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

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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 immediately started and in a short time a favorable result was obtained. Other groups in this country and abroad also obtained and published this result, although there was by no means agreement as to the number of prompt neutrons emitted per fission in uranium. With the knowledge of this result it can be said, however, that the Plutonium Project was born. Through the efforts of Fermi and others associated with him, the Government of the United States was approached in a serious way and was made aware of the consequences which could follow. The story of the approach to President Roosevelt has been told many times and will not be repeated here. Meanwhile, Fermi pushed on with experiments and theoretical analyses. The question of the production of neutrons in uranium when bombarded by neutrons was the subject of several further experiments which were reported in 1939 in *The Physical Review*. In these experiments his collaborators were Anderson, Szilard, and Hanstein. Meanwhile, with Booth, Dunning, Glasoe, and Slack, Fermi reported experiments on the properties of the fission fragments and evidence was presented to confirm the prediction that U^{235} was the so-called fissionable isotope of uranium. At this point in following Fermi's work a comprehensive picture no longer can be obtained from the published literature. The reports became classified and some have remained so although they no longer contain material which requires classification. In parallel with these experiments in the period 1939 and 1940, Fermi began to work on a chain-reacting system based on uranium and graphite. Sometimes alone and sometimes in consultation and collaboration with Leo Szilard, Eugene Wigner, and others, the theory of the chain reaction in a lattice of uranium and graphite was evolved. The theory of the slowing down of neutrons in a moderator was refined and the concept of "Fermi Age" introduced. The advantage of lumping the uranium and the role of resonance absorption was outlined. In a characteristic manner, Fermi identified the various factors upon which information would have to be obtained in order to assess the feasibility of a chain-reacting unit. Some of the experiments were initiated directly by Fermi at Columbia and some, concerning resonance absorption in lumps or spheres of uranium, were undertaken at Princeton University under the direction of E. P. Wigner. Details of all the experiments which were undertaken in this period cannot be related, but it is significant to note that by midsummer of 1940 experiments on the slowing down of neutrons by graphite and the absorption of neutrons by graphite were underway at Columbia. By the spring of 1941, it is my recollection that Fermi gave lectures to the group working with him in which the essential aspects of the chain-reaction theory, as we know it now, were outlined. The term "reproduction factor" was used and the formula $k=\epsilon\eta pf$ had been derived.

The summer of 1941 marked the time when the first large-scale experiment was attempted. Money had been made available by the Federal Government and the purchase of graphite and uranium of a quality suitable for nuclear work, or what was thought to be suitable for nuclear work, had been accomplished. This intermediate pile experiment, as it was called, was the first of what later became known as the exponential experiment. The principal component was a rectangular prism of graphite, 8 feet on a side and 11 feet tall. Eight tons of uranium were to be incorporated in the structure in lattice form. Various practical problems had to be faced and a closer look at the way in which the work was carried on will give an insight into the manner in which Enrico Fermi set about an important measurement and the manner in which he inspired his co-workers to strenuous effort. Fermi entered into all parts of the work with enthusiasm. His meticulous attention to the details of the experiment was always coupled with a complete awareness of the factors which might affect the ultimate result. At that time in 1941, the situation with respect to the cross sections for neutron absorption in uranium and graphite was such that the real values were always in danger of being obscured by the absorption due to unknown impurities. Fermi, especially, was concerned about the effect of entrained moisture and the effect on the result of the experiment of whatever provision was made to contain the powdered U^{238}, which was the only form in which a sizable amount of uranium could be obtained at that time. Once the materials were on hand, he spent long days in the mechanical task of erecting the exponential structure. The uranium oxide was heated to several hundred degrees centigrade and then placed in thin sheet steel cubical boxes, 288 of them 8 inches on the side, and while still hot the containers were soldered to guarantee the exclusion of moisture. Fermi insisted on doing his share of the filling and sealing of the boxes. In the erection of the graphite column, slots had to be cut for the subsequent insertion of foils for the measurement of neutron intensity. Fermi did not hesitate to push graphite blocks through a bench saw while saw and man disappeared in a cloud of graphite dust. As is well known, the exponential experiment requires a high-precision measurement of neutron flux throughout the lattice arrangement. A program of measurement of the activation of indium foils was developed which used a number of Geiger counters. By suitable standardization and careful control of the measuring techniques, high-precision results were obtained despite the well-merited poor reputation of the Geiger counter as a precision instrument. This first exponential experiment gave the result that the reproduction factor for the particular structure was 0.87. This result was disappointing and the conclusion quickly was reached that this result was not consistent with the cross sections as they were then known. Experiments were set underway to find the source of the impurities which caused the difference between the results observed and what was expected from the cross sections. The iron sheet of which the containing boxes were made was tested by neutron absorption. Experiments on the resonance neutron distribution in the neighborhood of a single cell of the lattice were undertaken. The purity of the uranium was questioned. Fermi took the lead in separating the impurities by an ether solvent extraction method. The intention was to collect sufficient of the impurity material to make a determination by the absorption of neutrons of the poisoning effect. This was done and Fermi did not fail to do his share of the time-consuming and laborious work involved in dissolving many pounds of uranium oxide and in a painstaking way using separating funnels to isolate the impurity. The conclusion was that the uranium did contain significant impurities which had not been taken into account in the calculations. Incidentally, "natural neutrons" first were observed in this experiment. They were, of course, the neutrons from the spontaneous fission of U^{238}.

As the result of discoveries at Berkeley, a new military objective was set for the graphite-uranium chain reaction. Fermi and his associates had always been convinced that a chain-reacting system would find military uses either as a power source for ships, for instance, or for the manufacture on a large scale of radioactive isotopes. The discovery of plutonium gave a new and definite goal for the project.

Hence, in the fall of 1941, the Metallurgical Project was organized at the University of Chicago under the leadership of Arthur H. Compton. Fermi was invited to join and continue the work which he had been directing at Columbia. He and his assistants made the transfer to Chicago by May, 1942. Fermi resumed his work with the exponential pile method on an expanded scale. With the full power of the Government behind the program and with the urgency of a war, both uranium and graphite flowed to the Laboratory in ever increasing amounts. The exponential pile method was applied with great success to analyzing successive batches of material. Experiments were carried out in which the dimensions of the lattice and the amount of uranium in each cell of the lattice were varied. In that summer of 1942 the planning of such experiments, their execution, and the analysis of the results occupied a good fraction of Fermi's time, but not all. Almost 30 separate exponential experiments were done. An exponential pile would be constructed in one or two days working time. In one or two days more, the neutron flux within it would be mapped. The results of the measurements would be given to Fermi, who in a few hours would announce the new value for the reproduction constant. This seeming miracle in calculation was possible because sometime during the period of construction of the exponential pile Fermi would find the time to work out all the correction factors and to prepare the equations so that the fewest mathematical operations would be required with the actual data to find the answer. Much of this labor of mathematical manipulation was done in the early morning hours, since it was his custom to be the first to work in the morning. Because the results were important for the planning of the next experiments, Fermi felt he had to have them at the earliest moment. Offers to assist in this drudgery were gently rejected because Fermi knew that he could get the answer sooner and without error by his own methods. In visiting him in his office, it was not necessary to knock on the door to discover if he was in. If you didn't hear the clacking of the desk calculating machine while walking down the corridor, it was a better bet to try the nearest laboratory.

In June of 1942, the exponential experiments revealed a result, namely k greater than one, for which it could be said that a sufficiently large lattice of uranium and graphite would be self-sustaining. At that time, plans were laid for the pile which first became critical on December 2, 1942. Nevertheless, there was such uncertainty in the value of the measured reproduction constant that Fermi gave his opinion that there was less than a 50% chance that the materials expected to be on hand by December would be sufficient to reach a self-sustaining reaction. Therefore, he urged that provision be made to make the pile as large as the physical space would permit so that as additional materials were received they could be added to the structure. Also, he insisted that provision be made so that the absorbing nitrogen of the atmosphere could be removed if needed. To permit this, the pile was shrouded in an enormous balloon cloth tent. The manufacturers of the balloon cloth understandably were perturbed when they noted that the order called for a balloon of rectangular cross-section. In the days just preceding December 2, 1942, Fermi steadily made measurements of the neutron intensity within the structure and several days before criticality actually was achieved, he was satisfied that the critical point easily would be reached with the materials at hand. Perhaps this helps to explain why on that memorable day while Fermi called to George Weil to withdraw the control rod ever farther out of the pile, the calmest man, the most self-possessed man, was the leader who, in his own mind, several days prior had passed the point of doubt about the success of the experiment.

During the months that the materials for the first pile were being accumulated, Fermi was actively engaged in two other endeavors. A group of physicists, headed by Herbert Anderson, continued the work of measuring the basic cross sections for neutron reactions of interest to the project. Of particular interest was the absolute determination of the number of neutrons emitted by a given source and the techniques for calibrating detection foils and Geiger counters. Although the exponential pile method just related gave a way of getting results in an integral fashion, persistent efforts were made to obtain ever more precise values for the basic constants. A standardized neutron flux was essential for bringing about this situation and Fermi pursued this goal with great determination.

While the Laboratory entrusted with the responsibility of the nuclear phases of the chain reaction was pursuing its objective, the Project as a whole was proceeding with plans for the production of plutonium. Fermi's notebooks, which often were brightly colored paper covered college examination books, show that early in 1942 and continuously he made calculations on heat transfer, critical size, temperature coefficients, control rod effectiveness, and other matters for a variety of production pile designs. Two kinds of production seemed to be required: A small amount of plutonium, gm amounts, to be obtained at the earliest possible moment so that the chemistry and nuclear properties of this strange element could be studied; and secondly, a large amount for weapon use. Fermi made calculations on a surface-cooled graphite pile for the first application and for a helium-cooled, a water-cooled, and a bismuth-cooled pile for the second. He was a member of the Policy Committee of the Laboratory and the record shows that he entered enthusiastically into the discussion of the engineering problems and the probable success for the various types of piles suggested. In reading the reports it is interesting to note, however, that when the purely engineering considerations began to demand ever larger quantities of structural materials in order to improve performance, it was Fermi who called the matter into question by indicating that the reproduction factor had to remain above one or the whole thing would not work. Repeatedly he was asked for his opinion of the effect of a certain structural change or of temperature on the operation, and repeatedly Fermi indicated that only recourse to experiment could give an answer. He knew that the project was proceeding without the detailed knowledge of fundamental constants which would have permitted all decisions to be made by calculation.

The demonstration of the chain reaction in December, 1942, focused attention on the next step, the design of production piles. To this task Fermi gave first priority. Early in 1943, it became clear that the light water-cooled version of the graphite pile evolved in the theoretical section under E. P. Wigner's direction had the best chance of success. Fermi accepted the design and put all his experimental resources to work to produce the information which the DuPont Company, which had accepted responsibility for the construction of the production units, demanded in order to be able to build these very large and costly machines. Let me cite several examples. A reactor operating at high power requires a massive shield. With many hundreds of cooling channels to be provided for, the penetrations of this shield become a matter of crucial importance in the design and construction. Which materials to use and in what thickness were the questions asked? Information on shielding for the mixture of slow neutrons, fast neutrons, and gamma rays emitted by the pile just did not exist. Fermi proposed to get the information with experiments. Time was extremely short and the only tool available was the first pile itself. The idea that the shield should be composed of a combination of a heavy element and hydrogenous material was readily apparent. Inquiry soon showed that the only practical heavy element in the quantities required was steel. Twenty-five tons of steel and suitable quantities of paraffin wax were used to mock up various compositions of shields in a five foot square hole fashioned in the top shield of the pile, which meanwhile had been moved to the Argonne Forest Laboratory. A period of two weeks of furious measurement transpired. Shielding material was stacked in the hole in the pile shield with detectors for neutrons and gamma rays inserted at various depths. As the shield became thicker, the pile was operated at ever higher power. Whereas the original structure was assumed to be limited to a total power output of 10 kilowatts, it was soon operating at 100 kilowatts and above. Gamma-ray intensity was measured with a Lauritzen electroscope, neutron intensity with standardized foils. By no means was this a clean experiment since the changing hardness of the radiation as it penetrated the shield had a pronounced effect on the efficiency of the detectors. Fermi, however, calling upon all his knowledge of neutron physics and of nuclear reactions did experiments to obtain the necessary corrections and ended up with a table of attenuation factors for neutrons and gamma rays for a certain mixture of iron and paraffin. Today that table of relaxation factors is still acceptable for the radiation emitted from a graphite pile. It was an example of brilliant interpretation of experimental data where time did not permit the careful analysis of detector response which would have been a normal procedure. Intuition and mathematical skill were substituted, with the result that a useful shield for the production piles was designed and at the required time.

Somewhat later, the question of the stability of the production piles came into question. Apparently it could not be calculated unequivocally that the temperature coefficient would have a negative sign which was thought to be a prerequisite for safe operation. Despairing of resolving the question by calculation, Fermi suggested removing the central portion of the first graphite pile and replacing it with a facsimile of what the production machine was to be. By heating the facsimile with electricity and observing the effect on the reactivity of the pile, he came to the conclusion that the temperature coefficient would be negative, but only slightly so. It was not the kind of experiment that was his preference. If time had permitted, he would have studied the effect of temperature on the basic effects involved. Fermi knew when short cuts were needed and when it was necessary to produce a concrete result as an encouragement to progress for the main project.

To pursue the plutonium production pile story further, we find Fermi at the Hanford Engineer Works in the State of Washington in the fall of 1944 assisting in the startup of the first large pile. It is customary even today to take an extended period of time, perhaps three months, to bring a large reactor to operating power.

This would have been clearly inconsistent with the purpose of the war-time project and it was greatly due to Fermi's advice, work and presence that the production reactors were brought to operating power in a brief span of time. This is not to say that the startup was uneventful. The first attempts at extended operation at design power encountered the serious poisoning by X^{135}. This was an unexpected phenomenon, but the team of physicists assigned to watch the startup soon had the answer. The reactivity was being affected by a factor which had all the properties of a radioactive substance. By studying the behavior of the reactor and combining this with the period of radioactive decay and yield of fission products, the culprit was identified and once identified could be countered.

Meanwhile, at the Argonne Forest Laboratory, Fermi was giving his attention more and more to the use of the first reactor as a tool in research. Characteristically, the first use was a practical one as far as the project was concerned. The reactivity of the pile could be used to measure the neutron absorption of samples introduced into it. An early example was the fact that the control rod calibration had to be corrected for atmospheric pressure. Under Fermi's direction this quickly evolved into a method for testing uranium and graphite, and became known as the danger coefficient method. It was then used in a routine way to analyze for purity, samples of all the uranium and graphite which were used in the production piles. This was a practical step but the same technique, refined and standardized, was used to measure the absorption cross sections of many of the elements of the periodic table. In fact, the collection and tabulation of these constants received a great acceleration because of the power of this pile method.

In this period, Fermi initiated the work with a beam of neutrons emitted from the pile. Neutrons in the resonance energy region, as well as thermal neutrons, were investigated. The technique of obtaining very slow neutrons, so-called "cold neutrons," by filtering through a long column of graphite was evolved. The transmission of neutrons through microcrystalline materials was studied and the effect of diffraction noted. These early diffraction effects were the direct antecedents of the crystal diffraction techniques which have developed into one of the most powerful methods for the study of crystal structures. Leona and John Marshall and H. L. Anderson were his principal collaborators in this work.

The nuetron chopper, which has been so successful as a tool for exploration of neutron reactions at energies in the 10- and 100-volt regions, was a Fermi invention. The prototype of the first chopper which Fermi made with his own hands was merely a stack of alternate aluminum sheet and steel shim stock held together with laboratory C-clamps. In a few hours, by measurement, Fermi had determined the transmission of a collimated beam of neutrons for the stack as a function of the angle of the plane of the sheets of the stack with the neutron beam. A calculation gave the energy resolution to be expected if the chopper of aluminum and steel plates were to be rotated in the beam at high speed. A short interval for shop work, and an operating chopper was in existence. It was not applied first to substances having resonances which might give exciting-looking graphs, but rather was used to study the cross section of boron, for after all it was boron which was used as the standard for all absorption cross section work.

A few titles of papers or reports by Fermi and his co-workers will suffice to indicate the range of problems made possible by the intense and steady source of neutrons provided by the pile:

Production of Low Energy Neutrons by Filtering through Graphite,
Phase of Neutron Scattering,
Transmission of Slow Neutrons through Microcrystalline Materials,
Interference Phenomena of Slow Neutrons,
Spin Dependence of Scattering of Slow Neutrons by Be, Al, and Bi on the Interaction between Neutrons and Electrons,

and so on. The list is long and it is abundantly clear that the new intensity was being exploited by a master of theory and experiment.

An example of Fermi's method of starting the investigation of a new effect may give a better insight into his methods and will illustrate why it was not only rewarding to work with him but a source of great pleasure as well.

When the first heavy-water reactor of the Argonne Laboratory had reached steady operation, I began to experiment with the collimation of the neutrons from the thermal column. By means of cadmium slits, a beam several inches high, but having an angular spread of only one or two minutes, was produced. The intensity as measured by a BF_3 counter was high, a few thousand counts per minute. Fermi was intrigued. He followed the beam with a counter away from the reactor, a distance of 100 ft or so, and noted how it was weakened by air scattering. He roughly checked the scattering cross section for nitrogen with his observations. We discussed the possible use of the beam in a scattering experiment and then he left for the day.

Several days later, he returned with the proposal that the highly collimated beam be used to look for the total reflection of neutrons. He had a piece of paper with the formula for the index of refraction and calculated values for the index for aluminum, carbon, and beryllium. It seemed that we needed a mirror about one foot wide made of one of these substances. It would take considerable shop work to fabricate one of aluminum. Beryllium, we didn't have. Why not graphite, there was plenty around? While I found a divided circle from a student spectrometer, Fermi worked away at making a flat block of graphite by lapping it against a steel surface plate. Soon we were searching for the totally reflected beam of neutrons and found it. Thus began a technique which has proved to be of great usefulness in the study of the phase of neutron scattering and in the production of polarized neutrons. Refinements in apparatus and technique have followed and in the hands of D. J. Hughes particularly, has produced results in neutron physics of classical stature.

Although by the summer of 1943 the principal decisions concerning plutonium production were made, there still was the problem of providing alternate methods if it were finally proved that the first choice, the graphite pile, should fail. Due to the urging of Harold Urey, plants for the production of heavy water had already been built. A recurring question was how much heavy water would be needed for a heavy water moderated pile. The situation here was quite contrary to that of the case of graphite. Graphite had always been available in quantity. It needed improvement as to its contained impurities. Heavy water, however, was practically nonexistent and experiments to determine the amounts needed for reactor construction had to be undertaken with very limited amounts. Fermi saw the opportunity for doing this using the first graphite pile. Deuterium could be bombarded in the pile. The flux of neutrons was known fairly reliably. The amount of tritium formed could be measured and thus the capture cross section for deuterium determined. Alex Langsdorf undertook this important task. No information existed for the absorption cross section of oxygen. This was determined by the so-called danger coefficient method; namely, a sample of graphite was compared with a sample of liquid carbon dioxide in the pile for its effect on the reactivity of the pile. With these results, Fermi was able to specify that six tons of heavy water would be adequate for a pile fueled with natural uranium. Fermi took an intense interest in the design of this pile, especially as it incorporated openings in the shield which would make possible better experiments with neutron beams. While this unit was under construction during the winter of 1943–44, Fermi left the Chicago Laboratory for Los Alamos. Just before doing so, he carried out a number of measurements, using the graphite pile, which were of vital significance for the work of the weapons laboratory. The first gram of enriched U^{235} produced at the Oak Ridge plant was delivered to him. By ingenious techniques, combining beam and danger coefficient experiments, he determined the fission cross section, the capture cross section, and the number of neutrons emitted per neutron absorbed for this highly important substance. The techniques used had long been in preparation and in a surprisingly short time he had the essential data. Later on, when the first gram of plutonium was produced by the Clinton Engineer Works, or X^{10}, as it was called, it too was sent to Fermi and he repeated the measurements for this material.

I am not able to report on the contributions made by Fermi to the work of the weapons laboratory. I saw him only once during the war on a visit to Los Alamos. I went to his laboratory and the situation appeared normal. There was a pile, the water boiler. Fermi sat at a table on which the recorders from counters were making their familiar noise. A yellow pencil was between his lips and a slide rule in his hand. Another experiment was underway.

Fermi frequently was asked what he thought would be the most important result of the developments in atomic energy. At least on one occasion, I heard him express the point of view that it would take a considerable time before the generation of electricity or the use of the pile in research, including research with pile-made isotopes could be judged as the more beneficial to mankind. His own hunch was that the research uses would win out.

His work in atomic energy clearly shows the enormous power possessed by an investigator who is skilled both in theory and in experiment. As such a man, Fermi was supreme. We cannot expect to see his equal for a very long time.

Dr. Bethe:

Thank you very much Dr. Zinn for this talk which had the one fault that the name of Walter Zinn did not appear sufficiently.

You have heard how the researches of Fermi and his collaborators led to the successful chain reaction and then to the production of plutonium which in turn formed the basis of the Los Alamos effort. You have heard also that Fermi himself joined Los Alamos in early 1944. Long before that time he had been coming to Los Alamos to advise us on our program of theoretical and experimental research. At Los Alamos we all loved him and relied on him to give us the simplicity of viewpoint and the masterful combination of experimental and theoretical insight which he possessed. After the war he often returned to the state of New Mexico which he had come to love. He made great contributions to the development of weapons, including the H Bomb, after the war just as he had during the war.

At the end of the war, Fermi returned to Chicago. You have heard of some of his researches on slow neutrons. You have also heard that his main interest soon shifted to new fields at the newest frontier of physics. You will now hear about this phase from Dr. Anderson who was associated with Fermi in his researches on meson physics.