even it was not without excitement: xenon-135 helped to make life "interesting".

Zinn then scanned the list of applications afforded by the intense beam of neutrons emerging from the pile. It had become abundantly clear in the war-time period that the pile had taken its place among the very useful experimental tools.

Regarding the third part of the atomic energy phase of Fermi's career, namely, the period at Los Alamos, Zinn remembers, "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."

With Herbert L. Anderson (Fermi's first and last co-worker in America) as narrator, the last chapter unfolded. On several occasions during his career Fermi had qualitatively changed the direction of his researches. Finally, he determined to find out what he could about mesons. With the new synchrocyclotron at Chicago, he and his very competent group were able to produce intense beams of either positive or negative mesons with which they made a careful study of the scattering in hydrogen at a variety of meson energies. The results of these experiments are of fundamental importance in any discussion of nuclear interactions.

Fermi immersed himself in every phase of the work, sometimes designing and calibrating equipment, sometimes assembling scintillation counters, sometimes tracing the orbits of the pions—even the soldering iron did not escape!

Anderson described the measurements of total cross sections for the scattering of pions in hydrogen by so-called transmission experiments. A very clear exposition was given of the multiplicity of events that can occur with the negative pions, in which connection an amusing story was related about Fermi's deduction of the ratios of the scattering intensities for the three types of pions. Anderson had shown him, in the midst of some data-taking, a preprint of Brueckner's work on the subject. After only a few moments of mediation, Fermi produced his interpretation of their new data.

The angular distribution measurements were given brief attention, then Anderson went on to considerations of the corresponding phase shift analysis. In the latter discussion he indicated how another principle, the condition of causality, helps in sorting out the physically admissible solutions from the many that are found mathematically.

This review of thirty years of Fermi's life, observed Chairman Bethe, is in reality the story of thirty years of modern physics. In his concluding remarks, Bethe recalled how often physicists would go to Fermi with their problems and how he always had time to listen. Sometimes he would give the answer straightaway; more often he would simply reformulate the problem in so clear a fashion that the answer would be all but crystallized, thus giving his listener the pleasure of taking the last easy step.

PHYSICS

*By **Enrico Fermi***

The following is a verbatim transcript of Enrico Fer last address before the American Physical Society, delive informally and without notes at Columbia Universi McMillin Theater on Saturday morning, January 30, 1 His retiring presidential address was delivered one earlier. The present speech, transcribed from a tape rec ing, is left deliberately in an unpolished and unedited fo Such informality would no doubt have been frowned u by Fermi, who was very particular about his publis writings. For those who knew Fermi or heard him sp however, the verbatim transcript may serve (as no for document could ever serve) to bring back for a mom the very sound of his voice. The paper was presented part of the session "Physics at Columbia University" dur the Society's 1954 annual meeting.

Mr. Chairman, Dean Pegram, fellow members, ladies and gentlemen:

IT seems fitting to remember, on this 200th anniversary of Columbia University, the key role that the University played in the early experimentation and the organization of the early work that led to the development of atomic energy.

I had the good fortune to be associated with the Pupin Laboratories through the period of time when at least the first phase of this development took place. I had had some difficulties in Italy and I will always be very grateful to Columbia University for having offered me a position in the Department of Physics at the most opportune moment. And in addition this offer gave me, as I said, the rare opportunity of witnessing the series of events to which I have referred.

In fact I remember very vividly the first month, January, 1939, that I started working at the Pupin Laboratories because things began happening very fast. In that period, Niels Bohr was on a lecture engagement in Princeton and I remember one afternoon Willis Lamb came back very excited and said that Bohr had leaked out great news. The great news that had leaked out was the discovery of fission and at least an outline of its interpretation; the discovery as you well remember goes back to the work of Hahn and Strassmann and at least the first idea for the interpretation came through the work of Lise Meitner and Frisch who were at that time in Sweden.

Then, somewhat later that same month, there was a meeting in Washington organized by the Carnegie In-

[C]OLUMBIA UNIVERSITY

the Genesis of the Nuclear Energy Project

ENRICO FERMI

stitution in conjunction with George Washington University where I took part with a number of people from Columbia University and where the possible importance of the new-discovered phenomenon of fission was first discussed in semi-jocular earnest as a possible source of nuclear power. Because it was conjectured, if there is fission with a very serious upset of the nuclear structure, it is not improbable that some neutrons will be evaporated. And if some neutrons are evaporated, then they might be more than one; let's say, for the sake of argument, two. And if they are more than one, it may be that the two of them, for example, may each one cause a fission and from that one sees of course a beginning of the chain reaction machinery.

So that was one of the things that was discussed at that conference and started a small ripple of excitement about the possibility of releasing nuclear energy. At the same time experimentation was started feverishly in many laboratories, including Pupin, and I remember before leaving Washington I had a telegram from Dunning announcing the success of an experiment directed to the discovery of the fission fragments. The same experiment apparently was at the same time carried out in half a dozen places in this country and in three or four, in fact I think slightly before, in three or four places in Europe.

Now a rather long and laborious work was started at Columbia University in order to firm up these vague suggestions that had been made as to the possibilities that neutrons were emitted and try to see whether neutrons were in fact emitted when fission took place and if so how many they would be, because clearly a matter of numbers is in this case extremely important because a little bit greater or a little bit lesser probability might have made all the difference between possibility and impossibility of a chain reaction.

Now this work was carried on at Columbia simultaneously by Zinn and Szilard on one hand and by Anderson and myself on the other hand. We worked independently and with different methods, but of course we kept close contact and we kept each other informed of the results. At the same time the same work was being carried out in France by a group headed by Joliot and Von Halban. And all the three groups arrived at the same conclusion—I believe Joliot may be a few weeks earlier than we did at Columbia—namely that neutrons are emitted and they were rather abundant, although the quantitative measurement was still very uncertain and not too reliable.

A curious circumstance related to this phase of the work was that here for the first time secrecy that has been plaguing us for a number of years started and, contrary to perhaps what is the most common belief about secrecy, secrecy was not started by generals, was not started by security officers, but was started by physicists. And the man who is mostly responsible for this certainly extremely novel idea for physicists was Szilard.

I don't know how many of you know Szilard; no doubt very many of you do. He is certainly a very peculiar man, extremely intelligent (LAUGHTER). I see that is an understatement (LAUGHTER). He is extremely brilliant and he seems somewhat to enjoy, at least that is the impression that he gives to me, he seems to enjoy startling people.

So he proceeded to startle physicists by proposing to them that given the circumstances of the period—you see it was early 1939 and war was very much in the air—given the circumstances of that period, given the danger that atomic energy and possibly atomic weapons could become the chief tool for the Nazis to enslave the world, it was the duty of the physicists to depart from what had been the tradition of publishing significant results as soon as the *Physical Review* or other scientific journals might turn them out, and that instead one had to go easy, keep back some results until it was clear whether these results were potentially dangerous or potentially helpful to our side.

So Szilard talked to a number of people and convinced them that they had to join some sort of—I don't know whether it would be called a secret society, or what it would be called. Anyway to get together and circulate this information privately among a rather restricted group and not to publish it immediately. He sent in this vein a number of cables to Joliot in France, but he did not get a favorable response from him and Joliot published his results more or less like results in physics had been published until that day. So that the fact that neutrons are emitted in fission in some abundance—the order of magnitude of one or two or three—became a matter of general knowledge. And, of course, that made the possibility of a chain reaction appear to most physicists as a vastly more real possibility than it had until that time.

Another important phase of the work that took place at Columbia University is connected with the suggestion on purely theoretical arguments, by Bohr and Wheeler, that of the two isotopes of uranium it was not the most abundant uranium 238 but it was the least abundant uranium 235, present as you know in the natural uranium mixture to the tune of 0.7 of a per cent, that was responsible at least for most of the thermal fission. The argument had to do with an even number of neutrons in uranium 238 and an odd number of neutrons in uranium 235 which, according to a discussion of the binding energies that was carried out by Bohr and Wheeler, made plausible that uranium 235 should be more fissionable.

Now it clearly was very important to know the facts also experimentally and work was started in conjunction by Dunning and Booth at Columbia University and by Nier. Nier took the mass spectrographic part of this work, attempting to separate a minute but as large as possible amount of uranium 235, and Dunning and Booth at Columbia took over the part of using this minute amount in order to test whether or not it would undergo fission with a much greater cross section than ordinary uranium.

Well, you know of course by now that this experiment confirmed the theoretical suggestion of Bohr and Wheeler, indicating that the key isotope of uranium, from the point of view of any attempt of—for example—constructing a machine that would develop nuclear energy, was in fact uranium 235. Now you see the matter is important primarily for the following reasons that at the time were appreciated perhaps less definitely than at the present moment.

The fundamental point in fabricating a chain reacting machine is of course to see to it that each fission produces a certain number of neutrons and some of these neutrons will again produce fission. If an original fission causes more than one subsequent fission then of course the reaction goes. If an original fission causes less than one subsequent fission then the reaction does not go.

Now, if you take the isolated pure isotope U-235, you may expect that the unavoidable losses of neutrons will be minor, and therefore if in the fission somewhat more than one neutron is emitted then it will be merely a matter of piling up enough uranium 235 to obtain a chain reacting structure. But if to each gram of uranium 235 you add some 140 grams of uranium 238 that come naturally with it, then the competition will be greater, because there will be all this ballast ready to snatch away the not too abundant neutrons that come out in the fission and therefore it was clear at the time that one of the ways to make possible the production of a chain reaction was to isolate the isotope U-235 from the much more abundant isotope U-238.

Now, at present we have in our laboratories a row of bottles labeled, more or less, isotope—what shall I say—iron 56, for example, or uranium 235 or uranium 238 and these bottles are not quite as common as would be a row of bottles of chemical elements, but they are perfectly easily obtainable by putting due pressure on the Oak Ridge Laboratory (LAUGHTER). But at that time isotopes were considered almost magically inseparable. There was to be sure one exception, namely deuterium, which was already at that time available in bottles. But of course deuterium is an isotope in which the two isotopes hydrogen one and hydrogen two have a ratio of mass one to two, which is a very great ratio. But in the case of uranium the ratio of mass is merely 235 to 238, so the difference is barely over one per cent. And that, of course, makes the differences of these two objects so tiny that it was not very clear that the job of separating large amounts of uranium 235 was one that could be taken seriously.

Well, therefore, in those early years near the end of 1939 two lines of attack to the problem of atomic energy started to emerge. One was as follows. The first step should be to separate in large amounts, amounts of kilograms or maybe amounts of tens of kilograms or maybe of hundreds of kilograms, nobody really knew how much would be needed, but something perhaps in that order of magnitude, separate such at that time fantastically large-looking amounts of uranium 235 and then operate with them without the ballast of the associated much larger amounts of uranium 238. The other school of thought was predicated on the hope that perhaps the neutrons would be a little bit more and that perhaps using some little amount of ingenuity one might use them efficiently and one might perhaps be able to achieve a chain reaction without having to separate the isotopes, a task as I say that at that time looked almost beyond human possibilities.

Now I personally had worked many years with neutrons, and especially slow neutrons, so I associated myself with the second team that wanted to use nonseparated uranium and try to do the best with it. Early attempts and studies, discussions, on how to separate the isotopes of uranium were started by Dunning and Booth in close consultation with Professor Urey. On the other hand, Szilard, Zinn, Anderson, and myself started experimentation on the other line whose first step involved lots of measurements.

Now, I have never yet quite understood why our measurements in those days were so poor. I'm noticing now that the measurements that we are doing on pion physics are very poor, presumably just because we have not learned the tricks. And, of course, the facilities that we had at that time were not as powerful as they are now. It's much easier to carry out experimentation with neutrons using a pile as a source of neutrons than it was in those days using radium-beryllium sources when geometry was the essential item to control or using the cyclotron when intensity was the desired feature rather than good geometry.

Well, we soon reached the conclusion that in order to have any chance of success with natural uranium we had to use slow neutrons. So there had to be a moderator. And this moderator could have been first water or other substances. Water was soon discarded; it's very effective in slowing down neutrons, but still absorbs a little bit too many of them and we could not afford that. Then it was thought that graphite might be perhaps the better bet. It's not as efficient as water in slowing down neutrons; on the other hand little enough was known of its absorption properties that the hope that the absorption might be very low was quite tenable.

This brings us to the fall of 1939 when Einstein wrote his now famous letter to President Roosevelt advising him of what was the situation in physics—what was brewing and that he thought that the government had the duty to take an interest and to help along this development. And in fact help came along to the tune of \$6000 a few months after and the \$6000 were used in order to buy huge amounts—or what seemed at that time when the eye of physicists had not yet been distorted—(LAUGHTER) what seemed at that time a huge amount of graphite.

So physicists on the seventh floor of Pupin Laboratories started looking like coal miners (LAUGHTER) and the wives to whom these physicists came back tired at night were wondering what was happening. We know that there is smoke in the air, but after all (LAUGHTER).

Well, what was happening was that in those days we were trying to learn something about the absorption properties of graphite, because perhaps graphite was no good. So, we built columns of graphite, maybe four feet on the side or something like that, maybe ten feet high. It was the first time when apparatus in physics, and these graphite columns were apparatus, was so big that you could climb on top of it—and you had to climb on top of it. Well, cyclotrons were the same way too, but anyway the first time when I started climbing on top of my equipment because it was just too tall—I'm not a tall man (LAUGHTER).

And then sources of neutrons were inserted at the bottom and we were studying how these neutrons were first slowed down and then diffused up the column and of course if there had been a strong absorption they would not have diffused very high. But because it turned out that the absorption was in fact small, they could diffuse quite readily up this column and by making a little bit of mathematical analysis of the situation it became possible to make the first guesses as to what was the absorption cross section of graphite, a key element in deciding the possibility or not of fabricating a chain reacting unit with graphite and natural uranium.

Well, I will not go into detail of this experimentation. That lasted really quite a number of years and required really quite many hours and many days and many weeks of extremely hard work. I may mention that very early our efforts were brought in connection with similar efforts that were taking place at Princeton University where a group with Wigner, Creutz and Bob Wilson set to work making some measurements that we had no possibility of carrying out at Columbia University.

Well, as time went on, we began to identify what had to be measured and how accurately these things that I shall call "eta", f, and p—I don't think I have time to define them for you—these three quantities "eta", f, and p had to be measured to establish what could be done and what could not be done. And, in fact, if I may say so, the product of "eta", f, and p had to be greater than one. It turns out, we now know, that if one does just about the best this product can be 1.1.

So, if we had been able to measure these three quantities to the accuracy of one per cent we might have found that the product was for example 1.08 plus or minus 0.03 and if that had been the case we would have said let's go ahead, or if the product had turned out to be 0.95 plus or minus 0.03 perhaps we would have said just that this line of approach is not very promising, and we had better look for something else. However I've already commented on the extremely low quality of the measurements in neutron physics that could be done at the time—where the accuracy of measuring separately either "eta", or f, or p was perhaps with a plus or minus of 20 per cent (LAUGHTER). If you compound, by the well-known rules of statistics, three errors of 20 per cent you will find something around 35 per cent. So if you should find, for example, 0.9 plus or minus 0.3—what do you know? Hardly anything at all (LAUGHTER). If you find 1.1 plus or minus 0.3—again, you don't know anything much. So that was the trouble and in fact if you look in our early work—what were the detailed values given by this or that experimenter to, for example, "eta" you find that it was off 20 per cent and sometimes greater amounts. In fact I think it was strongly influenced by the temperament of the physicist. Shall we say optimistic physicists felt it unavoidable to push these quantities high and pessimistic physicists like myself tried to keep them somewhat on the low side (LAUGHTER).

Anyway, nobody really knew and we decided therefore that one had to do something else. One had to devise some kind of experiment that would give a complete over-all measurement directly of the product "eta", *f*, *p* without having to measure separately the three, because then perhaps the error would sort of drop down and permit us to reach conclusions.

Well, we went to Dean Pegram, who was then the man who could carry out magic around the University, and we explained to him that we needed a big room. And when we say big we meant a really big room, perhaps he made a crack about a church not being the most suited place for a physics laboratory in his talk, but I think a church would have been just precisely what we wanted (LAUGHTER). Well, he scouted around the campus and we went with him to dark corridors and under various heating pipes and so on to visit possible sites for this experiment and eventually a big room, not a church, but something that might have been compared in size with a church was discovered in Schermerhorn.

And there we started to construct this structure that at that time looked again in order of magnitude larger than anything that we had seen before. Actually if anybody would look at that structure now he would probably extract his magnifying glass (LAUGHTER) and go close to see it. But for the ideas of the time it looked really big. It was a structure of graphite bricks and spread through these graphite bricks in some sort of pattern were big cans, cubic cans, containing uranium oxide.

Now, graphite is a black substance, as you probably know. So is uranium oxide. And to handle many tons of both makes people very black. In fact it requires even strong people. And so, well we were reasonably strong, but I mean we were, after all, thinkers (LAUGHTER). So Dean Pegram again looked around and said that seems to be a job a little bit beyond your feeble strength, but there is a football squad at Columbia (LAUGHTER) that contains a dozen or so of very husky boys who take jobs by the hour just to carry them through College. Why don't you hire them?

And it was a marvelous idea; it was really a pleasure for once to direct the work of these husky boys, canning uranium—just shoving it in—handling packs of 50 or 100 pounds with the same ease as another person would have handled three or four pounds. In passing these cans fumes of all sorts of colors, mostly black, would go in the air (LAUGHTER).

Well, so grew what was called at the time the exponential pile. It was an exponential pile, because in the theory an exponential function enters—which is not surprising. And it was a structure that was designed to test in an integral way, without going down to fine details, whether the reactivity of the pile, the reproduction factor, would be greater or less than one. Well, it turned out to be 0.87. Now that is by 0.13 less than one and it was bad. However, at the moment we had a firm point to start from, and we had essentially to see whether we could squeeze the extra 0.13 or preferably a little bit more. Now there were many obvious things that could be done. First of all, I told you these big cans were canned in tin cans, so what has the iron to do? Iron can do only harm, can absorb neutrons, and we don't want that. So, out go the cans. Then, what about the purity of the materials? We took samples of uranium, and with our physicists' lack of skill in chemical analysis, we sort of tried to find out the impurities and certainly there were impurities. We would not know what they were, but they looked impressive, at least in bulk (LAUGHTER). So, now, what do these impurities do?—clearly they can do only harm. Maybe they make harm to the tune of 13 per cent. Finally, the graphite was quite pure for the standards of that time, when graphite manufacturers were not concerned with avoiding those special impurities that absorb neutrons. But still there was some considerable gain to be made out there, and especially Szilard at that time took extremely decisive and strong steps to try to organize the early phases of production of pure materials. Now, he did a marvelous job which later on was taken over by a more powerful organization than was Szilard himself. Although to match Szilard it takes a few able-bodied customers (LAUGHTER).

Well, this brings us to Pearl Harbor. At that time, in fact I believe a few days before by accident, the interest in carrying through the uranium work was spreading; work somewhat similar to what was going on at Columbia had been initiated in a number of different Universities throughout the country. And the government started taking decisive action in order to organize the work. and, of course, Pearl Harbor gave the final and very decisive impetus to this organization. And it was decided in the high councils of the government that the work on the chain reaction produced by nonseparated isotopes of uranium should go to Chicago.

That is the time when I left Columbia University, and after a few months of commuting between Chicago and New York eventually moved to Chicago to keep up the work there, and from then on, with a few notable exceptions, the work at Columbia was concentrated on the isotope-separation phase of the atomic energy project.

As I've indicated this work was initiated by Booth, Dunning, and Urey about 1940, 1939, and 1940, and with this reorganization a large laboratory was started at Columbia under the direction of Professor Urey. The work there was extremely successful and rapidly expanded into the build-up of a huge research laboratory which cooperated with the Union Carbide Company in establishing some of the separation plants at Oak Ridge. This was one of the three horses on which the directors of the atomic energy project had placed their bets, and as you know the three horses arrived almost simultaneously to the goal in the summer of 1945. I thank you. (APPLAUSE)