then took the wrong boat—instead of the boat to Italy he took that to America. We were most fortunate to have him come and work with us here in this country in early 1939, and much of history would have been different if he had not come. Just at the same time that Fermi came to this country, came the news of fission and this news led to very spectacular developments about which you will now hear from Dr. Zinn of the Argonne National Laboratory.

REVIEWS OF MODERN PHYSICS VOLUME 27, NUMBER 3 JULY, 1955

Fermi and Atomic Energy

WALTER H. ZINN

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WE are assembled here today to honor the memory of a great scientist, and a cherished friend. This tribute would be paid to him even if nuclear physics had not brought about the discoveries and events of the past 16 years which have bequeathed to the world the amazing collection of endeavors now included in the term "atomic energy." The discovery of the fission of uranium happened to coincide with the arrival of Enrico Fermi in the United States. The rapid exploitation of this discovery in this country by means of a whole series of brilliant theories and experiments was in a large part inspired by him and, in a large part was the product of the work of his brain and of his hands. He inspired others, and he did so by example.

In relating his contribution to atomic energy, three periods of time may be recognized. The first extended from January, 1939 to May, 1942, which was the period during which Fermi's work was almost entirely at Columbia University. In this period, the group surrounding him was small, never exceeding nine or ten, and the organization for carrying out the work was uncomplicated, perhaps almost nonexistent. The second period extended from May, 1942, to the winter of 1943–1944. In May, 1942, Fermi and the group working with him at Columbia transferred to the Metallurgical Laboratory, at the University of Chicago, which had been specifically created to exploit the discoveries in uranium fission made up to that time. This second period marked the time during which Fermi's work was mainly directed toward bringing about the self-sustaining chain reaction and the construction and operation of the production piles at the Hanford Engineer Works of the Manhattan District. In May of 1944, his work was transferred entirely to the Los Alamos Laboratory in New Mexico. In July and August of 1945, the atomic bombs were detonated, thus bringing to a close Fermi's immediate participation in the development of weapons. For several years after the war, having returned to Chicago, he made some investigations using piles as the source of neutrons but his main interest rapidly shifted to other fields

As has been related in the Smyth report, that part of the Manhattan District which was called the Plutonium Project involved many people in a sizable number of organizations. In the time available, it is impossible to relate all the ways in which the work of Fermi and the suggestions he made and the inspiration he gave to others contributed to the rapid progress of the project. Neither can the help received by Fermi from a devoted group of colleagues and assistants be recognized properly. It is enough to note that Fermi always was generous in acknowledging the contributions of others and the help he received from his assistants.

The list of reports written by Fermi and his collaborators is long and only a few can be selected for mention here. These will emphasize results which applied directly to the objective of the project and are chosen with the full knowledge that much splendid physics research thereby is neglected.

In a conversation I had with him early in the month of January, 1939, he expressed the opinion that prompt neutrons should be expected to be emitted in the fission process; that the way to a chain reaction would be opened if it could be shown that the number emitted was greater than the number absorbed. An experiment to find such prompt neutrons, if they existed, was

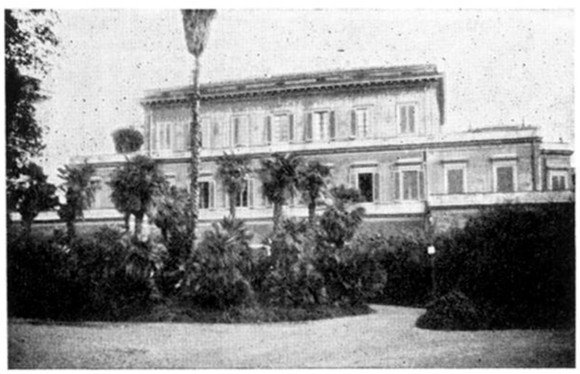

FIG. 1. Physics building in Rome where the neutron work was done.

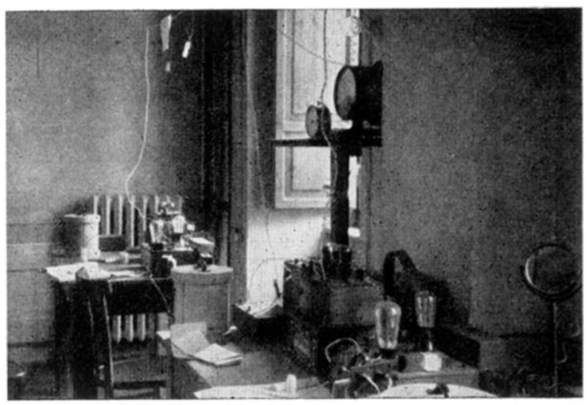

FIG. 2. The Geiger-Müller counter and lead houses used in the first year of the neutron work.

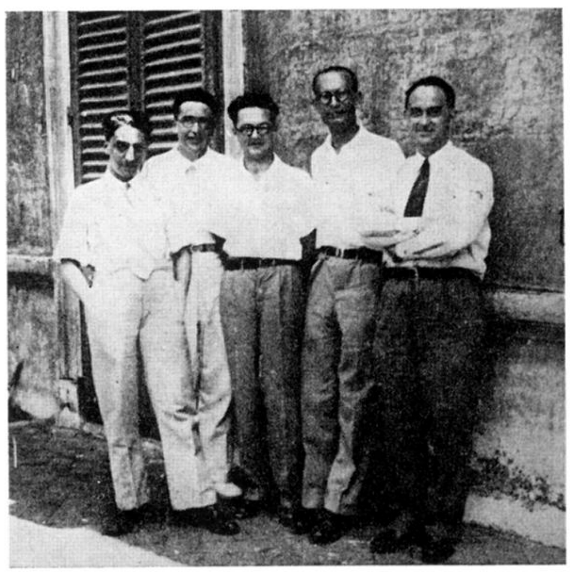

FIG. 3. A group taken in early summer 1934. From left to right, D'Agostino, Segrè, Amaldi, Rasetti, Fermi.

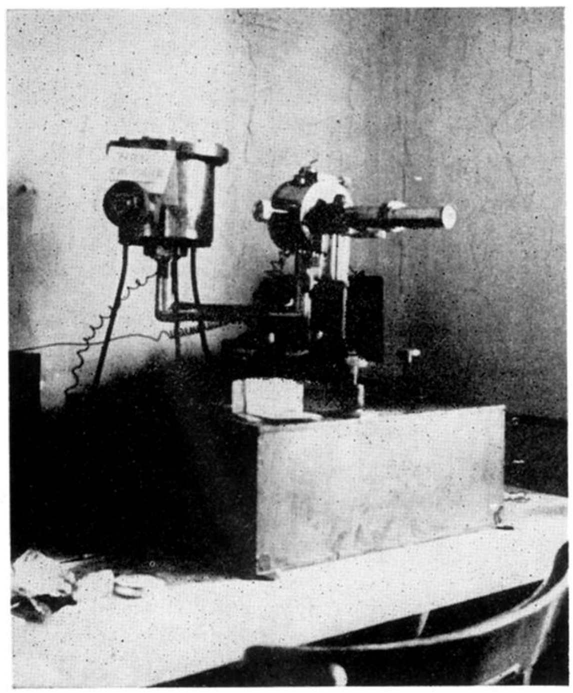

FIG. 4. Ionization chamber and electroscope used in Rome in the second year of the neutron work when the hydrogen effect made stronger activities available. Several chambers similar to this were then built in U.S.A. laboratories which we visited at the time and were jocosely called "The Roman Sign."