To Trudy
with best wishes
Herbert

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The Legacy of Fermi and Szilard

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Across the street from my office in the Enrico Fermi Institute of the University of Chicago, a huge piece of sculpture tries to convey more than can be read on the plaque below: "On December 2nd, 1942, Man achieved here the first self-sustaining chain reaction and thereby initiated the first controlled release of nuclear energy." It is a reminder of what happened on a bleak December day that changed the course of history.

In tracing the sequence of events that led to the chain reaction, I began to wonder what it was that selected those who played the principal roles.[1] How did it happen that it was Fermi who built the chain reaction and that it was Leo Szilard who had invented it eight years earlier. What drew these and others, too, so irresistibly to the chain reaction? Is it true, as Pasteur is supposed to have said, that "fortune favors the prepared mind"? If so, it would be one of the best reasons I know for a liberal education.

What prepared Szilard to invent the chain reaction? The simple, albeit incomplete, answer is that he had read H. G. Wells. The development of nuclear energy had been anticipated by H. G. Wells by 30 years. He wrote about it in one of his less well-known books, "The World Set Free," published in 1914. Some of his prophetic vision about what would happen in a world with nuclear energy is still unfolding.

The solid scientific fact that H. G. Wells had at his disposal when he wrote this book was what was known then about natural radioactivity: that uranium disintegrated by emitting alpha particles. This was a process yielding a million times more energy per atom than in ordinary combustion. The trouble was that it took place very slowly. What was needed, H. G. Wells realized, was a way to speed it up. Then, from a pound or two of uranium, enough energy could be obtained to light a great city, power the wheels of industry, drive airplanes and, inevitably, fashion devastating weapons of war. When H. G. Wells wrote his book in 1913, a year before World War I, he put 1933 as the date this essential step would be taken, a date that coincides almost exactly with the actual discovery of artificial radioactivity. Those who knew Szilard would understand instantly

Herbert L. Anderson was a member of the team that built the first atomic pile which resulted in the first self-sustaining (or controlled) nuclear chain reaction on December 2, 1942, at the University of Chicago. His account of those times, the first of two installments, is part of the Bulletin's *continuing series on the reminiscences of nuclear pioneers. Anderson is professor of physics at the University of Chicago and a former director of the Enrico Fermi Institute for Nuclear Studies.*

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why this idea would excite him and why he would keep turning it over and over in his mind until he could figure out what he could do with it.

It was in the fall of 1933 when Szilard found himself in London, a time when many exciting discoveries in nuclear physics were being made. Only the year before the neutron had been discovered by James Chadwick. Irene Curie and her husband, Frederic Joliot, were on the threshold of their discovery of artificial radioactivity.

Szilard read in the newspapers about an annual meeting of the British Association where Lord Rutherford was reported to have said, "Whoever talks about the liberation of atomic energy on an industrial scale is talking moonshine."[2] Such pronouncements by experts, who claim that something cannot be done, can irritate a man like Szilard, and it set him to thinking how he could prove otherwise.

Just after reading Rutherford's comment, Szilard was walking down Southhampton Row and pondering how he might prove Rutherford wrong. He stopped for a traffic light, and when the light changed to green and he crossed the street, it suddenly occurred to him that if he could find an element which when split by one neutron would then emit two neutrons, he could make a chain reaction. Such a chain reaction would be able to liberate energy on a large scale. His candidate for the proper element was beryllium which was thought to have the kind of instability that would emit neutrons when it disintegrated. In the spring of 1934, Szilard applied for a patent which described the laws governing such a chain reaction.[3] It turned out later, however, that beryllium was actually stable and could not sustain a chain reaction in this way.

Because he had read H. G. Wells, Szilard had a vivid conception of what might happen to the world if the great power of nuclear energy were turned to destructive purposes. He wanted it on the record that he had found the way to nuclear power, but he didn't want it to fall into unscrupulous hands. The Szilardian way to manage this was to have it kept secret by assigning the patent to the British Admiralty.

Szilard had to wait five years before a suitable nuclear reaction was found. When the fission of uranium was discovered at the end of 1938, Szilard knew instantly that this is what he had been looking for. Early in January 1939 he was in Princeton, New Jersey, visiting Eugene Wigner, who told him about Otto Hahn's discovery. The imminence of another world war seemed very real to both men and they agreed it was urgent to set up experiments to show whether, in fact, neutrons were emitted in the fission process in uranium. Szilard decided to go, post haste, to Columbia University where Fermi was. Szilard knew that if the chain reaction was to work, Fermi was the man to do it.

The timing is interesting here. Fermi had just been awarded the Nobel Prize for his discovery of artificial radioactivity produced by slow neutrons; and he had taken that occasion to begin his American career. Italy, under Mussolini, bending evermore deeply to the influence of Hitler, had adopted outrageous racial laws that threatened much that was important to him. So he took his family to New York and on January 9, 1939, arrived to take up a professorship at Columbia University.

Fission of Uranium

Just weeks before, on Dec. 22, 1938, Hahn and Strassmann had made their discovery of the fission of uranium.[4] The news of their discovery came to the United States via Niels Bohr, who had heard about it from Otto Frisch. Frisch, in turn, had gotten it from Lise Meitner, who while in Sweden had received a letter about it from her former collaborators in Germany who had done the work. The news was particularly exciting to Bohr because the fission of uranium seemed to be a beautiful and dramatic confirmation of his idea that the nucleus behaved like a liquid drop. He could have predicted it, but he hadn't. Bohr was so excited about this discovery that when he arrived in New York, on Jan. 16, 1939, he just had to tell it to someone. Frisch had asked Bohr not to let the cat out of the bag until he had a chance to do the decisive experiment that would demonstrate the energy release directly, but it was hard to restrain Bohr for very long.

Bohr went to Princeton, but a few days after he had settled there he came through New York on his way to Washington. He was anxious to see Fermi's reaction to his great news. Fermi wasn't in his office at Columbia, so Bohr went down to the basement where the cyclotron was. Fermi wasn't there either, but I was. Undeterred, he came right over and grabbed me by the shoulder. Bohr doesn't lecture to you, he whispers in your ear. "Young man," he said, "let me explain to you about something new and exciting in physics." Then he told me about the splitting of the uranium nucleus and how naturally this fit in with the idea of the liquid drop. I was quite enchanted. Here was the great man himself, impressive in his bulk, sharing his excitement with me as if it were of the utmost importance for me to know what he had to say. Suddenly everything I had done

"Bohr came right over and grabbed me by the shoulder."

in the last five years began to make sense. Neutrons brought about the fission of uranium, and neutrons had become my field.

As a graduate student, I already had some exposure to neutron physics working with Dana Mitchell and with John Dunning, who had made Columbia a center of neutron research. I had learned a good deal about what the others who worked with neutrons did and why. I had come to Columbia to become a radio engineer, but a part time job brought me in contact with Dana Mitchell who persuaded me to switch to physics. My knowledge of radio circuits turned out to be useful in building the cyclotron. At this point I had started my PhD research on the scattering of neutrons and had already completed most of the apparatus. I could understand what Bohr was saying, and what he said had exciting implications. I could sense the importance of the discovery and the new possibilities for experiments that would follow. Suddenly, I had a lot to talk about with Fermi.

Bohr couldn't wait for Fermi as he was on his way to Washington. As soon as he left I rushed off to find Fermi. I found him in his office, but he had anticipated me. He had already heard about the fission of uranium from Willis Lamb, who had just heard Bohr talk about it at Princeton. Before I had a chance to say anything he smiled in a friendly fashion and said, "I think I know what you want to tell me. Let *me* explain to you about fission." Then he went to the blackboard and in his inimitable, graphic way showed how the uranium nucleus would split in two. Then he estimated the amount of energy release from the mass defect. I have to say that Fermi's explanation was even more dramatic than Bohr's. It made the experimental possibilities even more exciting. It became obvious that one could check the energy release by looking for the intense burst of ionization from the splitting parts. We didn't know then that this was also Frisch's plan.

I had just completed an ionization chamber-linear amplifier apparatus that was just what was needed for the job. There was also the cyclotron I helped build which was available as a neutron source. It just seemed natural to offer to work with Fermi using these. After all, he had arrived at Columbia only a few weeks before and had neither equipment nor students to work with.

Working with Fermi

The fact is, I was immensely drawn to Fermi. He had an unusual grasp of physics which he kept at his fingertips always ready for use. When a problem arose, he had the knack to be able to go to the blackboard and simply work it out. The physics just flowed out of the chalk. He would start with the principles he was certain you knew, and he would write these in simple mathematical form, make a few plausible approximations to give equations that were easy to solve, obtain a formula, put in the numbers and calculate the result, usually with a small slide rule. If at any stage you appeared puzzled, he would clarify the argument in terms he knew you would understand. The fact that he could read me, and I him, made it easy and natural for us to work together.

Another quality was the *way* he did physics. Physics was not something you organized and then managed by getting other people to do the work. With Fermi, it was the work that made the physics worthwhile. He wanted to wrestle with nature himself, with his own hands. He liked to have someone to work with. He liked the companionship; the work went faster that way. He liked to talk about what he was thinking and to show what he was doing. So, as he put it, we made a deal. I would teach him Americana, and he would teach me physics. That's how things worked out. On that day we began a collaboration that continued happily, almost without interruption, until his death some 15 years later.

In any case, we didn't lose any time. We wanted to find out whether the heavy burst of ionization from fission could be seen. The linear amplifer-ionization chamber combination was just the thing for this. All we had to do was prepare a layer of uranium on one electrode and insert it in the chamber. That same afternoon we set up everything at the cyclotron. But the cyclotron was not working very well that day. Then I remembered those radon-beryllium sources on the thirteenth floor of Pupin Laboratory.

By that time Fermi had already left for a conference in Washington, the same meeting to which Bohr had gone. John Dunning, under whom I had been working as a graduate student, came back to the lab that night. The record in my notebook is dated January 25, 1939. With the neutron source near the ionization chamber and some paraffin to slow down the neutrons, we were able to see, from time to time, on the screen of our cathode ray oscilloscope, the huge ionization pulses that we expected from fission. It was the first time in America.

It was a very propitious moment. Dunning, recognizing the importance of what we had done, telegraphed Fermi in Washington about our observation. When the meeting opened the next day both Bohr and Fermi talked about the fission problem. Fermi was able to speak with the conviction of personal experience. Then he mentioned the possibility that neutrons might be emitted during the splitting. After all, the fission products would have a large neutron excess. Although this was only a guess, its implication for a chain reaction produced a great deal of excitement. Physicists at the meeting rushed to call their laboratories, and soon there were confirmations from a number of places throughout the country.

Things were happening fast. Fermi's arrival in the United States on January 9, 1939 was followed by Bohr's just one week later. The meeting in Washington occurred on January 26. Our observation of the energy release in fission had been made the previous day. Later we found that Frisch had already completed his experiment in Copenhagen on January 15, ten days before. On January 30 there was a news release by Watson Davis of Science Service that talked about a world standing on the brink of atomic power. This was also four months after the capitulation at Munich. Hitler was on the march; he was preparing his takeover of Czechoslovakia.

Neutrons from Uranium

Fermi didn't wait until the end of the meeting in Washington to return. He rushed back to Columbia and straightaway called me to his office. My notebook lists the experiments he felt we should do right away. The date was January 29, 1939.

Fermi saw from the first that if there was to be a chain reaction, if useful amounts of nuclear power were to be made, there would have to be a way to regenerate the fission reaction to involve large numbers of nuclei. This could come about if in the course of splitting, some neutrons (more than one on the average) were emitted. Then, with each fission more neutrons would be available for further fissions; and if not too many were lost, their number could continue to multiply until the reaction became self-sustaining.

In carrying out further experiments that could be done with the ionization chamber-linear amplifier combination all the members of Dunning's group working with the cyclotron participated. For several years, Eugene Booth and Norris Glasoe had devoted themselves to constructing the cyclotron. Francis Slack had only recently arrived to spend a sabbatical year at Columbia. The subsequent developments caused him to extend his stay many years beyond his original intention. Fermi's insistence that quantitative measurements be carried out prevailed, and the first paper, written only two weeks after our initial observation, reported the value of the fission cross-section for slow as well as for fast neutrons. The measurements were rather crude, but a rough number is better than none at all. In attempting to determine whether a chain reaction was possible, numerical values were crucial. The fission cross-section was of central importance. If this had turned out to be too small the chain reaction would have been impossible.

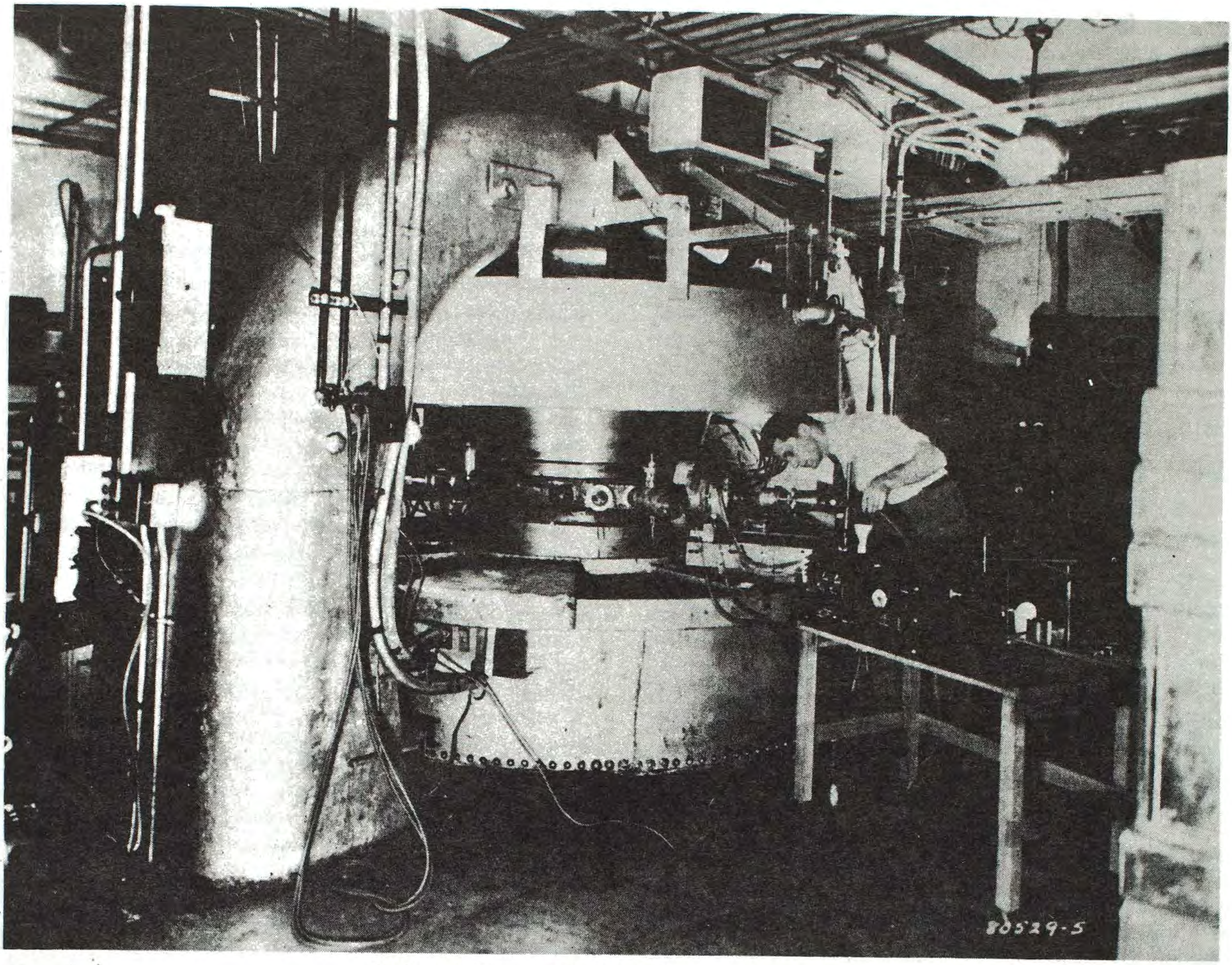
Ironically, more use was made of the radon-beryllium sources than the cyclotron in these experiments. What these natural sources lacked in intensity they made up in reliability and steadiness of output. In some experiments their small size was a distinct advantage. In those days the cyclotron was frequently down for repairs. And although the situation improved with time, Fermi, for the most part, preferred the radon-beryllium sources, and Dunning generously placed these facilities at his disposal. I had to learn how to prepare these sources and came to make a good many of them.

The first experiment was published in the March 1, 1939, issue of the *Physical Review* under the title "The Fission of Uranium."[5] It was a second paper, however, completed only two months after we had heard the news from Bohr, that gave the evidence for fast neutrons emitted by uranium. This paper, "Products of Neutrons in Uranium Bombarded by Neutrons," was published in the April 15, 1939 issue of the *Physical Review;* H. B. Hanstein, another graduate student, joined us for this work.[6]

Following the publication of the first paper on the fission of uranium, Dunning and his group began to follow a different line of research. They decided to test Bohr's idea that the rarer isotope of uranium with mass 235 was the one responsible for the fission we observed. Since this isotope makes up less than one percent of natural uranium, it was clear that, if this idea were correct, the cross-section for the fission in the pure uranium-235 isotope would be more than one hundred times larger than that ascribed to the natural mixture. By separating the 235 isotope, it would be much easier to obtain the chain reaction. More than this, with the separated isotope the prospect for a bomb with unprecedented explosive power would be very great.

Dunning knew he could obtain small quantities of the 235 isotope, quite pure, from Al Nier of the University of Minnesota. Nier was an expert with the mass spectrograph. His machine could be used to obtain small quantities of separated isotopes. He agreed to a joint project, and in a few months Nier had produced enough to carry out a definitive test.[7] The result was positive and, thereafter, Dunning turned his energies to the problem of separating the isotopes. The story of his success in this is told elsewhere.[8]

The way of isotope separation did not appeal to Fermi. He was not discouraged by the small cross-section for fission in the natural isotope. "Stay with me," he advised, "we'll work with natural uranium. You'll see. We'll be the first to make the chain reaction." I stuck with Fermi. As an added inducement he promised, in a half-serious tone, "Uranium is



Herbert Anderson at the Columbia cyclotron in 1939.

going to be a very important material, and some day you may be president of the Uranium Company of America."

We were not the only ones to realize that neutron emission was the key to the problem of the chain reaction. Nothing known then guaranteed the emission of neutrons. Neutron emission had to be observed experimentally and measured quantitatively. In France, with ominous war clouds threatening, Hans von Halben, Lew Kowarski, and Frederic Joliot-Curie, it turned out, had done essentially the same experiment as ours. Their paper in *Nature*[9] appeared a week or two earlier than ours in the *Physical Review*. However rapidly our work went in those first few months, the French group managed to publish something along the same lines a week or so ahead of us.

Szilard at Columbia

Besides the French group, another group was working on the neutron question. Leo Szilard and Walter Zinn had joined forces to demonstrate neutron emission with an experiment quite different from ours. They were also working in the Pupin Laboratories at Columbia, but on another floor. Szilard was not a member of the faculty, but he had arranged through Dean George B. Pegram for a guest appointment. Szilard had already established himself as a nuclear physicist of some standing. While in England he had gained fame for inventing an ingenious way to concentrate radioactive isotopes known as the Szilard-Chalmers process, and he showed how to produce neutrons from x-rays.

The work of Fermi and his group with neutrons and the radioactivity they could induce was particularly intriguing to Szilard. When he came to Columbia he persuaded Walter Zinn to work with him on the neutron emission problem. Zinn was already doing experiments on the scattering of fast neutrons in light elements. Since he had both the source of neutrons and the necessary detecting apparatus, namely, ionization chambers and a linear amplifier, he had also verified the emission of heavy fragments in fission. He had begun an attempt to find the fission with this equipment when Szilard entered the picture with the suggestion that the experiment would be much improved if the photoneutron source were used instead of the d-d neutrons. Zinn agreed,

Szilard convinced himself that with graphite a chain reaction using natural uranium would work. Hard evidence was lacking but he simply argued that it would be too risky to make any other assumption.

but pointed out that a photoneutron source was not available. Szilard quickly corrected this deficiency. He sent to Oxford for a hollow cylinder of beryllium he had used in experiments there. He also rented a gram of radium with some money he managed to borrow from a friend. At the first try with this new set up, the fast neutrons from fission were immediately evident.[10]

It was not Szilard's idea to compete with Fermi; he really wanted to work with him. He managed to interject himself into our experiment in an interesting way. He had a criticism of our neutron emission experiment. He went to Fermi and said: "In your experiment, Enrico, you used a radon-beryllium source. That source, as you know, has rather energetic neutrons. How do you know that some of the neutrons are coming not from fission but from a direct (n, 2n) reaction?" When Fermi conceded this point, Szilard was ready: "It just happens that I have a radium-beryllium photoneutron source that produces neutrons of much lower energy. With it, you won't have the problem of the (n, 2n) reaction." Fermi didn't think this was a serious question, but he did admit that the results would be less open to question if the photoneutron source were used. So we repeated the experiments with Szilard's source. We still found that uranium emitted more neutrons than it absorbed. In our paper we acknowledged a curious organization called The Association for Scientific Collaboration, a Szilardian creation.

It then appeared that none of the neutron emission experiments were really conclusive. Szilard urged a larger scale experiment, and the three of us joined forces to carry one out. A great deal of work was involved. There were long tin cans which we had to pack with uranium oxide powder and seal; we had to mix a huge solution of manganese after each irradiation; and we had to follow the radioactivity induced in it throughout the night.

Fermi's idea of doing an experiment was that everyone worked. He liked to work harder than anyone else, but everyone worked very hard. However Szilard thought he ought to spend his time thinking. He didn't want to stuff uranium in cans and he didn't want to stay up half the night measuring the manganese activity. For these duties, he announced, he had hired a young man by the name of S. E. Krewer, who would do these things better than he could. With this arrangement everything went very smoothly. Krewer was very competent and did all he had to very well.

That experiment was important in a number of ways, but it was the first and also the last experiment in which Fermi and Szilard collaborated. After that a mutually satisfactory arrangement developed in which Fermi and his associates did the experiments while Szilard worked hard behind the scenes to make them possible.

Szilard liked to say, with a twinkle in his eye, that Fermi's idea of being conservative was to play down the possibility that the chain reaction would work; but that his idea of being conservative was to assume it would work and then take all the necessary precautions.

The experiment was important for emphasizing the role of the resonance absorption in the chain reaction. Neutrons were lost in this process that were needed to make the chain reaction go. It was in reviewing what was happening in this experiment that we were led to realize that by lumping the uranium the effect could be reduced.

I remember how it was when the three of us—Fermi, Szilard and I—met together to look at the results. At a certain point it became clear that unless the resonance absorption effect were taken into account in some manner the result would be inconclusive. Our discussion about this proceeded aimlessly for awhile. Then Fermi asked to be excused for about 20 minutes. When he returned he announced that he had made a rough estimate which we then duly recorded in the paper.[11] The episode left us dissatisfied with our knowledge of the resonance absorption effect and I decided then and there to do a study of the resonance absorption for my PhD thesis.

The experiment had another important result. It made it clear that the absorption in hydrogen would be too large to make the chain reaction work with natural uranium.

Considering the possible alternative both Fermi and Szilard began to think that graphite had attractive possibilities. This idea emerged in an exchange of letters that took place just after Fermi went off to spend the summer at the University of Michigan. On July 8, 1939, less than a week after our paper had been sent to the *Physical Review*, Szilard wrote that he had come to the conclusion that a large scale experiment using carbon for slowing down the neutrons ought to be started without delay. It was a gamble, but the other possibilities looked less practical at the time. The problem was where to get money for the graphite.

Szilardian Approach

This was the kind of problem Szilard liked. Fermi, on the other hand, was not very good at the kind of promotion this required. As a matter of fact, Fermi had gone to Washington in March in an attempt to alert the government to the implications of atomic energy. Introduced by a letter from Pegram, Fermi

The famous letter, signed by Einstein but written by Szilard, was delivered personally to President Roosevelt in October 1939.

had talked to Admiral S. C. Hooper and a group of Navy men. However, no action followed, very likely because of his and Pegram's cautious language. As a summer respite, Fermi turned his attention to an interesting problem in cosmic rays, to calculate how the absorption of mesotrons depended on the state of condensation of the matter they traversed.

Szilard's approach was more elaborate and more successful. He convinced himself that with graphite a chain reaction using natural uranium would work. Hard evidence was lacking but he simply argued that it would be too risky to make any other assumption. He estimated he would need about $10,000 for the graphite. Since this was much more than he could hope to get from any university, he thought of going to the government for support, especially in view of the military implications.

Thinking that the best approach was to go directly to the top, Szilard acted on a suggestion from Gustav Stolper, a Viennese economist and friend of long standing. He went to see Alexander Sachs, a Lehman Corporation economist, reputed to have ready access to the White House. Sachs asked for a letter from Einstein to present to the President. The famous letter, signed by Einstein but written by Szilard, was delivered personally to President Roosevelt in October 1939. In the letter the possibility of a chain reaction was taken to be "almost certain." The possibility of extremely powerful bombs was presented as being "less certain" but highly dangerous if developed first for Nazi use. As the interview drew to a close Roosevelt showed he had understood what was wanted. "Alex," he said, "what you are after is to see that the Nazis don't blow us up." The letter was marked for action.

Nothing happened at first, then after a few weeks Sachs called. A meeting had been arranged with Lyman J. Briggs, the director of the National Bureau of Standards and two ordnance specialists, Colonel Keith R. Adamson of the Army and Commander Gilbert C. Hoover of the Navy. The officials met with the three Hungarian scientists—Eugene Wigner, Edward Teller, and Leo Szilard. After the case was presented, the question of money arose. Szilard thought $6,000 would suffice for the test of graphite he had in mind. There followed a long declamation from the Army representative about the nature of war. In the end he argued it wasn't weapons that won wars, but the morale of the troops. Then Wigner, in his very polite manner, interrupted him. He said, in his high pitched voice that it was very interesting to hear this. He had always thought that weapons were very important and that this is what cost money, that this is why the Army needed such a large appropriation. But he was interested to hear that he was wrong: it's not weapons but morale which wins the wars. If this was correct, perhaps one should take a second look at the budget of the Army, maybe the budget could be cut. Colonel Adamson wheeled around to look at Wigner and said, "Well, as far as those $6,000 are concerned, you can have it."[12]

That money was a foot in the door. It was an official recognition that the support of science was not only a federal responsibility but a necessity as well. The government could not overlook a development that threatened its survival. World War II had broken out in Europe. The atmosphere was full of worry and danger. It was not a time to procrastinate. As a result of Szilard's action we would get our graphite, and the federal coffers would be pried open, wider and wider, as the needs of "big" science came to be recognized.

While Szilard was busy bringing about miracles in Washington, I was occupied at the cyclotron, measuring the resonance absorption of uranium. Neutrons absorbed by uranium in this way were lost to the fission chain, and the measurements would make it possible to estimate what proportions of uranium and graphite would minimize this loss. It was also recognized that while neutrons captured in this way did not produce fission, they did produce plutonium-239, a new and relative stable isotope. According to Bohr and John Wheeler's theory, this isotope being of odd atomic weight would behave like the isotope uranium-235 with respect to fission. Thus plutonium-239 was a likely candidate for slow neutron fission.[13] As a nuclear explosive, its behavior could be expected to be quite similar to uranium-235. If a chain reaction were made to work with natural uranium, a highly fissionable material could be made without isotope separation. Thus, the chain reaction was a way to the bomb. This became the decisive factor in the support of its development. Moreover, for energy production, the production of plutonium was a way of converting the abundant isotope uranium-238 into a useful nuclear fuel. A careful balance was required however. Too much resonance absorption and the chain reaction wouldn't work; too little, and the production of an important product would be cut. Of course, in my thesis I emphasized only the fundamental physics involved.

Later, when it became important to have more detailed information about the resonance absorption we went to Princeton. There a group under H. D. Smyth, principally John Wheeler and Eugene Wigner, had been doing theoretical studies of the chain reaction. Princeton had a cyclotron and two young physicists, Robert R. Wilson and Edward C. Crentz, who seemed anxious to work with Fermi and me on this problem. The occasion brought us in close contact with Eugene Wigner who gave our results his sharpest scrutiny.[14]

I had already received the galley proof from the *Physical Review* of my thesis paper when it was decided to withhold it from publication for the duration of the war. Szilard was responsible for this. He

had been promoting the idea that the uranium research should be kept secret. The destructive possibilities of the chain reaction were very real to him. He was deeply concerned that the research might go forward more rapidly in Germany. Nuclear weapons in Nazi hands before ours would be a world disaster. It was very important that they should not know of our progress or even of our interest. Szilard's earlier attempt to persuade Joliot and his group in France to withhold their results from publication failed, but now France was under the threat of the Nazi army.

To set the pattern of withholding papers from publication, at least in the *Physical Review*, Szilard needed a paper to withhold. For this, my thesis on the resonance absorption in uranium would serve perfectly. A guarantee was given and a deposit of $75 made to the library of Columbia University by Dean Pegram. The ultimate publication of my thesis was thereby assured, and I was able to obtain my doctoral degree. Thereafter, those who submitted papers dealing with uranium research to the *Physical Review* could be asked to withhold them from publication by the editors.

I worked on the measurements during the summer and all of the fall of 1939. Fermi was occupied with his calculations on the ionization loss of mesotrons. Szilard was busy tracking down the graphite he wanted us to measure. As the months rolled by I became more and more deeply involved. The number of measurements that could be made to do the job really right seemed endless. Then in January 1940, Fermi completed his calculations. Looking over what I had done, he decided that I had come far enough, so I quickly wrote up what results I had and joined him again.

Graphite Measurements

About this time Leo Szilard's efforts to procure enough graphite for a test of its neutron absorption properties began to bear fruit. Cartons of carefully wrapped graphite bricks began to arrive at the Pupin Laboratory until one and one-half tons had come, enough for the experiment. Fermi returned to the chain reaction problem with enthusiasm. This was the kind of physics he liked best. Together we stacked the graphite bricks into a neat pile. We cut narrow slots in some of the bricks for the rhodium foil detectors we wanted to insert, and soon we were ready to make measurements.

The rhodium foils were Fermi's favorite neutron detectors. He had used them in his early experiments in Rome. The radioactivity induced in rhodium by slow neutrons has a quite short half-life, 44 seconds. This leaves very little time after they have been irradiated to get them under the Geiger counter for measurement. The Geiger counter had to be separated from the neutron source and was installed in Fermi's office some distance down the hall from the room with the graphite pile. A precise schedule was followed for each measurement. With the rhodium in place in the graphite, the source was inserted in its position inside the pile and removed after a one minute exposure. To get the rhodium foil under the Geiger counter in the allotted 20 seconds took coordination and some fast legwork. The division of labor was typical. I removed the source on signal; Fermi, stopwatch in hand, grabbed the rhodium and raced down the hall at top speed. He had just enough time to place the foil carefully into position, close the lead shield and, at the prescribed moment, start the count. Then with obvious satisfaction of seeing everything go right, he would watch the flashing lights of the scaler, tapping his fingers on the bench in time with the clicking of the register. Such a display of the phenomenon of radioactivity never failed to delight him.

The results of this work had the greatest significance for the uranium project. Szilard's gamble had paid off. The absorption of neutrons in graphite was

Although a few scientists like Szilard were convinced that atomic bombs were feasible, most had doubts that atomic power would come in time to affect the war.

small enough to make it the obvious choice of material for slowing down the neutrons in the chain reaction. Moreover, the basic theoretical techniques for describing the behavior of neutrons in such circumstances were set forth. Thus was Fermi's "age theory." It came to enjoy wide usage in the Uranium Project. The graphite pile method became the standard way to test all the subsequent batches of graphite which came in ever increasing numbers as the work proceeded.

After the success of the graphite measurements we found ourselves waiting again for larger amounts of graphite and uranium. This gave us an opportunity to study the fission process itself in some detail. Although a large number of radioactive species had been found among the fission products, most of the work had been confined to the identification and the genetic relationships among them. The quantitative aspects of the relationships were lacking. We wanted to know the branching ratio, the fraction of fissions which gave rise to a given radioactive series.[15]

The work went forward along these lines because an able radiochemist, A. V. Grosse, had come to Columbia on a Guggenheim fellowship to participate in the new work. It was partly Grosse's enthusiasm and buoyant personality as well as the fact that the cyclotron was working well that made us think this research would be the thing to do. It was a good arrangement. Grosse devised the methods of radiochemical separation, and these were carried out by Fermi and me. What amused Grosse was seeing his physicist colleagues turning into chemists. It was fun watching us work, he said later. It looked as if I, the assistant, was doing the supervising while Fermi, the famed Nobel laureate, was doing most of the hard work.

A great many ether separations were carried out. This involved boiling away fairly large quantities of ether. Fermi had none of the requisite patience of a good chemist and tried to speed this process beyond what was prudent. Then there would be an explosion and the ether would burst into flame. Fermi would step back startled, his eyebrows singed, but he wasn't deterred. Later, when the ether method became an important way to purify uranium, Fermi could give some cautionary advice.

It became clear from the measurements on uranium and graphite that the chain reaction in a natural uranium-graphite system might be possible but only by exercising the greatest care in guarding against undesirable losses of neutrons. In particular, the loss of the neutrons by leakage from the confines of the structure could only be reduced sufficiently by making a very large structure. In order to determine whether a larger structure would work, Fermi invented the exponential experiment. The scheme was similar to that used to measure the absorption of graphite. In a rectangular column, now made up with the uranium-graphite lattice, a neutron source was placed near the base. The neutron density was then measured along the height of the column. By determining whether the exponential decrease in the neutron density was less than or greater than that expected from leakage, it was possible to determine the value of the reproduction factor and whether it was greater than or less than one.

The accuracy of this experiment increases with the size of the column, so it was necessary to wait until a fairly large amount of graphite and uranium oxide could be obtained. A new grant of $40,000 was obtained from the Uranium Committee, and by the end of September 1941 enough material had arrived to permit a definite test, using a column 8′ by 8′ at its base and 11′ in height. This was too big a pile for any of the rooms available in Pupin, so Pegram found us a large room in Schermerhorn Hall.

We were faced with a lot of hard and dirty work. The black uranium oxide powder had to be packed in cubical tin cans 8″ on a side. The uranium had to be heated to drive off undesired moisture and then packed hot in the containers and soldered shut. To get the required density, the filling was done on a shaking table. Our little group, which by that time included Bernard Feld, George Weil, and Walter Zinn, looked at the heavy task before us with little enthusiasm. It would be exhausting work. Fortunately, Fermi managed to recruit some members of the Columbia football squad to assist in this. In those days football players were expected to earn some of their support by doing useful work for the University. It was a pleasure to see them work; they made it seem easy. Fermi tried to do his share of the work; he donned a lab coat and pitched in to do his stint with the football men, but it was clear that he was out of his class. The rest of us found a lot to keep us busy with measurements and calibrations that suddenly seemed to require exceptional care and precision.

The result of the measurement was $k = 0.87$ for the reproduction factor. This was appreciably less than 1.0, but it was possible to think of enough improvements in purity, geometry, and density of uranium to believe that the prospects for a k larger than one were fairly promising.

While all this was going on high level committees were busy discussing how much support and emphasis our work should be given. In the spring of 1941, the National Defense Research Committee asked the National Academy of Sciences to appoint a special committee to review the military importance of the uranium work. Until then, although a few scientists like Szilard were convinced that atomic bombs were feasible, most members of the Uranium Committee were interested in the controlled chain reaction and had doubts that atomic power would come in time to affect the current war.

The opinion of the National Academy Committee evolved rapidly as information from those working in the field became available to them. In May the first cautious report placed the emphasis on power and discussed the difficulties of separating enough uranium-235 for a bomb. The second report in July revealed the work on plutonium and mentioned the possibility of a plutonium bomb. Shortly afterwards the outlook for a uranium-235 bomb brightened, owing both to progress in isotope separation and to information received from the British. In its third report the committee saw in the chain reaction not only its possibility for producing power but also, more important for the immediate emergency, its application as a producer of plutonium—a likely competitor to uranium-235 as a material from which atomic bombs might be made.

It was time to push the uranium work vigorously. On December 6, 1941, the National Defense Research Committee announced an "all-out" effort. The next day brought the attack on Pearl Harbor, and immediately afterwards the United States entered the war against Japan, Germany and Italy. If atomic weapons were at all feasible, it was considered essential that the United States should have them first, before the Nazis.

It was under these circumstances that the Metallurgical Laboratory was organized. It operated first under the Office of Scientific Research and Development and six months later under the United States Army's Corps of Engineers, Manhattan District, together with the uranium-235 separation projects. The stated purpose of the Metallurgical Laboratory was first to develop the chain reaction with natural uranium and second to use this to produce plutonium.

The choice of a place and a leader for the Metallurgical Laboratory was decided by practical considerations. Fermi would have liked to pursue his uranium work at Columbia. But Columbia was already engaged in two different uranium separation projects, one headed by Harold Urey and the other by John Dunning, and was thus hesitant to underwrite a third project in the same general area. With Italy at war with the United States, Fermi, still an Italian national, was an enemy alien and not eligible to hold major responsibilities. Arthur H. Compton of the University of Chicago, who had served as head of the National Academy Committee, was chosen director. The enterprise needed a forceful spokesman who could organize a large scale project and be effective at high levels in the government. Compton was just right for this. He decided to make Chicago the center of the work. Samuel K. Allison was already working on the problem there and ready to join forces with us. Fermi, more than a little unwilling, could hardly do otherwise than take his little group to Chicago to continue what he had started at Columbia.

"Fermi tried to do his stint with the football men, but he was out of his class."

The Move to Chicago

The move to Chicago began in the early spring of 1942. The United States was now heavily engaged in the war, the outcome of which appeared very uncertain. The work of the Metallurgical Laboratory acquired a great sense of urgency and was given high priority. It grew rapidly in size and number of personnel, and its work was classified *Secret.* Fermi's little group of physicists was quickly outnumbered by the influx of many other groups: chemists to work out the chemistry of the fission products and the separation of plutonium; engineers to design the plants; metallurgists to fabricate uranium metal; and even doctors and biologists to study the effects of radiation and to recommend safeguards against such hazards. These groups were set within an organizational framework that tried to push the work forward rapidly but under strict rules of military security.

It was intended originally to construct the pile in the Argonne Forest outside of Chicago. Some construction had started, but around October 20 labor difficulties arose. Since it was clear that we would be ready to assemble the pile before the building was completed, Fermi became concerned about a serious delay. He went to Compton to tell him that he believed he could make the chain reaction work safely right in Chicago.

Compton said, "Let's hear your analysis." When Compton was satisfied, he agreed. But he did have this reservation:[16]

> The only reason for doubt . . . was that some new, unforeseen development might develop under the conditions of release of nuclear energy of such vastly greater power than anyone had previously handled. . . . And after all, the experiment would be performed in the midst of a great city. We did not see how a true nuclear explosion, such as that of an atomic bomb, could possibly occur. But the amount of potentially radioactive material present in the pile would be enormous and anything that would cause excessive ionizing radiation

It was a great temptation for me to pull the strip and be the first to make a pile chain react. But Fermi had made me promise that I would insert and lock all cadmium rods in place, go to bed, and nothing more.

in such a location would be intolerable.

The outcome of the experiment might thus greatly affect the city. As a responsible officer of the University of Chicago, according to, every rule of organizational protocol, I should have taken the matter to my superior. But that would have been unfair. President Hutchins was in no position to make an independent judgment of the hazards involved. Based on considerations of the University's welfare the only answer he could have given would have been—No. And this answer would have been wrong. So I assumed the responsibility myself. In the building under the west stands of the Stagg Athletic Field was a squash court. I told Fermi to use this room and go ahead with the critical experiment. . . .

Actual assembly of the pile began only after the decision was made on November 14 to build it in the squash tennis court under the West Stands. Other parts of the West Stands had already been in service for the series of exponential pile experiments that were carried out in the beginning under the direction of Martin Whittaker, then under Zinn, and in the end under Zinn and me jointly. The exponential piles were used to find the best lattice dimensions, to test the various batches of uranium, and to study the effect of adding other material that might be required in a high power reactor.

Planning for Chicago Pile #1 began early in July as soon as it became evident that sufficient purified uranium and graphite would be delivered by November; Norman Hilberry had carried through a remarkably successful procurement effort. It was necessary to allow time for exponential experiments in sufficient number to tie down the design of the final lattice. The record shows that the groups of Anderson and Zinn working together built and measured as many as 16 exponential piles in the two month period between September 15 and November 15.

To make the best use of the material that would be available, it was decided to build the pile in a spherical shape and to make provision for substituting carbon dioxide for air within the structure. We decided to mount the pile on a cradle of wood timbers and to enclose the whole structure within a balloon cloth tent.

I spent the summer visiting the lumber yards around Chicago and contracting for an awesome number of 4″ x 6″ timbers. I remember the astonishment with which my inquiry was received at the Sterling Lumber Company which became a main supplier. For the balloon cloth enclosure I went to the Goodyear Rubber Company in Akron, Ohio. The company had a good deal of experience building blimps and rubber rafts but a square balloon 25′ on a side seemed a bit odd to them. In wartime a lot is done with no questions asked. I had good credentials and a high priority rating, and that was good enough for them. They built the balloon in short order to my specifications, suppressing a justifiable curiosity.

For the construction of the pile Fermi assigned the responsibility jointly to Zinn and me. Our two groups combined for a concerted effort. Two special crews were organized: one machined the graphite, the other pressed the uranium oxide powder using specially made dies in a large hydraulic press. Both crews managed to keep their output up to the rate of the deliveries. Thus in our report for the month ending October 15, Zinn and I could state that 210 tons of graphite had been machined. A separate group under Volney C. Wilson was in charge of control and measuring devices.

On Monday, November 16, we opened the rubberized balloon cloth envelope and started erection of the pile inside it. We organized into two shifts: Wally Zinn took the day shift, mine was the night shift.

The frame supporting the pile was made of wooden timbers. Gus Knuth, the millwright, would be called in. We would show him by gestures what we wanted, he would take a few measurements, and soon the timbers would be in place. There were no detailed plans or blueprints for the frame or the pile. Each day we would report on the progress of the construction to Fermi, usually in his office in Eckhart Hall. There we would present our sketch of the layers we had assembled and indicate what we thought could be added on the following shifts. Since some of the graphite was of better quality than the rest, it was important to arrange its disposition carefully. Fermi spent a good deal of time calculating the most effective location for the various grades of graphite on hand.

A particularly difficult point was where to put the uranium oxide and where the uranium metal. We knew that because of its higher reproduction factor the metal should be in the central part of the pile, but we had to decide at what layer to begin to install it at a point fairly near the actual center. But then a substantial amount of uranium metal of high quality arrived from Frank Spedding's group in Ames, Iowa, after the construction was well underway. The plan was changed immediately to take advantage of the improvement this would give. We ended up with a metal core neither spherical nor central, but it didn't matter.

The details for constructing the pile were determined day-by-day at those meetings in Fermi's office. One important detail was the location of the cadmium control strips. These were needed to keep

When the switch was made, everyone waited in the sudden silence. Everyone realized the significance of that switch.

the pile from becoming too reactive once it began to approach the critical size. We wanted a number of control rods distributed widely in the structure. This meant that some had to be installed at a rather early stage. A simple design for a control rod was developed which could be made on the spot: cadmium sheet nailed to a flat wood strip was inserted in a slot machined in the graphite for this purpose. The strips had to be inserted and removed by hand. Except when the reactivity of the pile was being measured, they were kept inside the pile and locked using a simple hasp and padlock, the only keys to which were kept by Zinn and myself. One special, particularly simple, control rod was built by Zinn; it operated by gravity through weights and a pulley and was called "Zip." It was to be pulled out before the pile went into operation and held by hand (Zinn's) with a rope. In case of an emergency or if Zinn collapsed, the rope would be released and Zip would be drawn into the pile by gravity.

Once the 15th layer had been reached, we introduced the practice of measuring the neutron activity at a fixed point in the structure. We did this with a boron trifluoride counter once the construction quota had been filled at the end of each shift. Each day the measurements of the activity of this counter were reported to Fermi who used it to improve his estimate of how much bigger the pile would have to be. Thus, we always had a good idea of how much more we had to do.

As the pile grew the estimate of its critical size became increasingly accurate. Thus, we could tell that on the night between December 1st and 2nd, during my shift, the 57th layer would be completed and the pile could be made critical. That night the construction proceeded as usual with all cadmium rods in place. When the 57th layer was completed, I called a halt to the work in accordance with the agreement we had reached in the meeting with Fermi that afternoon. All the cadmium rods but one were then removed and the neutron count taken, following the standard procedure which had been followed on the previous days. It was clear from the count that once the only remaining cadmium rod was removed, the pile would go critical. It was a great temptation for me to pull the final cadmium strip and be the first to make a pile chain react. But Fermi had anticipated this possibility. He had made me promise that I would make the measurement, record the result, insert all cadmium rods, lock them all in place, go to bed, and nothing more. The next morning, December 2, was, as Wally Zinn remembers it:[17]

> It was a very cold day. To those of us who worked in the West Stands, cold was not a new experience. That gloomy structure with its high stacks of graphite bars filling all corridors, stairwells, and wherever 500 tons of the black stuff could be stored was completely unheated. Perhaps the importance of our jobs had something to do with it, but we really worked fast to keep warm. To help, we tried charcoal fires in empty oil drums—too much smoke. Then we secured a number of ornamental, imitation log, gas-fired fireplaces. These were hooked up to the gas mains, but they gobbled up the oxygen and replaced it with fumes which burned the eyes. The scientists and technicians could use physical activity to keep warm, but the security guards had to stand in one place at the entrances. The University of Chicago came to the rescue. Years before, big league football had been banned from the campus; we found in an old locker a supply of raccoon fur coats. Thus, for a time we had the best dressed collegiate-style guards in the business.

I was on hand, bright and early, to tell Fermi that all was ready. He took charge then.

Fermi had prepared a routine for the approach to criticality. The last cadmium rod, attended by George Weil, was pulled out step by step. At each step a measurement was made of the increase in the neutron activity, and Fermi checked the result with his prediction, based on the previous step. That day his little six-inch pocket slide rule was busy for this purpose. At each step he was able to improve his prediction for the following. The process converged rapidly, and he could make predictions with increased assurance in their accuracy. When he arrived at the last step, Fermi was quite certain that he could make the pile go critical.

When the cadmium rod was pulled out to the position he asked for next, the increase in neutron intensity was noticeably quickened. At first you could hear the sound of the neutron counter, clickety-clack, clickety-clack. Then the clicks came more and more rapidly, and after a while they began to merge into a roar; the counter couldn't follow anymore. That was the moment to switch to a chart recorder. But when the switch was made, everyone watched in the sudden silence the mounting deflection of the recorder's pen. It was an awesome silence. Everyone realized the significance of that switch; we were in the high intensity regime and the counters were unable to cope with the situation anymore. Again and again, the scale of the recorder had to be changed to accommodate the neutron intensity which was increasing more and more rapidly. Suddenly Fermi raised his hand: "The pile has gone critical," he announced. No one present had any doubt about it. Then everyone began to wonder why he didn't shut the pile off. But Fermi was completely calm. He waited another minute, then another, and then when it seemed that the anxiety was too much to bear, he ordered "Zip in!" Zinn released his rope and there was a sigh of relief when the intensity dropped abruptly and obediently to a more modest level. It was a dramatic demonstration that the chain reaction worked.

No cheer went up, but everyone had a sense of excitement. They had been witness to a great moment in history. Wigner was prepared with a bottle of Chianti wine to celebrate the occasion. We drank from paper cups and then began to say things to one another. But there were no words that could express adequately just what we felt.

Only 42 persons were present at the experiment; they were mostly the scientists who had done the work. But there was also Crawford Greenewalt of the du Pont Company. His judgment would be critical for the du Pont Company to build the plutonium production piles. For him the demonstration was impressive; it was this performance that convinced him they should.

Those present were: H. M. Agnew, S. K. Allison, H. L. Anderson, H. M. Barton, T. Brill, R. F. Christy, A. H. Compton, E. Fermi, R. J. Fox, S. A. Fox, D. K. Froman, A. C. Graves, C. H. Greenewalt, N. Hilberry, D. L. Hill, W. H. Hinch, W. R. Kanne, P. G. Koontz, H. E. Kubitschek, H. V. Lichtenberger, G. Miller, G. Monk, Jr., H. W. Newson, R. G. Nobles, W. E. Nyer, W. P. Overbeck, H. J. Parsons, G. S. Pawlicki, L. Sayvetz, L. Seren, L. A. Slotin, F. H. Spedding, W. J. Sturm, L. Szilard, A. Wattenberg, R. J. Watts, G. L. Weil, E. P. Wigner, M. H. Wilkening, V. C. Wilson, L. Woods, and W. H. Zinn.

West Stands to Argonne

The original pile in the West Stands, dubbed CP-1, had a short life. It lived for three months, but those were three very active months for the first chain reactor. It turned out to be a marvelous experimental tool. Its sensitivity for neutron absorption and production was beyond the wildest dreams of those of us who had struggled so hard to make such measurements before. It was, in fact, a neutron multiplier of almost unlimited power. Change the number of neutrons a little and soon the effect would be multiplied by a million times or more. The sensitivity was, in fact, limited by even rather slight changes in the pressure and temperature of the air inside. The temperature coefficient of the reproduction factor, long an unanswered and difficult problem, was now measured precisely with the greatest of ease by the simple expedient of opening a window to admit some of the cold outside air to cool the pile. The pile became an indispensible device for the design of its successors.

The building at the Argonne site was now complete, and we had a whole group of young and eager engineers from du Pont to help us. They had come to Chicago to be indoctrinated in the new art. Having a good idea of what new features ought to be installed, we did not hesitate to disassemble and rebuild the pile at the Argonne site. The rebuilt pile was named CP-2. We made it into a marvelous source of thermal neutrons and a precise instrument for measuring neutron activity.

The important assignment of the Argonne pile was to test various aspects of the plutonium production piles that were to be built at Hanford. One of the first jobs was to design and test a suitable radiation shield, a project carried out by Fermi and Zinn and completed in just two weeks after CP-2 had been put into operation. A regular program of uranium metal testing was set up, and tests were made of control rods and the neutron characteristics of the lattice. At the same time a number of physics experiments, for which the pile provided an unusual opportunity, were squeezed in. One special feature was the "thermal column." This was a graphite column, set on top of the pile, from which thermal neutrons in considerable number would be emitted essentially free of neutrons of higher energy.

By the summer of 1943 the work at the Argonne site was in full swing. The concerted effort in which many of us had joined to make the chain reaction a reality was now behind us, and the various groups began to undertake a widening variety of activity using the chain reacting pile as a research facility. The scheduling this required anticipated the operation of the great high energy accelerators of later years. Fermi was particularly active in the experiments during this period, working closely with John Marshall and Leona Woods.

The main task of the Argonne lab was the support of the Hanford design. It was a training ground for the young du Pont engineers who would later go to Hanford; it answered technical questions that arose as the design developed, and it stood prepared to solve problems that might arise as the Hanford reactors were placed into operation. At this time Crawford Greenewalt asked me to join the du Pont Company in Wilmington to help guide the design. I spent most of my time in Wilmington for the next six months.

The design of the plutonium production piles was the responsibility of Eugene Wigner who had assembled a remarkably good group of physicists and engineers. They had evolved good design and the du Pont Company was given the task of building it and making it work. At Chicago there was a good deal of skepticism whether du Pont could perform as required. The task was enormous, the time was short, and there was neither experience nor knowledge in the new technology. But in Wilmington I discovered how such miracles were performed. The du Pont Company was highly organized and very wisely managed. There were many competent men with a degree of loyalty and dedication I had not appreciated. The lack of knowledge and experience which worried us at Chicago was managed by a group of men with exceptional skill in putting questions to and obtaining answers from those who knew more. From Crawford Greenewalt, I learned that the du Pont Company put down $750 million worth of construction in about two years time, a feat then never equalled before or since. The reactors operated well and produced enough plutonium in time to bring the decisive end to the war we were after. I left du Pont with the greatest admiration for them and for the men with

The successful Trinity test was the climax of the Los Alamos wartime period.

whom I worked—Crawford Greenewalt, Hood Worthington, and Dale Babcock—and for the remarkable performance they put on altogether.

In the summer of 1944, J. Robert Oppenheimer, the scientific director of "Project Y" at Los Alamos, came to Chicago several times to persuade Fermi to move to Los Alamos. Fermi had been there for the first time in April 1943, when the project was beginning to function. He returned there on other occasions but now he was wanted full time, not for any specific assignment but because of his general wisdom. He planned to move to Los Alamos in August, but at the last moment he was called to Hanford for the starting of the pile. Then there was the problem of the xenon poisoning that he stayed to solve. Finally, in September 1944 he moved to Los Alamos where he remained through December 1945.

Upon his arrival, Fermi became the leader of the especially organized F (for Fermi) Division whose general responsibility was to investigate problems that did not fit into the routine of work of the other divisions. Four groups were placed in the F Division. One, under Edward Teller, pursued the theoretical study of the "Super" (the hydrogen bomb), in which Teller had been engaged for some time. Egon Bretscher headed another group on the "Super," which was the experimental counterpart of Teller's. Fermi's greatest personal participation was in the other two groups: the Water Boiler group under L. D. P. King, and the group under me called F-4. The F-3 group included several young people, among them Joan Hinton a student in physics and an Olympic skier who later left the United States to work in communist China. The F-4 group consisting initially of just Darragh Nagle, Julius Tabin, and myself was organized when we came together to Los Alamos from Chicago in November 1944. Fermi was again surrounded by a new group of young people who joined him with enthusiasm in in work that was really play.

The first Sunday after my arrival in Los Alamos, Fermi asked me to join him on a long hike up one of his favorite mountain trails within easy access of Los Alamos. That hike turned out to be a four hour lecture. I was treated to a comprehensive review of what Los Alamos was all about, who was doing what, how far they had come, and what the problems were. By Monday morning I felt I belonged to the place.

An example of what new and different things could be done was the water boiler that Perc King was building. It was an extremely simple nuclear reactor, and though small in size (only one foot in diameter) it could serve as a strong source of neutrons. It used enriched uranium in water solution, a novel product of the new "atomic" technology, undreamed of only six years earlier.

My group—Darragh Nagle, Julius Tabin, and I—had an assignment to help construct the water boiler, but it was considered uncommitted and available for other problems that might come up. Not long after I arrived, I was invited to attend a meeting of the Research Council of which Fermi and other division leaders were members. Non-members like myself were invited occasionally to hear what problems were under discussion. The idea was to widen the exposure of the problems to those who might see a solution and be willing to do something about it. Thus, when the problem arose of how to determine the critical size of the uranium-235 bomb, I proposed that my F-4 group undertake experiments to measure the fissions produced in a uranium-235 sphere using the neutrons from the water boiler. Fermi had a similar idea, but our methods differed in the details. I wanted to use a fission chamber for a detector; Fermi wanted to catch the fission fragments on cellophane foils. We were both encouraged to go ahead, Fermi with Joan Hinton's help, I with Nagle's and Tabin's. Both measurements were successful and gave similar results. The design of the uranium-235 bomb could now go forward with the added assurance of hard experimental numbers.

The experiments on criticality took up January and February 1945. In March it was recognized that no amount of experimental work would yield as much information as an actual explosion, and plans were made for such a test, under a code name of "Project Trinity."

I found myself again invited to the evening meetings of the Research Council. This time the problem under discussion was how to measure the efficiency of the plutonium bomb, the one that was going to be used at the Trinity test. Throughout the first meeting, I listened. When I came to the next meeting I had a suggestion, one that was very different from the other proposals that were being considered. "Why not," I argued, "use a radiochemical method to measure the efficiency? When the bomb exploded the fission products from the reaction as well as the unburned plutonium would be deposited on the ground. A comparison of the characteristic radiations of each, coupled with a knowledge of the branching ratios of the fission products would give directly the fraction of the plutonium burned. It was natural for me to think of the fission product branching ratios because of the work I had done earlier with A. V. Grosse and Fermi. My proposal was accepted, and I immediately went to work on it.

Nagle and Tabin were there to help, of course. But chemists were needed to do the rather elaborate radiochemistry involved. I persuaded Nathan Sugarman to come to Los Alamos from Chicago to do the job. He brought along some of the excellent radiochemists in his group, Seymour Katkoff, Lester Winsberg, and Don Engelkemeir among them. I needed a chemistry lab and got the one that had just been completed for Segre, to his great chagrin.

The method worked well. Army tanks were converted to pick up the samples of the dirt from under the exploded bomb, and accurate measurement of the efficiency of the bomb was obtained. The result was important. It helped decide at what height the bomb should be exploded. While we were analyzing our samples Oppie came to see us every day, he was so anxious to have our results. It was hard to tell him to be patient. Later the principle of our experiment became the basis of the method for the long range detection of nuclear explosions, using air samples instead of those from the ground.

The Trinity test was made at Alamogordo in the desert land of southern New Mexico on July 16 after long preparations. I had persuaded Fermi to come down from Los Alamos and to give us a hand with the tanks we had prepared to gather samples of radioactive dirt after the explosion. At the moment of the explosion he stood with the others at an observation point some 10,000 meters away from the steel tower supporting the atomic device. He later related that he did not hear the sound of the explosion, so great was his concentration on the simple experiment he was performing: he dropped small pieces of paper and watched them fall. When the blast of the explosion hit them, it dragged them along, and they fell to the ground at some distance. He measured this distance and used the result to calculate the power of the explosion. His results turned out to agree well with those obtained with more elaborate preparation, including ours.

The successful Trinity test was the climax of the Los Alamos wartime period. Within a month two atomic bombs were dropped on Japan with devastating effect. They brought the war to an abrupt end.

Suddenly, we lost all interest in bombs. Our thoughts turned to the universities and the research and teaching we could do. Fermi joined the newly formed Institute for Nuclear Studies at the University of Chicago. Soon after I accepted an offer of Assistant Professor there.

Before leaving Los Alamos I decided to exploit my knowledge of the Hanford chain reactors. They could be used to make new isotopes and I devised a scheme to produce tritium and helium^3 in substantial quantity. The actual production was done at Chicago with the help of Julius Tabin and Aaron Novick; we measured their nuclear magnetic moments. Later at Chicago we would build a great cyclotron and with it make pions and muons. The pions would tell us about nuclear forces, and we would learn to use muons to probe the nucleus and ultimately the elementary nucleons themselves. Unraveling such mysteries, learning more about man and his world, was what made fighting and winning the war worthwhile. "Fortune favors the prepared mind."

NOTES

1. For an official history of the chain reaction, see R. G. Hewlett and O. E. Anderson, "The New World, 1939/1946," *A History of U.S. Atomic Energy Commission* (University Park, Pa.: Pennsylvania State University Press, 1962).

Other sources drawn on in this narrative include: B. T. Feld and Gertrud Weiss Szilard, eds., *The Collected Works of Leo Szilard,* Vol. I (Cambridge: Massachusetts Institute of Technology Press, 1972); Laura Fermi, *Atoms in the Family* (Chicago: University of Chicago Press, 1954); Gertrud Weiss Szilard and K. R. Winsor, eds., "Reminiscences, by Leo Szilard," *Perspectives in American History,* 2 (1968); E. Amaldi, *et al.*, ed., *Collected Papers of Enrico Fermi,* Vol. II (Chicago: University of Chicago Press, 1965); E. Fermi, *Physics Today* 8 (1955), 12; H. D. Smyth, *Atomic Energy for Military Purposes* (Princeton, N.J.: Princeton University Press, 1945); Arthur H. Compton, *Atomic Quest: A Personal Narrative* (New York: Oxford University Press, 1956), pp. 137-39; and Walter H. Zinn, unpublished speech on the occasion of the 25th anniversary of the first chain reaction, University of Chicago, Dec. 2, 1967.

2. There is some question whether Lord Rutherford actually used the word "moonshine."

3. British Application No. 19157 filed June 28, 1934, issued as Patent No. 630,726; see Julius Tabin, "Patents, Patent Applications and Disclosures (1923-1959)," Part V in *Collected Works of Leo Szilard,* p. 527.

4. Otto Hahn and F. Strassmann, "Über den Nachweis und das Verhalten der bei der Bestrahlung des Urans mittels Neutronen Entstehenden Erdalkalimetalle," *Die Naturwissenschaften,* 27 (Jan. 6, 1939), 11-15.

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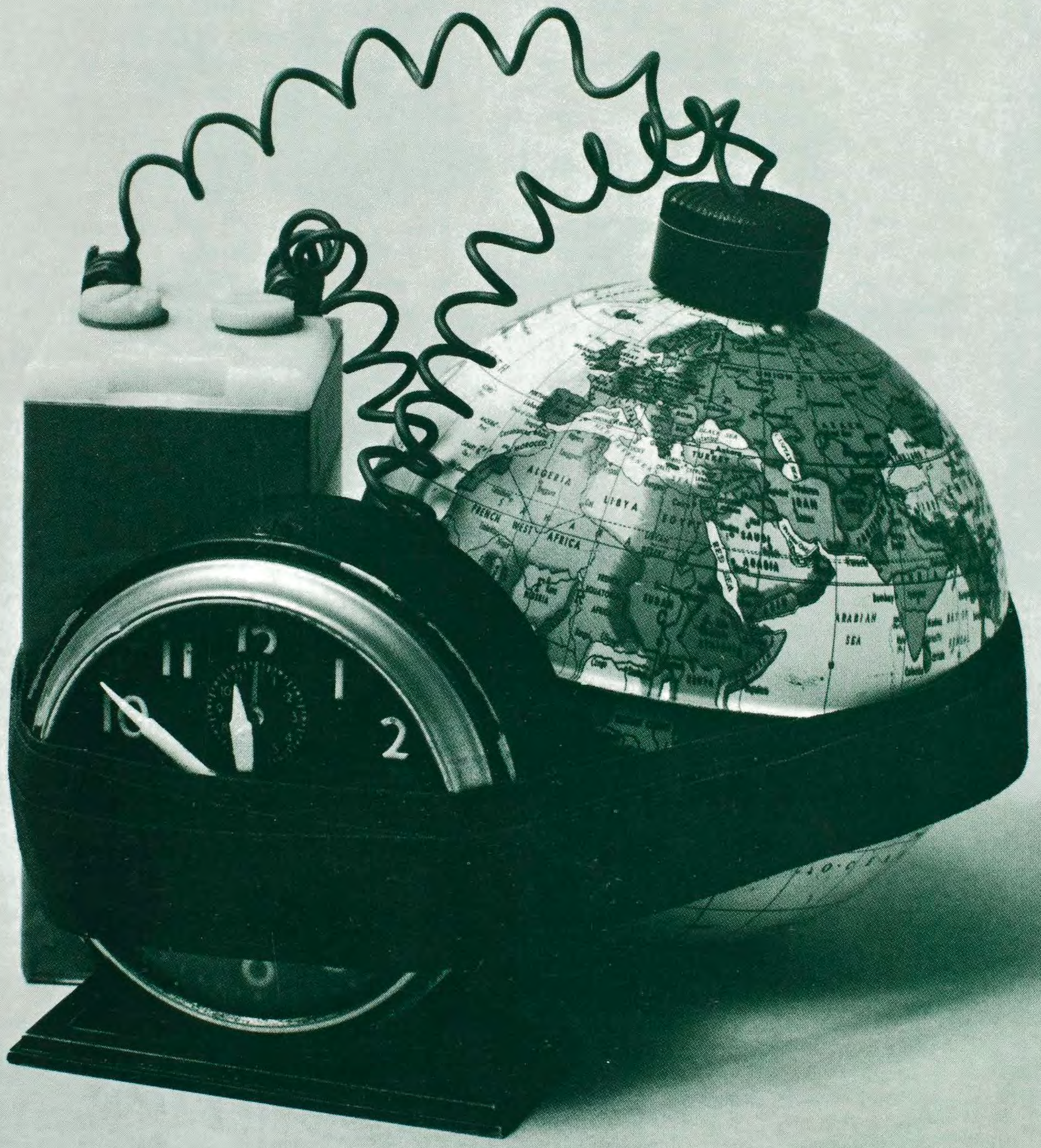

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'ALL IN OUR TIME'

The Legacy of Fermi and Szilard

HERBERT L. ANDERSON

Across the street from my office in the Enrico Fermi Institute of the University of Chicago, a huge piece of sculpture tries to convey more than can be read on the plaque below: "On December 2nd, 1942, Man achieved here the first self-sustaining chain reaction and thereby initiated the first controlled release of nuclear energy." It is a reminder of what happened on a bleak December day that changed the course of history.

In tracing the sequence of events that led to the chain reaction, I began to wonder what it was that selected those who played the principal roles.[1] How did it happen that it was Fermi who built the chain reaction and that it was Leo Szilard who had invented it eight years earlier. What drew these and others, too, so irresistibly to the chain reaction? Is it true, as Pasteur is supposed to have said, that "fortune favors the prepared mind"? If so, it would be one of the best reasons I know for a liberal education.

What prepared Szilard to invent the chain reaction? The simple, albeit incomplete, answer is that he had read H. G. Wells. The development of nuclear energy had been anticipated by H. G. Wells by 30 years. He wrote about it in one of his less well-known books, "The World Set Free," published in 1914. Some of his prophetic vision about what would happen in a world with nuclear energy is still unfolding.

The solid scientific fact that H. G. Wells had at his disposal when he wrote this book was what was known then about natural radioactivity: that uranium disintegrated by emitting alpha particles. This was a process yielding a million times more energy per atom than in ordinary combustion. The trouble was that it took place very slowly. What was needed, H. G. Wells realized, was a way to speed it up. Then, from a pound or two of uranium, enough energy could be obtained to light a great city, power the wheels of industry, drive airplanes and, inevitably, fashion devastating weapons of war. When H. G. Wells wrote his book in 1913, a year before World War I, he put 1933 as the date this essential step would be taken, a date that coincides almost exactly with the actual discovery of artificial radioactivity. Those who knew Szilard would understand instantly

Herbert L. Anderson was a member of the team that built the first atomic pile which resulted in the first self-sustaining (or controlled) nuclear chain reaction on December 2, 1942, at the University of Chicago. His account of those times, the first of two installments, is part of the Bulletin's *continuing series on the reminiscences of nuclear pioneers. Anderson is professor of physics at the University of Chicago and a former director of the Enrico Fermi Institute for Nuclear Studies.*

Fermi had an unusual grasp of physics which he kept at his fingertips always ready for use. . . .The physics just flowed out of his chalk.

why this idea would excite him and why he would keep turning it over and over in his mind until he could figure out what he could do with it.

It was in the fall of 1933 when Szilard found himself in London, a time when many exciting discoveries in nuclear physics were being made. Only the year before the neutron had been discovered by James Chadwick. Irene Curie and her husband, Frederic Joliot, were on the threshold of their discovery of artificial radioactivity.

Szilard read in the newspapers about an annual meeting of the British Association where Lord Rutherford was reported to have said, "Whoever talks about the liberation of atomic energy on an industrial scale is talking moonshine."[2] Such pronouncements by experts, who claim that something cannot be done, can irritate a man like Szilard, and it set him to thinking how he could prove otherwise.

Just after reading Rutherford's comment, Szilard was walking down Southhampton Row and pondering how he might prove Rutherford wrong. He stopped for a traffic light, and when the light changed to green and he crossed the street, it suddenly occurred to him that if he could find an element which when split by one neutron would then emit two neutrons, he could make a chain reaction. Such a chain reaction would be able to liberate energy on a large scale. His candidate for the proper element was beryllium which was thought to have the kind of instability that would emit neutrons when it disintegrated. In the spring of 1934, Szilard applied for a patent which described the laws governing such a chain reaction.[3] It turned out later, however, that beryllium was actually stable and could not sustain a chain reaction in this way.

Because he had read H. G. Wells, Szilard had a vivid conception of what might happen to the world if the great power of nuclear energy were turned to destructive purposes. He wanted it on the record that he had found the way to nuclear power, but he didn't want it to fall into unscrupulous hands. The Szilardian way to manage this was to have it kept secret by assigning the patent to the British Admiralty.

Szilard had to wait five years before a suitable nuclear reaction was found. When the fission of uranium was discovered at the end of 1938, Szilard knew instantly that this is what he had been looking for. Early in January 1939 he was in Princeton, New Jersey, visiting Eugene Wigner, who told him about Otto Hahn's discovery. The imminence of another world war seemed very real to both men and they agreed it was urgent to set up experiments to show whether, in fact, neutrons were emitted in the fission process in uranium. Szilard decided to go, post haste, to Columbia University where Fermi was. Szilard knew that if the chain reaction was to work, Fermi was the man to do it.

The timing is interesting here. Fermi had just been awarded the Nobel Prize for his discovery of artificial radioactivity produced by slow neutrons; and he had taken that occasion to begin his American career. Italy, under Mussolini, bending evermore deeply to the influence of Hitler, had adopted outrageous racial laws that threatened much that was important to him. So he took his family to New York and on January 9, 1939, arrived to take up a professorship at Columbia University.

Fission of Uranium

Just weeks before, on Dec. 22, 1938, Hahn and Strassmann had made their discovery of the fission of uranium.[4] The news of their discovery came to the United States via Niels Bohr, who had heard about it from Otto Frisch. Frisch, in turn, had gotten it from Lise Meitner, who while in Sweden had received a letter about it from her former collaborators in Germany who had done the work. The news was particularly exciting to Bohr because the fission of uranium seemed to be a beautiful and dramatic confirmation of his idea that the nucleus behaved like a liquid drop. He could have predicted it, but he hadn't. Bohr was so excited about this discovery that when he arrived in New York, on Jan. 16, 1939, he just had to tell it to someone. Frisch had asked Bohr not to let the cat out of the bag until he had a chance to do the decisive experiment that would demonstrate the energy release directly, but it was hard to restrain Bohr for very long.

Bohr went to Princeton, but a few days after he had settled there he came through New York on his way to Washington. He was anxious to see Fermi's reaction to his great news. Fermi wasn't in his office at Columbia, so Bohr went down to the basement where the cyclotron was. Fermi wasn't there either, but I was. Undeterred, he came right over and grabbed me by the shoulder. Bohr doesn't lecture to you, he whispers in your ear. "Young man," he said, "let me explain to you about something new and exciting in physics." Then he told me about the splitting of the uranium nucleus and how naturally this fit in with the idea of the liquid drop. I was quite enchanted. Here was the great man himself, impressive in his bulk, sharing his excitement with me as if it were of the utmost importance for me to know what he had to say. Suddenly everything I had done

"Bohr came right over and grabbed me by the shoulder."

in the last five years began to make sense. Neutrons brought about the fission of uranium, and neutrons had become my field.

As a graduate student, I already had some exposure to neutron physics working with Dana Mitchell and with John Dunning, who had made Columbia a center of neutron research. I had learned a good deal about what the others who worked with neutrons did and why. I had come to Columbia to become a radio engineer, but a part time job brought me in contact with Dana Mitchell who persuaded me to switch to physics. My knowledge of radio circuits turned out to be useful in building the cyclotron. At this point I had started my PhD research on the scattering of neutrons and had already completed most of the apparatus. I could understand what Bohr was saying, and what he said had exciting implications. I could sense the importance of the discovery and the new possibilities for experiments that would follow. Suddenly, I had a lot to talk about with Fermi.

Bohr couldn't wait for Fermi as he was on his way to Washington. As soon as he left I rushed off to find Fermi. I found him in his office, but he had anticipated me. He had already heard about the fission of uranium from Willis Lamb, who had just heard Bohr talk about it at Princeton. Before I had a chance to say anything he smiled in a friendly fashion and said, "I think I know what you want to tell me. Let *me* explain to you about fission." Then he went to the blackboard and in his inimitable, graphic way showed how the uranium nucleus would split in two. Then he estimated the amount of energy release from the mass defect. I have to say that Fermi's explanation was even more dramatic than Bohr's. It made the experimental possibilities even more exciting. It became obvious that one could check the energy release by looking for the intense burst of ionization from the splitting parts. We didn't know then that this was also Frisch's plan.

I had just completed an ionization chamber-linear amplifier apparatus that was just what was needed for the job. There was also the cyclotron I helped build which was available as a neutron source. It just seemed natural to offer to work with Fermi using these. After all, he had arrived at Columbia only a few weeks before and had neither equipment nor students to work with.

Working with Fermi

The fact is, I was immensely drawn to Fermi. He had an unusual grasp of physics which he kept at his fingertips always ready for use. When a problem arose, he had the knack to be able to go to the blackboard and simply work it out. The physics just flowed out of the chalk. He would start with the principles he was certain you knew, and he would write these in simple mathematical form, make a few plausible approximations to give equations that were easy to solve, obtain a formula, put in the numbers and calculate the result, usually with a small slide rule. If at any stage you appeared puzzled, he would clarify the argument in terms he knew you would understand. The fact that he could read me, and I him, made it easy and natural for us to work together.

Another quality was the *way* he did physics. Physics was not something you organized and then managed by getting other people to do the work. With Fermi, it was the work that made the physics worthwhile. He wanted to wrestle with nature himself, with his own hands. He liked to have someone to work with. He liked the companionship; the work went faster that way. He liked to talk about what he was thinking and to show what he was doing. So, as he put it, we made a deal. I would teach him Americana, and he would teach me physics. That's how things worked out. On that day we began a collaboration that continued happily, almost without interruption, until his death some 15 years later.

In any case, we didn't lose any time. We wanted to find out whether the heavy burst of ionization from fission could be seen. The linear amplifer-ionization chamber combination was just the thing for this. All we had to do was prepare a layer of uranium on one electrode and insert it in the chamber. That same afternoon we set up everything at the cyclotron. But the cyclotron was not working very well that day. Then I remembered those radon-beryllium sources on the thirteenth floor of Pupin Laboratory.

By that time Fermi had already left for a conference in Washington, the same meeting to which Bohr had gone. John Dunning, under whom I had been working as a graduate student, came back to the lab that night. The record in my notebook is dated January 25, 1939. With the neutron source near the ionization chamber and some paraffin to slow down the neutrons, we were able to see, from time to time, on the screen of our cathode ray oscilloscope, the huge ionization pulses that we expected from fission. It was the first time in America.

It was a very propitious moment. Dunning, recognizing the importance of what we had done, telegraphed Fermi in Washington about our observation. When the meeting opened the next day both Bohr and Fermi talked about the fission problem. Fermi was able to speak with the conviction of personal experience. Then he mentioned the possibility that neutrons might be emitted during the splitting. After all, the fission products would have a large neutron excess. Although this was only a guess, its implication for a chain reaction produced a great deal of excitement. Physicists at the meeting rushed to call their laboratories, and soon there were confirmations from a number of places throughout the country.

Things were happening fast. Fermi's arrival in the United States on January 9, 1939 was followed by Bohr's just one week later. The meeting in Washington occurred on January 26. Our observation of the energy release in fission had been made the previous day. Later we found that Frisch had already completed his experiment in Copenhagen on January 15, ten days before. On January 30 there was a news release by Watson Davis of Science Service that talked about a world standing on the brink of atomic power. This was also four months after the capitulation at Munich. Hitler was on the march; he was preparing his takeover of Czechoslovakia.

Neutrons from Uranium

Fermi didn't wait until the end of the meeting in Washington to return. He rushed back to Columbia and straightaway called me to his office. My notebook lists the experiments he felt we should do right away. The date was January 29, 1939.

Fermi saw from the first that if there was to be a chain reaction, if useful amounts of nuclear power were to be made, there would have to be a way to regenerate the fission reaction to involve large numbers of nuclei. This could come about if in the course of splitting, some neutrons (more than one on the average) were emitted. Then, with each fission more neutrons would be available for further fissions; and if not too many were lost, their number could continue to multiply until the reaction became self-sustaining.

In carrying out further experiments that could be done with the ionization chamber-linear amplifier combination all the members of Dunning's group working with the cyclotron participated. For several years, Eugene Booth and Norris Glasoe had devoted themselves to constructing the cyclotron. Francis Slack had only recently arrived to spend a sabbatical year at Columbia. The subsequent developments caused him to extend his stay many years beyond his original intention. Fermi's insistence that quantitative measurements be carried out prevailed, and the first paper, written only two weeks after our initial observation, reported the value of the fission cross-section for slow as well as for fast neutrons. The measurements were rather crude, but a rough number is better than none at all. In attempting to determine whether a chain reaction was possible, numerical values were crucial. The fission cross-section was of central importance. If this had turned out to be too small the chain reaction would have been impossible.

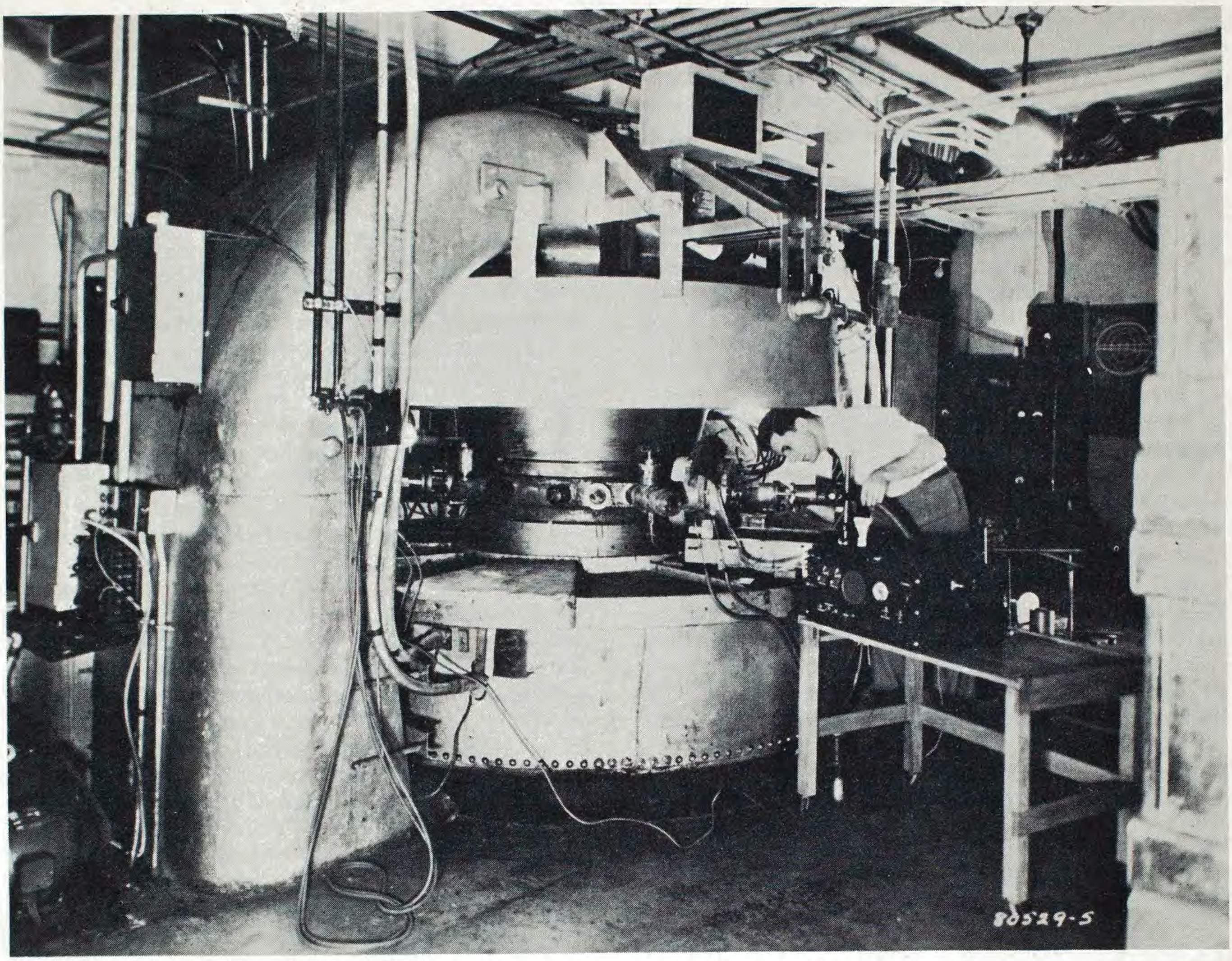
Ironically, more use was made of the radon-beryllium sources than the cyclotron in these experiments. What these natural sources lacked in intensity they made up in reliability and steadiness of output. In some experiments their small size was a distinct advantage. In those days the cyclotron was frequently down for repairs. And although the situation improved with time, Fermi, for the most part, preferred the radon-beryllium sources, and Dunning generously placed these facilities at his disposal. I had to learn how to prepare these sources and came to make a good many of them.

The first experiment was published in the March 1, 1939, issue of the *Physical Review* under the title "The Fission of Uranium."[5] It was a second paper, however, completed only two months after we had heard the news from Bohr, that gave the evidence for fast neutrons emitted by uranium. This paper, "Products of Neutrons in Uranium Bombarded by Neutrons," was published in the April 15, 1939 issue of the *Physical Review;* H. B. Hanstein, another graduate student, joined us for this work.[6]

Following the publication of the first paper on the fission of uranium, Dunning and his group began to follow a different line of research. They decided to test Bohr's idea that the rarer isotope of uranium with mass 235 was the one responsible for the fission we observed. Since this isotope makes up less than one percent of natural uranium, it was clear that, if this idea were correct, the cross-section for the fission in the pure uranium-235 isotope would be more than one hundred times larger than that ascribed to the natural mixture. By separating the 235 isotope, it would be much easier to obtain the chain reaction. More than this, with the separated isotope the prospect for a bomb with unprecedented explosive power would be very great.

Dunning knew he could obtain small quantities of the 235 isotope, quite pure, from Al Nier of the University of Minnesota. Nier was an expert with the mass spectrograph. His machine could be used to obtain small quantities of separated isotopes. He agreed to a joint project, and in a few months Nier had produced enough to carry out a definitive test.[7] The result was positive and, thereafter, Dunning turned his energies to the problem of separating the isotopes. The story of his success in this is told elsewhere.[8]

The way of isotope separation did not appeal to Fermi. He was not discouraged by the small cross-section for fission in the natural isotope. "Stay with me," he advised, "we'll work with natural uranium. You'll see. We'll be the first to make the chain reaction." I stuck with Fermi. As an added inducement he promised, in a half-serious tone, "Uranium is



Herbert Anderson at the Columbia cyclotron in 1939.

going to be a very important material, and some day you may be president of the Uranium Company of America."

We were not the only ones to realize that neutron emission was the key to the problem of the chain reaction. Nothing known then guaranteed the emission of neutrons. Neutron emission had to be observed experimentally and measured quantitatively. In France, with ominous war clouds threatening, Hans von Halben, Lew Kowarski, and Frederic Joliot-Curie, it turned out, had done essentially the same experiment as ours. Their paper in *Nature*[9] appeared a week or two earlier than ours in the *Physical Review.* However rapidly our work went in those first few months, the French group managed to publish something along the same lines a week or so ahead of us.

Szilard at Columbia

Besides the French group, another group was working on the neutron question. Leo Szilard and Walter Zinn had joined forces to demonstrate neutron emission with an experiment quite different from ours. They were also working in the Pupin Laboratories at Columbia, but on another floor. Szilard was not a member of the faculty, but he had arranged through Dean George B. Pegram for a guest appointment. Szilard had already established himself as a nuclear physicist of some standing. While in England he had gained fame for inventing an ingenious way to concentrate radioactive isotopes known as the Szilard-Chalmers process, and he showed how to produce neutrons from x-rays.

The work of Fermi and his group with neutrons and the radioactivity they could induce was particularly intriguing to Szilard. When he came to Columbia he persuaded Walter Zinn to work with him on the neutron emission problem. Zinn was already doing experiments on the scattering of fast neutrons in light elements. Since he had both the source of neutrons and the necessary detecting apparatus, namely, ionization chambers and a linear amplifier, he had also verified the emission of heavy fragments in fission. He had begun an attempt to find the fission with this equipment when Szilard entered the picture with the suggestion that the experiment would be much improved if the photoneutron source were used instead of the d-d neutrons. Zinn agreed,

Szilard convinced himself that with graphite a chain reaction using natural uranium would work. Hard evidence was lacking but he simply argued that it would be too risky to make any other assumption.

but pointed out that a photoneutron source was not available. Szilard quickly corrected this deficiency. He sent to Oxford for a hollow cylinder of beryllium he had used in experiments there. He also rented a gram of radium with some money he managed to borrow from a friend. At the first try with this new set up, the fast neutrons from fission were immediately evident.[10]

It was not Szilard's idea to compete with Fermi; he really wanted to work with him. He managed to interject himself into our experiment in an interesting way. He had a criticism of our neutron emission experiment. He went to Fermi and said: "In your experiment, Enrico, you used a radon-beryllium source. That source, as you know, has rather energetic neutrons. How do you know that some of the neutrons are coming not from fission but from a direct (n, 2n) reaction?" When Fermi conceded this point, Szilard was ready: "It just happens that I have a radium-beryllium photoneutron source that produces neutrons of much lower energy. With it, you won't have the problem of the (n, 2n) reaction." Fermi didn't think this was a serious question, but he did admit that the results would be less open to question if the photoneutron source were used. So we repeated the experiments with Szilard's source. We still found that uranium emitted more neutrons than it absorbed. In our paper we acknowledged a curious organization called The Association for Scientific Collaboration, a Szilardian creation.

It then appeared that none of the neutron emission experiments were really conclusive. Szilard urged a larger scale experiment, and the three of us joined forces to carry one out. A great deal of work was involved. There were long tin cans which we had to pack with uranium oxide powder and seal; we had to mix a huge solution of manganese after each irradiation; and we had to follow the radioactivity induced in it throughout the night.

Fermi's idea of doing an experiment was that everyone worked. He liked to work harder than anyone else, but everyone worked very hard. However Szilard thought he ought to spend his time thinking. He didn't want to stuff uranium in cans and he didn't want to stay up half the night measuring the manganese activity. For these duties, he announced, he had hired a young man by the name of S. E. Krewer, who would do these things better than he could. With this arrangement everything went very smoothly. Krewer was very competent and did all he had to very well.

That experiment was important in a number of ways, but it was the first and also the last experiment in which Fermi and Szilard collaborated. After that a mutually satisfactory arrangement developed in which Fermi and his associates did the experiments while Szilard worked hard behind the scenes to make them possible.

Szilard liked to say, with a twinkle in his eye, that Fermi's idea of being conservative was to play down the possibility that the chain reaction would work; but that his idea of being conservative was to assume it would work and then take all the necessary precautions.

The experiment was important for emphasizing the role of the resonance absorption in the chain reaction. Neutrons were lost in this process that were needed to make the chain reaction go. It was in reviewing what was happening in this experiment that we were led to realize that by lumping the uranium the effect could be reduced.

I remember how it was when the three of us—Fermi, Szilard and I—met together to look at the results. At a certain point it became clear that unless the resonance absorption effect were taken into account in some manner the result would be inconclusive. Our discussion about this proceeded aimlessly for awhile. Then Fermi asked to be excused for about 20 minutes. When he returned he announced that he had made a rough estimate which we then duly recorded in the paper.[11] The episode left us dissatisfied with our knowledge of the resonance absorption effect and I decided then and there to do a study of the resonance absorption for my PhD thesis.

The experiment had another important result. It made it clear that the absorption in hydrogen would be too large to make the chain reaction work with natural uranium.

Considering the possible alternative both Fermi and Szilard began to think that graphite had attractive possibilities. This idea emerged in an exchange of letters that took place just after Fermi went off to spend the summer at the University of Michigan. On July 8, 1939, less than a week after our paper had been sent to the *Physical Review*, Szilard wrote that he had come to the conclusion that a large scale experiment using carbon for slowing down the neutrons ought to be started without delay. It was a gamble, but the other possibilities looked less practical at the time. The problem was where to get money for the graphite.

Szilardian Approach

This was the kind of problem Szilard liked. Fermi, on the other hand, was not very good at the kind of promotion this required. As a matter of fact, Fermi had gone to Washington in March in an attempt to alert the government to the implications of atomic energy. Introduced by a letter from Pegram, Fermi

The famous letter, signed by Einstein but written by Szilard, was delivered personally to President Roosevelt in October 1939.

had talked to Admiral S. C. Hooper and a group of Navy men. However, no action followed, very likely because of his and Pegram's cautious language. As a summer respite, Fermi turned his attention to an interesting problem in cosmic rays, to calculate how the absorption of mesotrons depended on the state of condensation of the matter they traversed.

Szilard's approach was more elaborate and more successful. He convinced himself that with graphite a chain reaction using natural uranium would work. Hard evidence was lacking but he simply argued that it would be too risky to make any other assumption. He estimated he would need about $10,000 for the graphite. Since this was much more than he could hope to get from any university, he thought of going to the government for support, especially in view of the military implications.

Thinking that the best approach was to go directly to the top, Szilard acted on a suggestion from Gustav Stolper, a Viennese economist and friend of long standing. He went to see Alexander Sachs, a Lehman Corporation economist, reputed to have ready access to the White House. Sachs asked for a letter from Einstein to present to the President. The famous letter, signed by Einstein but written by Szilard, was delivered personally to President Roosevelt in October 1939. In the letter the possibility of a chain reaction was taken to be "almost certain." The possibility of extremely powerful bombs was presented as being "less certain" but highly dangerous if developed first for Nazi use. As the interview drew to a close Roosevelt showed he had understood what was wanted. "Alex," he said, "what you are after is to see that the Nazis don't blow us up." The letter was marked for action.

Nothing happened at first, then after a few weeks Sachs called. A meeting had been arranged with Lyman J. Briggs, the director of the National Bureau of Standards and two ordnance specialists, Colonel Keith R. Adamson of the Army and Commander Gilbert C. Hoover of the Navy. The officials met with the three Hungarian scientists—Eugene Wigner, Edward Teller, and Leo Szilard. After the case was presented, the question of money arose. Szilard thought $6,000 would suffice for the test of graphite he had in mind. There followed a long declamation from the Army representative about the nature of war. In the end he argued it wasn't weapons that won wars, but the morale of the troops. Then Wigner, in his very polite manner, interrupted him. He said, in his high pitched voice that it was very interesting to hear this. He had always thought that weapons were very important and that this is what cost money, that this is why the Army needed such a large appropriation. But he was interested to hear that he was wrong: it's not weapons but morale which wins the wars. If this was correct, perhaps one should take a second look at the budget of the Army, maybe the budget could be cut. Colonel Adamson wheeled around to look at Wigner and said, "Well, as far as those $6,000 are concerned, you can have it."[12]

That money was a foot in the door. It was an official recognition that the support of science was not only a federal responsibility but a necessity as well. The government could not overlook a development that threatened its survival. World War II had broken out in Europe. The atmosphere was full of worry and danger. It was not a time to procrastinate. As a result of Szilard's action we would get our graphite, and the federal coffers would be pried open, wider and wider, as the needs of "big" science came to be recognized.

While Szilard was busy bringing about miracles in Washington, I was occupied at the cyclotron, measuring the resonance absorption of uranium. Neutrons absorbed by uranium in this way were lost to the fission chain, and the measurements would make it possible to estimate what proportions of uranium and graphite would minimize this loss. It was also recognized that while neutrons captured in this way did not produce fission, they did produce plutonium-239, a new and relative stable isotope. According to Bohr and John Wheeler's theory, this isotope being of odd atomic weight would behave like the isotope uranium-235 with respect to fission. Thus plutonium-239 was a likely candidate for slow neutron fission.[13] As a nuclear explosive, its behavior could be expected to be quite similar to uranium-235. If a chain reaction were made to work with natural uranium, a highly fissionable material could be made without isotope separation. Thus, the chain reaction was a way to the bomb. This became the decisive factor in the support of its development. Moreover, for energy production, the production of plutonium was a way of converting the abundant isotope uranium-238 into a useful nuclear fuel. A careful balance was required however. Too much resonance absorption and the chain reaction wouldn't work; too little, and the production of an important product would be cut. Of course, in my thesis I emphasized only the fundamental physics involved.

Later, when it became important to have more detailed information about the resonance absorption we went to Princeton. There a group under H. D. Smyth, principally John Wheeler and Eugene Wigner, had been doing theoretical studies of the chain reaction. Princeton had a cyclotron and two young physicists, Robert R. Wilson and Edward C. Crentz, who seemed anxious to work with Fermi and me on this problem. The occasion brought us in close contact with Eugene Wigner who gave our results his sharpest scrutiny.[14]

(To be concluded next month. Notes for both this month's and next month's sections will appear at the end of the latter.)