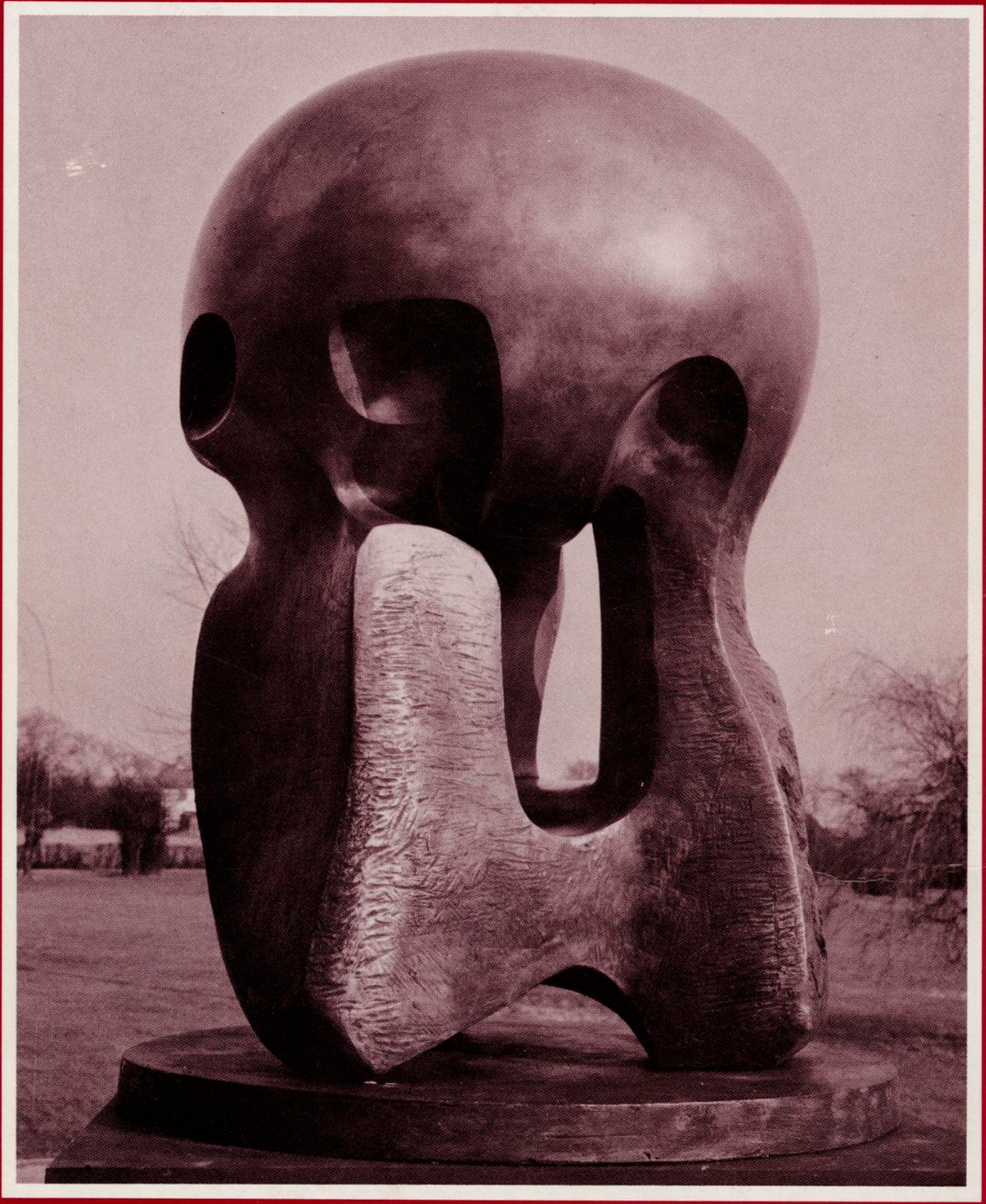
The Nuclear Chain Reaction—Forty Years Later

Proceedings of a University of Chicago commemorative symposium



Edited by Robert G. Sachs

The Nuclear Chain Reaction–Forty Years Later

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Proceedings of a University of Chicago symposium commemorating the fortieth anniversary of the first controlled, self-sustaining nuclear chain reaction

Edited by Robert G. Sachs

The University of Chicago

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PREFACE

President Hanna Gray's charge to the members of the 40th Anniversary Committee was simply "to decide on a format for the anniversary, give an intellectual structure and substance to the events and sponsor them." We were thereby permitted a rather broad scope in our considerations.

No member of the committee had been a direct participant in the "Fermi experiment," nevertheless we recognized that the nostalgic element for those who had participated must be a singular consideration. At the same time we also recognized that the public interest and controversy over the ultimate consequences of this experiment was more representative of the passage of four decades. Therefore we decided that an appropriate way to commemorate the event would be to hold a public symposium led by some of those who had been involved in the nuclear enterprise and in the debates concerning its consequences. The audience would consist of invitees who had a direct or indirect association with the project as well as interested members of the University community and the public. The subject matter would include, especially for the benefit of the public, the story of the scientific developments leading to the event and of the subsequent political activities of the Chicago group of scientists. It would also include discussion of the consequences of the controlled nuclear chain reaction and the controversy surrounding them.

To this end we arranged a symposium in four sessions, a session covering the aforementioned story, one on the peaceful technical consequences other than nuclear power, another on nuclear power, and a culminating session on control of nuclear armaments. Because of the time limitations set by our one and one-half day schedule, the first two sessions were squeezed into a half day, allowing essentially no time for discussion. We felt that it was more important to allow time for audience participation in the controversial sessions on nuclear power and nuclear arms, and a half day was allowed for each of them.

The first session included two formal talks and one reminiscence. The opening talk was devoted to a brief survey of scientific discovery leading to the experiment in Stagg Field. Although the story of the actual experiment on December 2, 1942 was not included in this session, reminiscences were provided in informal after-dinner speeches by two people who were there at the time, Crawford Greenewalt and Albert Wattenberg. Their remarks are incorporated at the end of these proceedings. For completeness, a more detailed description of this event, written by Wattenberg and published in a Bulletin of Atomic Scientists anniversary edition, is repeated here in Appendix B to the opening talk, with the permission of the Editor of the Bulletin. We are also pleased to include here as Appendix A to the opening talk an unpublished reminiscence about events leading up to and including the chain reaction by Herbert L. Anderson, who was a key participant in the experiment but was unable to attend the symposium.

In view of existing public questions concerning the sense of social responsibility of the scientific community, the Committee felt that it was important to recall the crusade for civilian control and international control of nuclear energy led by "atomic" scientists. Therefore the second talk of Session 1 was a historical essay by Alice Smith on the political activities of the scientific community, especially the Chicago scientists, during and immediately after World War II. This was followed by Edward Levi's reminiscences about the role of the Chicago group in the campaign for civilian rather than military control of nuclear energy.

Public discussions of the applications of nuclear science tend to focus on the controversial issues associated with possible nuclear power plant accidents or with weapons. Little attention is usually given to the many areas of scientific advance and health-related applications descending directly from the first chain reaction. It is safe to say that millions of lives have been saved or extended by many years by these means. It is also clear that the associated advances in physical and biological science have been of enormous importance to the technological revolution (even exclusive of nuclear power) of the last four decades.

The second session on the first day dealt with these issues, although very briefly, considering their scope. Two formal talks were presented, one on the application of neutrons to physical sciences and technology, the other primarily on nuclear medicine. The talks provided a wealth of newly organized material covering a great range of technical subjects. They comprise a rich source of information that can be used to increase public awareness of these important facets of the technology.

The most direct peacetime consequence of the discovery that the nuclear chain reaction could be controlled was the nuclear power program. Because of its economic and social importance and because of the controversy surrounding it, a half-day session was devoted to the subject. Our aim was to offer a reasonably balanced view of the history of nuclear power and the problems associated with it. The session consisted of three talks and a discussion, one talk was on the history and prospects and another on the problems of governance of nuclear power. The third talk concerned the special role of Argonne National Laboratory in the development of nuclear power. Argonne was the successor organization to the University of Chicago's Metallurgical Laboratory, the institution within which the Stagg Field experiment was performed. The discussion was led by a panel consisting of two invited discussants and the speakers.

The ceremonial session was marked at the site of the first chain reaction (the Henry Moore sculpture) by a talk given by John Simpson entitled "There Still is Time." This served as an excellent introduction to the culminating half-day session on nuclear armaments. In this session there were two formal talks, one on the need, problems and prospects for arms control and the other on the need for nuclear armaments and acceptable conditions for an arms control agreement. A panel consisting of two invited discussants and the speakers then led a comprehensive discussion. In reading this discussion it should be kept in mind that changes have taken place on the arms control scene since the time of the symposium.

As speakers, discussants and session chairmen in this program we chose some who have been associated with and concerned with the nuclear program since its beginning and others whose active interest originates at a later date. The result was that the symposium provided a balance between the perspectives of those who have lived with the issues from the beginning and those with a more recent perspective. It therefore offered a valuable picture of the way in which thought on this subject has developed during the past forty years.

Of course this picture reveals the enormous differences of opinion that have arisen among the scientists, just as they have among members of the larger community. Nevertheless, the divergence of opinion of this selected group will certainly have historical value—although it is not yet history. Their perspectives will also be of interest to many who were not able to be present at the symposium. Therefore they deserve to be rendered in a form more permanent than the tapes on which the proceedings were recorded. We have attempted here in the Proceedings to provide such a permanent record. Our objective in preparing the Proceedings has been to retain as far as is possible the informal style and flavor of the actual symposium. To that end, the authors have been asked to do as little editing of the transcripts of their talks (and the discussion) as is consistent with clarity and reasonable grammatical structure. However, some valuable supplementary material, omitted from the symposium because of time limitations, has been added.

Since there was audience participation in the discussions, it was not always possible for us to identify the questioner. We apologize herewith to anyone of those anonymous questioners whose voice the Editor should have been able to identify.

The members of the Committee wish to express their appreciation to President Hanna Gray for her strong support of the symposium and of the effort to prepare these proceedings. We also greatly appreciate the efforts of her staff on behalf of the symposium, in particular those of Jonathan Kleinbard, Duel Richardson, and Dolores Ford who performed the difficult task of transcribing the tapes. The Editor is particularly indebted to his secretary, Kathy Visak, for her help in putting together the pieces and in implementing the editorial requirements.

Finally the Editor wishes to acknowledge the research support over a period of many years from the Division of High Energy Physics of the Department of Energy and from its predecessor organizations of the Atomic Energy Commission, support that has made it possible for him to keep in touch with the scientific developments flowing from this remarkable experiment and with the people who are responsible for those developments.

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Chairman, 40th Anniversary Committee

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SESSION I

History of the Chain Reaction

Eugene P. Wigner, Chairman

HISTORICAL BACKGROUND OF THE CP-1 EXPERIMENT

Robert G. Sachs*

The production of the first artificial self-sustaining nuclear chain reaction using the first Chicago pile (CP-1) under the west stands of Stagg Field at The University of Chicago was an unusual achievement in many ways, but because the military, technological, social and political implications of the event were so significant, the way in which the achievement exemplifies the process of science is often, understandably, overlooked or ignored.

Especially in a community of scholars and scientists gathered to consider the history and consequences of this event, it is important to recall the way in which that aspect entered into its history. Although contributions from some of the great minds of all time are an integral part of the story, it is not to seek glory in this past scientific experience that we must recall it, but rather to remind the world and ourselves of the best of our scientific and scholarly traditions, that is, of the real meaning of our work. Our commitment is to the science of the present and of the future but in order to know where we are going it is important for us to know whence we come. And when so significant a piece of the past is so close at hand, the lessons of its triumphs and mistakes take on a more vivid, and possibly unforgettable, quality.

This fortieth anniversary is a peculiarly appropriate time to talk about the history of nuclear science because the event we are commemorating is poised in time at the midpoint of that history. In 1902, forty years before CP-1, a discipline of nuclear science did not exist as such but it was in that year, six years after the discovery of natural radioactivity that Rutherford concluded that the energies of the radioactive emanations are larger by a factor of a million than energies released in ordinary chemical reactions among atoms and molecules. He pointed out that this energy must be stored in the atoms that are the source of the radioactivity and that the scale of energy was large enough to resolve one of the great mysteries of that time, the source of the enormous energy emitted from the sun and stars. Until that time there had been no known process capable of producing such a large amount of energy from the available amount of matter.

Rutherford arrived at his conclusion on the basis of the results of experiments making use of what we now consider to be primitive instruments to detect, identify and

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*Philip Morrison was originally scheduled to be the speaker on historical background but, as the result of an accidental injury that occurred about a week before the symposium, he was unable to serve. Because of the short time available, it was not possible to find an equally expert speaker. The 40th Anniversary Committee therefore assigned its chairman the task of preparing and delivering the talk.

I am not an expert on this subject and did not have the time to develop it from primary sources. With apologies, I therefore admit that this material is based on secondary sources and anecdotes collected over my years as a physicist from many of those who were participants in this history. My most useful secondary source was Radioactivity and Nuclear Physics by James M. Cork (D. Van Nostrand Company, Inc., New York, 1947). In addition I made heavy use of the Reviews of Modern Physics article by Louis A. Turner, which is mentioned in the text.

measure the energies of the emanations. Nevertheless these were the first instruments of nuclear physics and they provided the starting point for the development of the increasingly sophisticated instrumentation characterizing the field and leading to its rapid development in later years.

However, they were basically passive experiments in the sense that the experimenter simply took whatever the radioactive material had to offer and tried to understand it. In this respect nuclear physics at that time was an observational science, much like astronomy. It should be remarked, though, that the chemistry of radioactive substances was an active science because it was by chemical methods that the source of radioactivity could be identified, separated and concentrated, steps that were required as a precursor to the observations made by the physicist. I emphasize the complementary relationship between the chemist and physicist because it plays an essential role in the history of nuclear science, especially in the discoveries relating to nuclear fission. We shall return to that later.

It was in the following year,1903, that an active rather than passive experiment of a type to become characteristic of nuclear physics was first carried out. At that time, Lenard attempted to study the structure of atoms by measuring the way in which a beam of cathode rays (electrons) was deflected in passing through matter. He did not obtain the quantitative information required to establish the existence of a nuclear atom but, from the fact that most of the electrons were able to pass through matter he was able to conclude that atoms were almost transparent to fast electrons. This qualitative result led him to suggest that the atom consists of a small impenetrable center surrounded by an electron cloud.

Eight years later, in 1911, Rutherford reported the first of his famous experiments on the scattering of alpha rays by atoms. He was able to carry out a detailed quantitative analysis of the scattering measurements to show that the observed deflections of the alpha particles were the result of the electrical repulsion of the positively charged alpha particle by a positively charged object, the "nucleus", of radius much smaller than that of an atom and having a mass of the same magnitude as the total mass of the atom. Not only did he thereby confirm the qualitative picture of Lenard, but later he was able to show, as suggested by Van Den Brock in 1913, that the nucleus carried a positive electric charge equal in magnitude to Z times the electron charge, where Z is the sequential "atomic number" assigned to the position of the atom in the periodic table of the (chemical) elements. Thus was born the quantitative model of the nuclear atom with its positively charged massive nucleus having dimensions of one-trillionth of a centimeter surrounded by a compensating cloud of negatively charged electrons carrying only a tiny fraction of the atomic mass.

This discovery, which was a triumph of a very carefully thought out experiment followed by equally thoughtful theoretical analysis may be described as the birth of the field of nuclear physics after a period of gestation of some 15 years, beginning with the discovery of radioactivity.

The discovery had a tremendous impact on all of atomic and molecular physics and on the foundations of chemistry. The advent of the nuclear atom made possible Bohr's quantized model of the hydrogen atom which was a touchstone of the magnificent intellectual revolution of our understanding of nature, the quantum mechanical theory of matter and fields. Although the conceptual revolution associated with the development of quantum mechanics is not our immediate subject, it represents the fabric of the development of theoretical physics during the period with which we are concerned, and the nuclear physics, although much more experimental than theoretical in character during the ensuing years, was woven into the fabric of the theory, becoming an essential part of the intellectual ferment and excitement of those times.

The next major step in nuclear science was again due to Rutherford who, in 1919, showed that it was possible to transform one element into another, to "transmute" some light elements (i.e., elements of small atomic weight) into other elements by bombarding them with alpha rays. An alpha particle, the basic unit making up the rays, can enter into the nucleus of a target atom releasing a proton (nucleus of the hydrogen atom) in the process.

Thus Rutherford reversed the process of alpha radioactivity, which is due to the emission by a heavy nucleus of an alpha particle and the resulting transmutation of the original nucleus to that of another (lighter) element. The particle is emitted with quite high energy because, as soon as it emerges from the (nuclear) force field holding it into the nucleus it is subject to the strong repulsive force between its positive electrical charge and the large positive charge of the residual nucleus. Therefore it is accelerated away from the nucleus until, when it is free of the atom, it has a kinetic energy millions of times greater than the typical energies of the atomic electrons.

The availability of such high energies was essential for Rutherford to be able to probe the structure of atoms with alpha particles because this electrical repulsion between the alpha particle, which is the nucleus of the helium atom, and any other atomic nucleus places a limit on the depth of penetration into the atom of an alpha particle of given energy. Until the year 1932, natural radioactivity was the only available source of nuclear particles sufficiently energetic for carrying out such experiments. But, then, Cockcroft and Walton succeeded in building a transformer capable of producing electrical potentials of about 300,000 volts and they were able to accelerate protons, the nuclei of hydrogen, the lightest of atoms, to sufficient energy to demonstrate another type of nuclear reaction. They found that when a nucleus of lithium is bombarded by protons, it may absorb a proton and disintegrate into two alpha particles. Thus one atom of lithium is converted into two atoms of helium. By comparing the measured masses of the proton, lithium nucleus and alpha particle with the net energy balance in the nuclear reaction it was possible for the first time to confirm the famous Einstein relation of the special theory of relativity, $E = mc^2$

This success of Cockcroft and Walton in providing a source of artificially accelerated nuclear particles opened the way to overcoming the constraints imposed by having to depend on radioactive sources of fast nuclear particles for experiments in nuclear physics and chemistry. Although the old sources continued to be used, for example, for the discovery of artificial radioactivity by Curie and Joliot in 1934, the invention of the Cockcroft-Walton machine opened an entirely new branch of science, accelerator physics, with its later creation of the cyclotron, the electrostatic generator, and then the great particle accelerators of modern times.

The cyclotron and electrostatic accelerator became the essential tools of experimental nuclear physics but, even though these machines contributed in an essential way to the history of CP-1, the creation of this branch of physics in 1932 is not by any means the only dramatic event that took place in physics that year,* a year that was cornucopia for the physicist. It was the year in which the heavy isotope of hydrogen, "deuterium", was discovered and it was the year in which the first form of antimatter, the positron, or positively charged electron was discovered. But most important af all for our story, it was the year in which Chadwick discovered the neutron.

*For further details see Bromley's contribution.

The neutron is an electrically neutral nuclear particle having the same mass as the proton. In fact, it has much the same nuclear properties as the proton but it is not the nucleus of an atom like the proton (which is the nucleus of hydrogen) because, in the absence of an electrical charge, it does not attract electrons. But for the same reason, it is not repelled by the positive electric charge of an atomic nucleus. Therefore, even the slowest moving neutron can penetrate into the depths of an atom and enter its nucleus.

The scientific significance of this possibility was immediately recognized; neutrons of any energy could penetrate into the nucleus of any atom and be used to study properties of that nucleus. One could study the way in which neutrons are scattered from nuclei or the way in which they are absorbed into nuclei, thereby transmuting them from one species to another. This latter process has become a primary source of artificial radioactivity.

A select few realized the possible technological significance of the existence of the neutron. Already in 1929, before the discovery of the neutron, Corbino, who was Fermi's teacher, had recognized that exothermic reactions between nuclei, that is reactions producing more energy than required to initiate them, might be a source of macroscopic energy, just as exothermic chemical reactions among atoms to form molecules are our principal artificial source of heat and energy. But until the neutron was discovered, the only known nuclear reactions required enormous energy or high temperatures like those in the sun and other stars to initiate them because of the electrical repulsion between nuclei. The neutron made it possible to release the energy stored in nuclei but, to obtain macroscopic amounts of energy, it was necessary to have a "bucket" of neutrons rather than be limited to working with one neutron at a time. Imagine having to depend for heat on the burning of coal by bombarding it with one oxygen atom at a time!

Leo Szilard, one of the most imaginative physicists of this century, conceived of the possibility of releasing energy by making use of what the physicists call (n, 2n) reactions, those in which one neutron enters a nucleus and two neutrons emerge. If such a reaction satisfied the right conditions, he reasoned, then it would be possible to start from a few neutrons surrounded by the reactive material so that after the newly produced neutrons were absorbed they would produce twice their number, each of them then repeat the process, redoubling the number of neutrons. Since the time between generations would be very short, this doubling and redoubling process, or chain reaction, would quickly increase the number of neutrons to macroscopic levels, producing macroscopic amounts of heat from the energy released by the reaction. In 1934 Szilard patented such a process on the assumption that beryllium could be used as the active material that would undergo the (n, 2n) reaction. Although beryllium does not work and, in fact, no ordinary (n, 2n) reaction having adequate efficiency has been found for the purpose, Szilard had the opportunity to apply the notion of the chain reaction later.

In the meantime the scientific possibilities of the neutron were exploited immediately, especially by Fermi who developed the techniques for working with slow neutrons to a fine art.

In 1934 Fermi realized that it might be possible to extend the periodic table beyond the element of highest atomic number, Z = 92, that is, beyond uranium, by adding a neutron to the uranium nucleus, thereby producing a slightly unstable nucleus which would therefore emit an electron (the radioactive process known as beta decay) to become the nucleus of atomic number 93, that is, a transuranium element. He and his collaborators had already shown that many elements could be converted to beta emitters in this way. They found that the products of uranium bombarded by slow neutrons were radioactive as expected, but consisted of four different radioactive species having different lifetimes.

Fermi's group tried to identify the chemical nature of these radioactive products and found that some of the material behaved chemically in a way similar to the chemical behavior expected for element 93. Furthermore they established that the chemistry of this material was not what would be expected of elements near to, but having smaller atomic number than uranium. They concluded that they had, indeed, produced a transuranium element.

This discovery was naturally of great interest to chemists since it extended the periodic table of chemical elements. There was a surge of activity by chemists and physicists who sought to understand the identity and properties of the transuranium elements and other artificially radioactive elements produced when elements of atomic number near 92 absorbed neutrons. Just one of them, I. Noddack, was critical of Fermi's chemistry and suggested that elements of considerably lower atomic number formed by spitting the uranium nucleus would have the observed chemical behavior. Therefore she concluded that much more elaborate chemical tests were required to establish the existence of the transuranium elements. There is no evidence that this argument had any influence on others in the field although, in the long run it turned out to be correct.

An impressive description of the transuranium related activity leading to the discovery of fission, and the status of the world knowledge of fission up to December, 1939 is presented in the January 1, 1940 Reviews of Modern Physics by Louis A. Turner. This article gives a clear picture of the work that immediately followed Fermi's apparent discovery of the transuranium elements.

The period from 1934 to 1939 was marked by the studies of the apparent transuranium elements, but the results seemed to raise more questions than they answered. It was only in 1939, when Hahn and Strassman unambiguously identified one of the radioactive species as an isotope of the relatively light element, barium, that the correct picture emerged: the uranium nucleus was spitting into two fragments each a nucleus having about one-half of the atomic number of uranium.

Announcement of this result led to frenetic world-wide activity to confirm the conclusion, to investigate the nature of the phenomenon, and to explain it. Turner lists 29 papers on the transuranium elements from their "discovery" by Fermi in 1934 through the year 1938. In the year 1939 alone he lists 104 papers relating to the fission process. They include German, French, British, U.S., Italian, Russian and Japanese work, all of the papers dated 1939. Within that year the science of the fission process was established.

Since by that time, direct physical measurements of the atomic masses were available, it was well known that the mass of the uranium nucleus was considerably larger that the sum of the masses of any two stable nuclei having about half the atomic number. Therefore, on the by then well established basis of the Einstein relation $E = mc^2$, it could be concluded that the nuclear fragments produced by fission must have large kinetic energies. Since these fragments occur as fast-moving ions, their detection by direct physical measurements was a straightforward matter. In many physics laboratories there was a rush to confirm the occurrence of fission in that way, the first reported successful demonstrations being those of O. Frisch and F. Joliot. Measurements of the energies of the fragments, the X-rays emitted by them, as well as further accumulated chemical information made it possible to show that these fission products consist of a great variety of elements having atomic numbers in the range from 34 to 57. Of course, for any given fission event, the atomic numbers of the two "instantaneously" produced fragments must add up to 92 since electrical charge is conserved.

I say "instantaneously" (the actual time for fission is less than one trillionth of a second) because the fragments are quite unstable and undergo rapid beta decay into other elements. This is the principal reason for the intense, short lived radioactivity of nuclear waste. This instability was another aspect of fission that was anticipated immediately upon discovery of the phenomenon. Light atomic nuclei are made up of roughly equal numbers of neutrons and protons. But because the electrically charged protons repel one another, it is easier for heavier nuclei to accumulate more neutrons than protons, and this difference increases with increasing atomic number. Thus the fissionable uranium isotope, ^{235}U, contains 143 neutrons compared to its 92 protons. But the stable forms of nuclei having half the atomic number of uranium have a much smaller neutron excess. Therefore the fragments obtained by splitting the uranium nucleus contain too many neutrons to be stable.

Such an unstable nucleus can stabilize itself in two ways: one is by the already mentioned beta decay in which a neutron converts itself to a proton. The other is by shedding neutrons, that is, by neutron emission. That the latter process might occur was also quickly realized and experiments were carried out to determine how many neutrons are released in the fission process. The earliest measurements indicate that the number was probably greater than two.

The importance of this number was quickly recognized; the names Von Halban, Joliot and Kowarski are prominent in Turner's reference list but, of course, the importance of neutron multiplication had already been emphasized by Szilard, before the discovery of fission, so there is no doubt that he and many others had it in mind, but chose not to get into print. I can also remember listening to Fermi on a national radio broadcast explaining the importance of the recent discovery of fission. He indicated the height to which a battleship could be lifted by the energy released in the fission of a kilogram of uranium. I do not recall the number and will not burden you with my own recalculation. It is enough to say that it was impressive. Another public discussion was provided by an article in Collier's magazine which, if I recall correctly, was directly concerned with the possibility of using fission to produce bombs. After that, there was silence on the subject; the scientific community imposed a voluntary censorship on itself because of the fear that the Nazis would exploit the information.

The question of whether or not the fission process could lead to a chain reaction is addressed in the Turner article but, as he reconized, there were many unanswered technical questions to be resolved before a definite answer could be given. However at the time he wrote, in December 1939, it was already clear that an affirmative answer was likely. It was also clear that to actually demonstrate the scientific feasibility of a controlled chain reaction a major scientific and technological effort would be required.

The fact that the fissioning uranium nucleus was the rare isotope ^{235}U, only 0.7 percent abundant in natural uranium, meant that in the bombardment of natural uranium with neutrons many would be absorbed by the abundant isotope ^{238}U and would not contribute to the process. It was necessary to determine these rates of capture for neutrons of different energy and to find ways to bring the neutron energy down (since fission produced fast neutrons) to the energy at which the fission process occurs most efficiently without losing them on the way down. Only then could one hope to produce a controlled chain reaction.

I will not go into the oft-repeated tale of the way in which Szilard and Wigner convinced Einstein to sign a letter to President Roosevelt in order to obtain government support for this effort and the other activities associated with the development of atomic weapons. Enough support was quickly obtained for Fermi to start a series of small experiments at Columbia University using his well developed methods for working with slow neutrons. He introduced the "exponential pile" which was the forerunner of the chain reacting pile, to determine in a very systematic way the neutron multiplication properties associated with various arrangements of the materials. From this information it was possible to deduce the feasibility of a chain reaction and, in particular, how large a pile would be requred to convert it from an exponential to a chain reacting pile, the difference being the number of neutrons lost by escaping through the surface. It quickly became apparent that a rather large scale effort would be needed to obtain the required amounts of materials and to insure that they were pure enough. The impurities absorbed neutrons and even very small amounts of certain impurities were capable of preventing the chain reaction.

Their larger effort was mounted at The University of Chicago beginning in April 1942, at first with larger exponential piles and then, of course with CP-1. A great amount of scientific imagination, care and hard work went into these studies and a large industrial effort was required to produce the required amounts of pure uranium and graphite, the latter being the means for slowing the neutrons. Because of the careful work with exponential piles, Fermi knew what to expect to a very reasonable degree of accuracy when the final pile was being assembled. The experimental procedures carried out to verify these expectations while CP-1 was assembled are described in Al Wattenburg's beautiful article in the commemorative issue of the Bulletin of Atomic Scientists (See Appendix B). It is needless for me to say that the scientific expectations were realized.

This quick and superficial review of the history illustrates clearly the accelerating growth in scientific knowledge, which undergoes a self sustaining chain reaction in its own way. The existence of the atomic nucleus was established only thirty years before the event we are observing took place, in another fifteen years the discovery of quantum mechanics had revolutionized thinking about the physical universe, and it was only ten years before the demonstration of the chain reaction that the existence of the neutron was discovered. After the war, this process of acceleration picked up where it left off, and it continues, but it is hard to imagine a more graphic example than the one we are discussing today.

I would like to conclude with an observation concerning a personal reaction to the experience of reviewing this history. As a physicist who did not participate in any of these events but who is familiar with the science and the scientists of those times, I am impressed by the scientific imperative of the story. With scientists of every nation and every political persuasion involved in trying to unravel the nature of the fission process and realizing its potential implications, there is no way in which the eventual attainment of the controlled chain reaction could have been avoided. The only thing that would have stopped it would have been the discovery that it was forbidden by natural law. And the essential difference that the war made was in timing and in the focus on weapons.

It may be enlightening to speculate about what might have happened had 1939 been a year of real peace and world stability. Although the time scale (and financial scale) would have been different, my guess is that the scientific momentum generated by the discovery of fission would have led to its early exploitation to meet scientific objectives - the development of research reactors, tracers for physical and biological research, and nuclear medicine. That those uses were high on the priority list is manifest from the rapidity with which their development occurred, even during the war. We will be hearing about these developments later this afternoon.

The industrial objectives - the development of nuclear power would probably have come much more slowly. Those of us who were involved in that in the early days know how difficult it was to convince the industrial community that such an esoteric field could have economically practicable consequences - but it would have come somewhere in the world.

Finally, military applications would have taken place in secret, with no public awareness of the reality of the threat, in those nations whose military were capable of recognizing the potential, and acting on it. It is frightening to think about what that would have meant for us. However, that is small comfort when facing the real events of the past although I trust that we can come away from tomorrow's session on arms control with some small hope for the future of the world just as we leave this session with great hope for the future of science, if the world permits its realization.

APPENDIX A

THE FIRST CHAIN REACTION

H. L. Anderson

THE FIRST CHAIN REACTION*

H. L. Anderson

Szilard's Invention

When I think about the chain reaction I can't help but think first of all of Leo Szilard. Leo Szilard, you know, invented the chain reaction. The story he tells[1] is that he was in London about the time of a meeting of the British Association for the Advancement of Science. It was September 1933. He had read in the newspaper a speech by Lord Rutherford, who was quoted as saying that he who talks about the liberation of energy on an industrial scale is talking moonshine.

Well, Szilard is the kind of man who, upon hearing such a sweeping pronouncement from such a great man, can't resist trying to prove him wrong. To quote him[2]:

> This set me to pondering as I was walking the streets of London, and I remember that I stopped for a red light at the intersection of Southampton Row. As the light changed to green and I crossed the street, it suddenly occurred to me that if we could find an element which is split by neutrons and would emit TWO neutrons when it absorbed ONE neutron, such an element, if assembled in sufficiently large mass, could sustain a nuclear chain reaction.

Such a chain reaction would be able to liberate energy on a large scale. His candidate for the proper element was beryllium because there was reason to believe that neutrons would be split off when this element disintegrated. It turned out that beryllium wouldn't work. There was an error in the published value of the mass of helium that made it appear that beryllium was unstable. It turned out that beryllium was, in fact, stable. When Szilard gave up on beryllium he did not give up on the idea of the chain reaction. In the spring of 1934 he applied for a patent in which the principles governing such a chain reaction were set down.

What was it that prepared Szilard for the idea of the chain reaction? There was a book by H. G. Wells. It is called "The World Set Free: A Story of Mankind." It had been written in 1913, mind you, when radioactivity was still in its infancy. It was known that uranium disintegrated by emitting alpha particles. The energy release was a million times greater than the energy release in atomic processes. H. G. Wells recognized that some day this might become a source of energy which could be used with dramatic effect. The difficulty, as he saw it, was that the alpha particles were emitted at too slow a rate. The energy didn't come out fast enough. Well, that's the basis of the story. In the book the scientists figure out a way to make radioactive substances decay faster. It took them 20 years to figure this out. Thus, the way to nuclear power was opened in 1933. Interestingly enough, 1933 was the year artificial radioactivity was discovered. It was a remarkable prediction. In the book, the first use of the large scale release of nuclear power was in a nuclear war. In fact, the book tells about a nuclear war that devastates some of the great cities of Europe. The prophesies seem remarkable in view of what we know now. The ending is, however, a happy one. Nuclear power makes possible a new dimension in living. People come to their senses; they learn how to join together for the common good. A world government is established. Nuclear weaponry is outlawed and the beneficial uses of nuclear power are developed and put into effect. It is no longer necessary for people to crowd into large cities built along the coasts. With abundant energy from nuclear power communities can be established at remote sites anywhere, allowing a variety of living styles in relative abundance and security.

When he decided to patent the idea of the chain reaction, Szilard worried about possible military misuse. He was very much aware of the dangerous developments in Germany. He decided to assign the patent to the British Admiralty[3] because he knew that then it would be kept secret. In fact, it was sealed secret in 1936 and not published until September 28, 1949.

I thought you might like to see what Szilard looked like. Here, in Figure 1, you see him with Einstein. It is part of the story that at a certain point, when money was needed to buy the graphite we wanted, Szilard persuaded Einstein to write his famous letter to President Roosevelt.[4] The photograph was taken at a reenactment of that occasion.

The man I visualize when I think of Szilard is better represented by the photograph reproduced as Figure 2. Here the impish side of his personality peers out at you. Just looking at his face is enough to make you realize that some bright idea is clicking away in his head.

Dunning's Cyclotron

Szilard is the first of the characters I wanted to introduce in this account. Now I introduce myself. I thought you might want to know how I came in. I was there, just by chance really, at the right place at the right time. I was a graduate student at Columbia University. I had gone through the College at Columbia with the idea of becoming an electrical engineer. These were the years of the Great Depression. I was interested in the training that would most likely land me a job. I chose electrical engineering, and even completed the course. But in the process I was discovered by John R. Dunning, Professor of Physics. It was at the time when Professor Dunning wanted, more than anything, to build a cyclotron. I like to think of Dunning as the New York version of Ernest Lawrence. Both came from the wide open spaces in America. Lawrence from South Dakota, Dunning from Nebraska. Dunning was not quite in the same class as Lawrence, but he was pretty good. The list of his accomplishments is quite impressive. He had drive, energy, and lots of enthusiasm. Moreover, he had the personality and the talent to get all kinds of people to suport his enterprises. The cyclotron he wanted to build was modelled after the one Lawrence built with Livingston in Berkeley. When Dunning found that a duplicate of the magnet used by Lawrence and Livingston in their cyclotron was available, he arranged to get it to Columbia. As Figures 3 and 4 make evident, the Columbia magnet is the twin of the one at Berkeley. How Dunning got the money it took to do the rest in those days before government supported research, he never revealed to me. But he was always rushing off. It takes a lot of rushing around to get money.

I had been interested in radio from my high school days. In engineering school I developed a special interest in transmission lines. When I started working on the cyclotron, it occurred to me that the best way to get the highest voltage on the accelerating electrodes (the dees) of the cyclotron was to feed them with a pair of resonant, preferably concentric, lines properly coupled to a high-power oscillator. I wrote up a design study and showed it to Dunning. He liked the idea and the Columbia cyclotron became the first to use concentric line feeding of the dees. The schematic diagram of the system shown in Figure 5 is reproduced from the mimeographed report we wrote. Concentric line feeding became a common feature of cyclotron design. We did not publish the paper but mimeographed copies were circulated to interested parties.

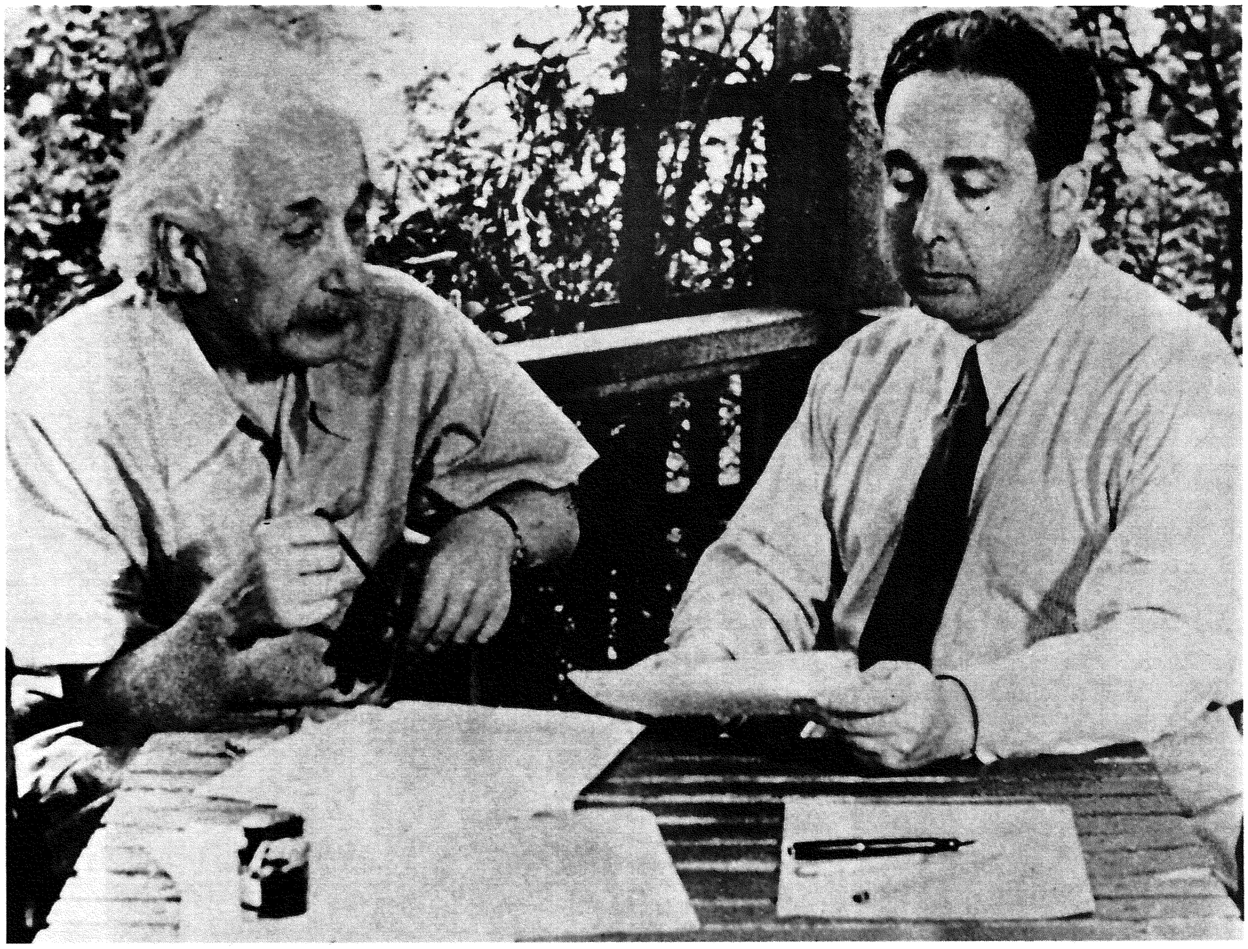


Figure 1

Figure 2

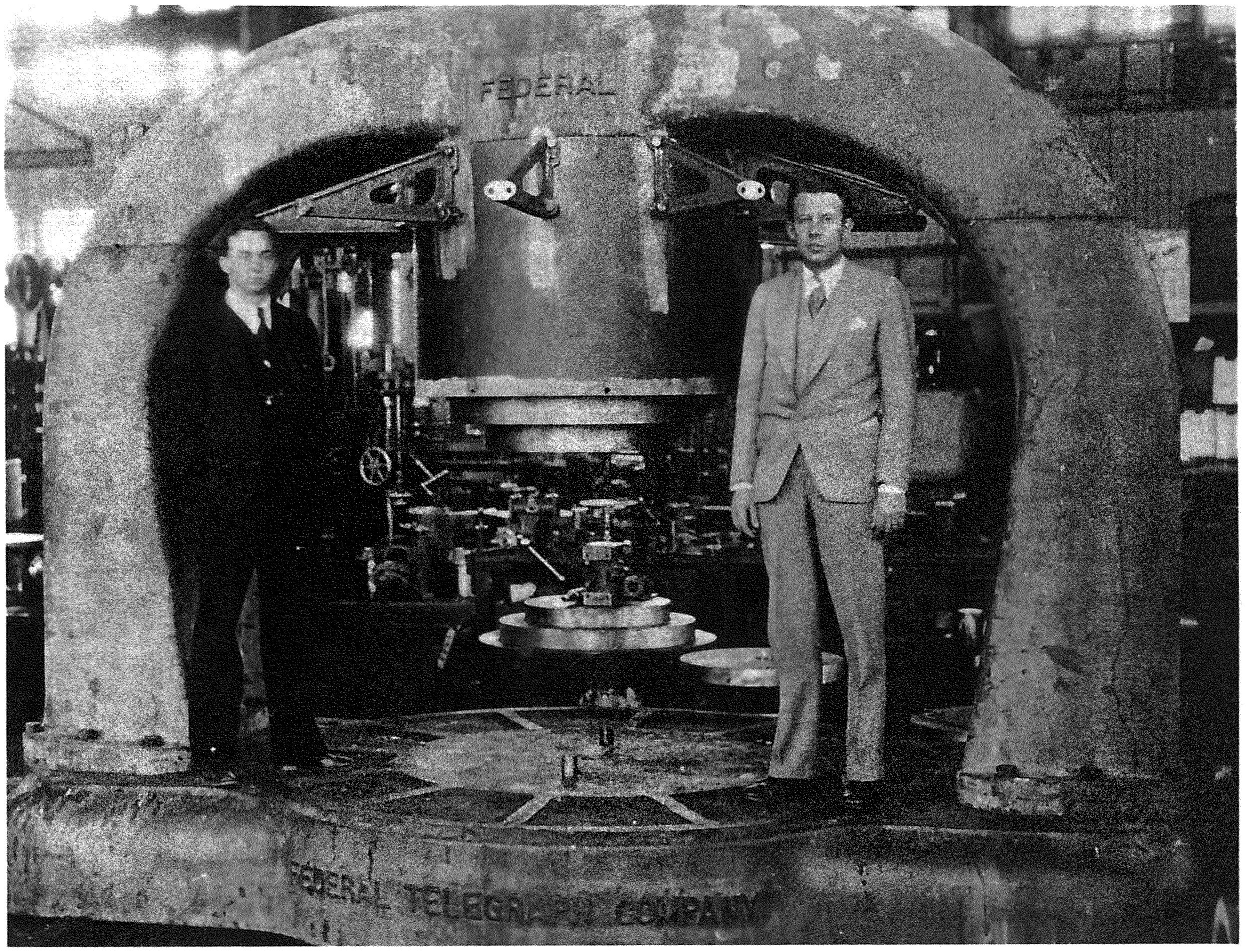


Figure 3

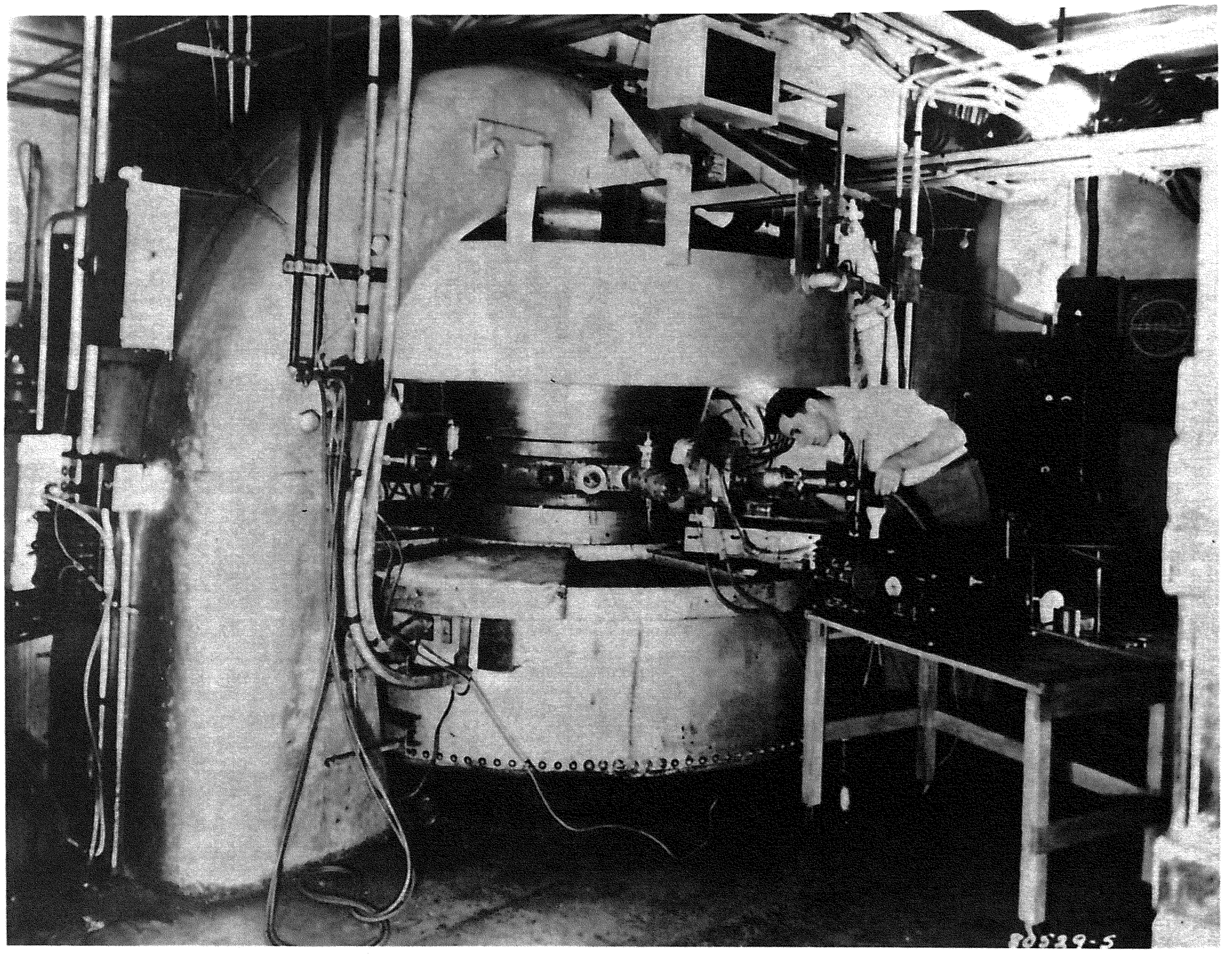


Figure 4

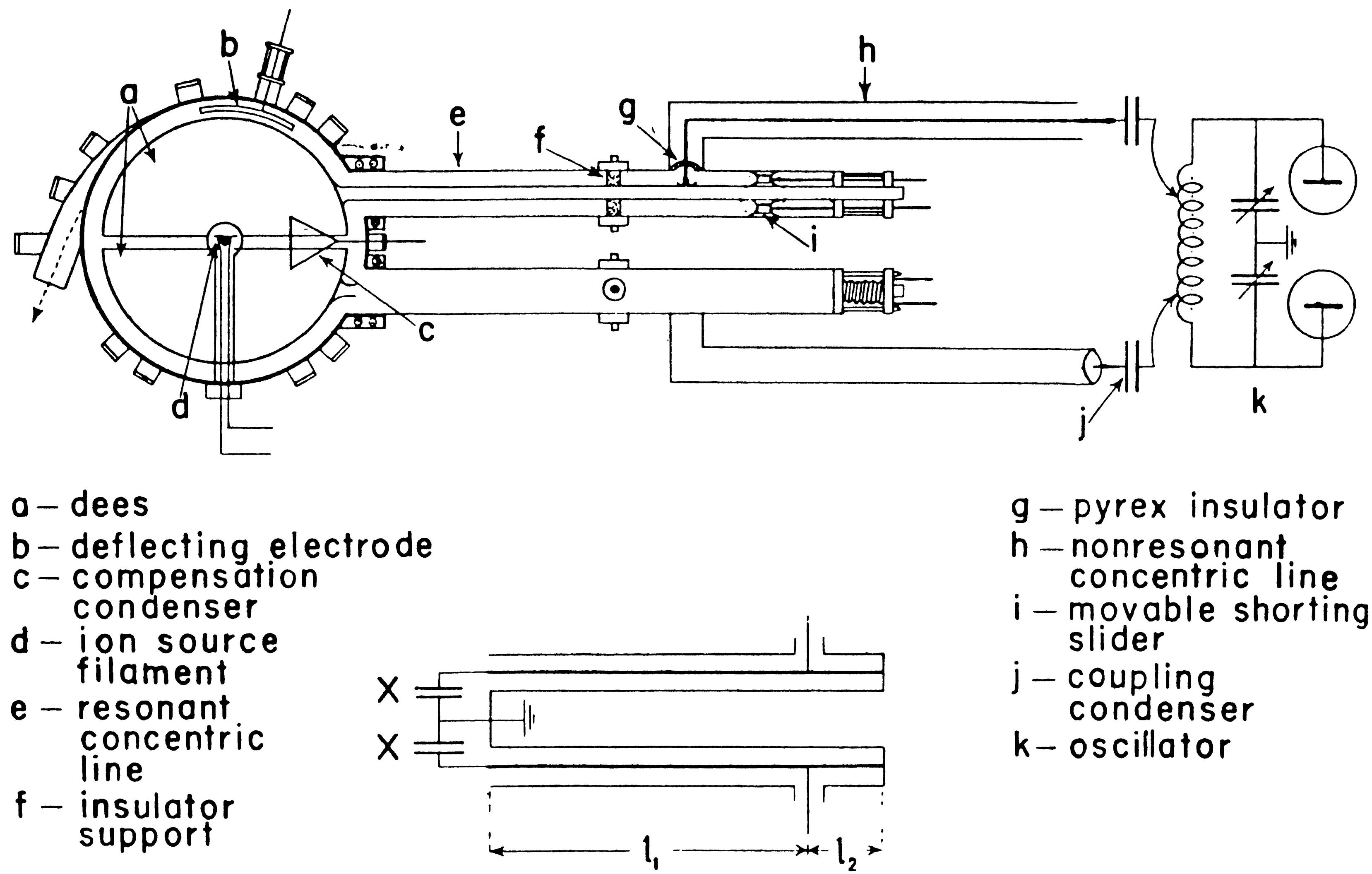

a
b
c
d
e
f
g
h
i
j
k
X
X
l_1
l_2
a — dees
b — deflecting electrode
c — compensation condenser
d — ion source filament
e — resonant concentric line
f — insulator support
g — pyrex insulator
h — nonresonant concentric line
i — movable shorting slider
j — coupling condenser
k — oscillator

Figure 5

The work was presented as a contributed paper at a meeting of the American Physical Society. Curiously enough, the paper I wrote was reproduced almost in full in the book "Le Cyclotron"(4).

Another picture of the cyclotron is given in Figure 6. It shows Dunning and me and the original concentric resonant lines. My official position was Research Assistant and although I was still only a graduate student I had contributed quite a lot to the building of the cyclotron.

Bohr's Excitement

I emphasize this part of my story because when 1939 came around and Enrico Fermi arrived at Columbia, I was in a position to make him an offer. Fermi had just won the Nobel Prize. He arrived with his family in New York on January 9. On the 10th he appeared at Columbia. He had decided to take up an offer of a professorship at Columbia rather than to return to Italy after receiving his Nobel Prize in Stockholm. On January 15, Niels Bohr arrived. He brought with him the news about the discovery of fission. He knew that Otto Frisch, then a young experimentalist at Bohr's Institute in Copenhagen was getting ready to verify the discovery of fission by direct observation of the energy release. It was Hahn and Strassmann(6) who discovered that uranium could be split by neutrons. They used radiochemical methods to show that uranium, upon bombardment with neutrons, would give barium, an element with an atomic number about half that of uranium. It was their work, published at the end of December 1938, that started the series of events recounted here. Niels Bohr, who had come for a stay at Princeton, was on his way to attend a conference in Washington(7). It was a meeting for theoreticians at which the latest developments in nuclear physics were to be discussed. By the time he was ready to leave Princeton, Bohr had heard the result of Frisch's experiment. It was a most exciting development.

Well, on his way to Washington, Bohr thought it would be a good idea to drop by and see Fermi to tell him about the exciting new physics. He came to the Pupin Physics Laboratory looking for Fermi. When he came in he went down to where the cyclotron was. He didn't find Fermi; he found me instead. I was the only person around. He hadn't seen me before but that didn't stop him. He grabbed me by the shoulder and said, "Young man, let me tell you about fission." I was, of course, greatly thrilled to have one of the great men of physics focus his attention on me. As you may know, Bohr doesn't really talk to you. He gets quite close and holds on to you while he whispers in your ear. Moreover, he was very excited about what he had to say. When he had unburdened himself he went off, presumably to catch the train to Washington. But I had heard enough to catch the excitement.

I was then a graduate student and the research I was getting ready to do had a lot to do with neutrons, so what Bohr had to say made a lot of sense to me. At the time, a lot of the physics at Columbia had to do with neutrons and Fermi's work was followed closely. Fermi was no stranger to Columbia. He had been there on previous occasions. Also, Eduardo Amaldi, one of his close collaborators in Rome, had come to Columbia before. I had come to know them both somewhat. At Columbia, neutron research was the main interest of John R. Dunning and Dana P. Mitchell, among others. In my case, I was getting ready to use the cyclotron as a source of neutrons for my Ph.D. thesis. As a result, I had already built a lot of the apparatus that might be used to study fission. When Bohr left I felt I had something to tell Fermi. He had an office on the 7th floor of Pupin. I went there and said, "Professor Fermi, I've come to tell you that I have just seen Professor Bohr. He was looking for you and he told me some very interesting things."

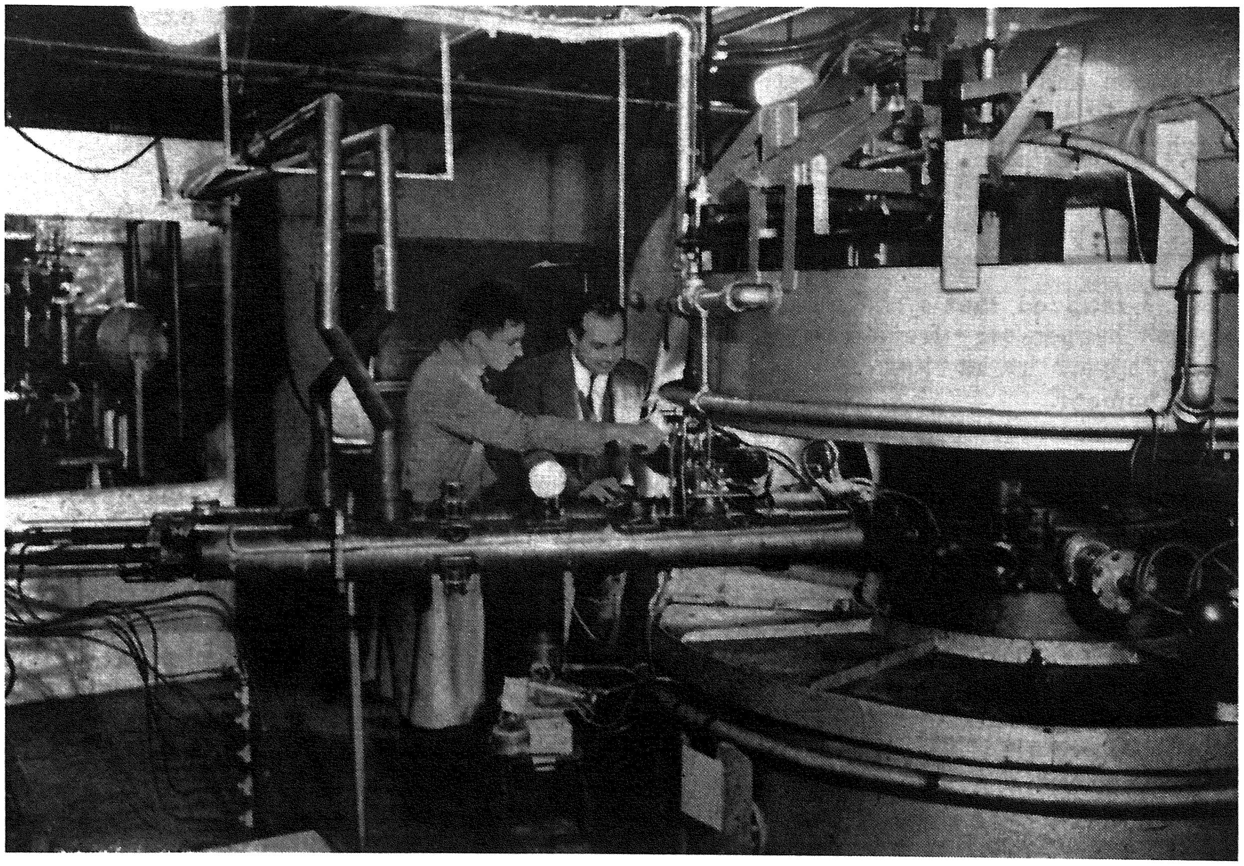


Figure 6

Fermi interrupted me. A smile broke out and he said, "Let ME tell you about fission." Then I heard again, but this time much more graphically, how the energy would appear when the uranium was split and the pieces flew apart by Coulomb repulsion. Then I knew this was what I wanted to work on and I said to myself, "Here's Fermi, he knows so much and is such a marvelous guy but he's just arrived; if he wanted to do an experiment he would need some apparatus to work with." Then I told him how I had helped build the cyclotron and made the suggestion that perhaps we could work together with it. He seemed to like the idea and nodded appreciatively. He realized, of course, that there might be some problems. He would have to clear it with Dunning.

Observing Fission

I realized that I had all the apparatus needed to look for fission right away. It doesn't happen very frequently that a graduate student will just happen to have just the right apparatus at just the right time to test out an important new scientific development.

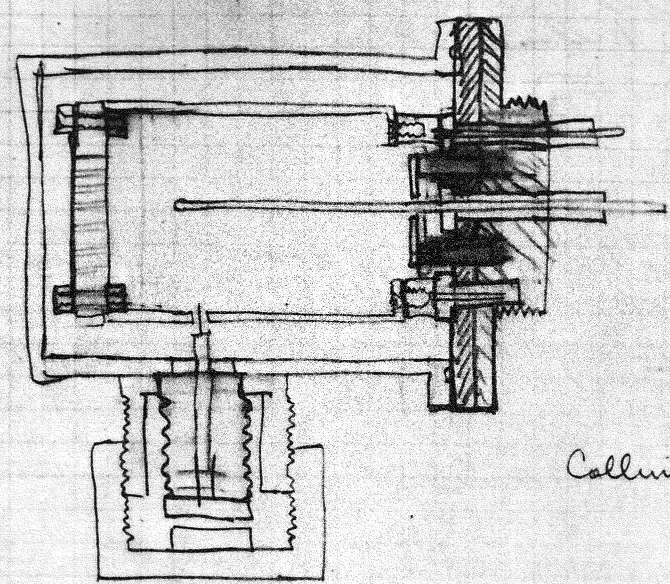
I have a notebook in which I wrote some of these things down. I thought you might like to see some of the pages from that notebook. Figure 7 shows a sketch of the ionization chamber I was going to use in my thesis work. With Fermi's help, I quickly converted it to look for uranium fission. We coated the inner electrode with some uranium oxide from the chemistry storeroom and connected it to the input of the linear amplifier I had built. We would do it differently today, but in those days of vacuum tubes we used an ionization chamber-linear amplifier combination. The number of ions produced by ionizing particles were measured without multiplication. You had to build a very good amplifier. I had built the amplifier and had the chamber ready and working. Here I calculated, as you can see from the figure, whether the alpha particles from uranium could, by pile up, give pulses big enough to look like those from fission. My calculation showed that the probability would be very low indeed. Then on the next page, Figure 8, is the record of the experiment!

On the very day I wanted to try the experiment the cyclotron wasn't working. You know how that happens. But I remembered that upstairs on the 13th floor there were those radon-beryllium neutron sources. Those were the neutron sources that Dunning and Mitchell had been using in their neutron research before the cyclotron had been built. In fact, since the cyclotron was only just beginning to work, much of their research was still based on those sources. Furthermore, it was one of my duties to help make those neutron sources. Among the things I did was to grind the beryllium to a fine powder and to put in the radon. Grinding beryllium affected my lungs and is the reason I am short of breath.

In Figure 8 there is a sketch that shows the ionization chamber, the radon-beryllium source, and some paraffin. There's some lead around to protect against the gamma rays and paraffin to slow down the neutrons. The ionization chamber was connected via the linear amplifier to a cathode ray oscilloscope. I simply looked at the pulses visually and wrote down how big each pulse was. As you can see from the figure, there was one that was 10 divisions high, another at 12 divisions, another at 11 divisions, and so on. Altogether, there were 33 larger pulses in 60 minutes. When I removed the source, there weren't any. That single page had all the evidence needed to verify that uranium does undergo fission with slow neutrons and releases a lot of energy, about 200 MeV, close to the amount Fermi had already estimated when he told me about fission. Well, here's the next page (Figure 9), in which I checked to see that the alpha particles alone wouldn't do it.

1/23/39

PRESSURE IONIZATION CHAMBER



Collimation Ratio 

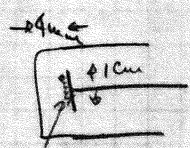
$\frac{a}{b} = .1$

—000—

1/25/39

DISINTEGRATION OF URANIUM

Using Neutron Chamber #1



Layer of Uranium Oxide

α particle kicks = 3000/min

Assuming Resolving time = 10^{-3} sec

This gives

$$\frac{265 \times 8}{60 \times 10^{3}} = \frac{3720}{60 \times 10^{3}} = \frac{1}{15} \sim .07$$ Average per 10^{-3} sec

265×6

Probability of Having 10 α particles recorded simultaneously

$$\omega(n) = \frac{\nu^{n} e^{-\nu}}{n!} = \frac{(.07)^{10} e^{-.07}}{10!} = \frac{2 \times 10^{-12}}{3.6 \times 10^{6}} \cong 10^{-18}$$

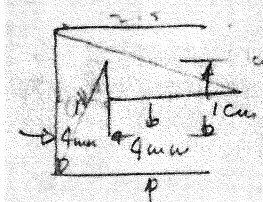
Average time for 10 α particles $= \frac{10^{-3}}{10^{-18}} = 10^{15}$ seconds

= practically never.

Figure 7

Ra-Be Neutrons (500 mc)

Most kicks are due to .4 cm range α part ~ .65 MV
These are 1 div on CRO (Gain set at 1.8)
Good many 3 div kicks these we ascribe to A



$R = \sqrt{1.4^2 + 4^2} = \sqrt{21.2} \cong 1.5$ cm corresponds to 2.7 MEV

14x
14
56
14
196
16
212

Now large kicks which occur infrequently abt 1 every 2 minutes.

Time	Size in div
9:22:15	
9:25:55	10
9:28:05	?
9:30:10	12
9:31:15	11
9:34:45	?
9:36:20	11
9:38:25	12

16 = 2
Visually

By Counter 33 in 60 minutes = .55 count/min

—with Neutron Source Removed:— in 20 min — 0 counts.

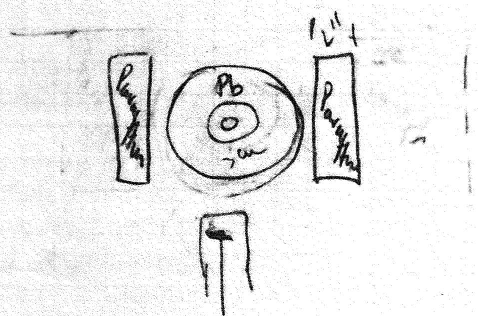
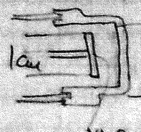


Figure 8

1/26/39 65

Chamber #1 Converted to do Uranium Expt

2



- Background of α particle kicks 15:00:6

—— ooo ——

1/27/38

α PARTICLE BACKGROUND

CRO at 1.8 (longest kick in 1/2 min)

6

	Amp Gain	Size of Longest Δ	Counter On	Counter off	Time	Count/min	Time
No Source	1:100		13:00:6	28:21:4	20 min	370	2:07 PM
micro phone	2:80	13	4:00:6	14:42:1	1 min	4976	
	2:60	12	0:00:3	9:50:7	1 min	4720	
	2:40	11	14:00:7	17:47:0	1 min	1809	
	2:20	10	18:00:0	21:5:6	1 min	1486	
	2:00	9.5	22:00:6	25:47:3	1 min	1813	
	1:100	6	25:00:3	25:34:7	1 min	279	
	1:90	5	26:00:5	26:2:1	1 min	12	
	1:80	5	27:00:1	27:2:4	1 min	19	
	1:70	3.7	29:00:7	29:1:5	1 min	6	
		4.0	1:00:1	1:00:2	2 min	.5	2:50 PM

2 minute watch of CRO Here showed 4.0 kick will kick Thyratron.

1:50	3.5	1:00:2	1:02:0	20 min	.61
1:00	2.3	2:00:0		90 min	

Figure 9

That was the 25th of January and by that time Fermi as well as Bohr and many others were already in Washington for their meeting. Professor Dunning, who showed up that evening, was very excited by the result I'd gotten and immediately dispatched a telegram to Fermi saying that we'd verified the fission of uranium by the ionization chamber method. Fermi, of course, could appreciate all the implications instantly. The next day there was a speech by Bohr and then one by Fermi about uranium fission and its possible implications. It was an exciting meeting. The net result of what went on was summarized in a news release put out for Science Service by Watson Davis, a well-known science reporter of the time. I still have a copy of the original mimeographed report that was circulated before it appeared in print (Appendix B). It says, "Is the world on the brink of releasing atomic power? This question..." There was a lot of excitement. H. G. Wells' scientific fantasy is referred to and I was very pleased to see my name in print. I was only a graduate student.

Fermi's Experiments

Fermi didn't stay until the end of the conference. He was anxious to come back and get to work. If you read the article, you'll notice that it says that almost everybody there coming from a lab that had a cyclotron or some other particle accelerator immediately called a colleague to say, "Look, why don't you set up and do so and so?" Within a day or two there were at least four experimental groups that had confirmed the result.

Anyway, Fermi came back to Columbia full of ideas. He called me to his office and wrote on the blackboard a list of the experiments he thought he should do. I copied the list in my notebook. Here they are in Figure 10. We just had to get busy and do those experiments. It was the 29th of January, only 20 days after Fermi had arrived in the country. The time scale for doing physics then, compared to now, still amazes me. We didn't do all those experiments but we did most of them. They didn't take very long to do. We didn't do #1, measure the lifetime by cutting off oscillations by blocking grids. Nor did we try to answer #2, can uranium be split by gamma rays? #3, remove uranium from the chamber—well that was an easy check experiment. #4, collecting the splitters on a cellophane foil and measuring the radioactivity deposited. This was an important experiment we actually did. An additional cellophane foil was inserted to show that some of the splitters had longer rangers than others. #5, measure the range—we did that. #6, uranium 239 may emit an electron with mean life of one second and then split. Do this if mean life is appreciable. We didn't do this one. #7, after splitting uranium, emission of neutrons? This became our most important line of research. It led directly to the chain reaction. To test the neutron emission, we used a favorite scheme of Fermi and Amaldi. The sketch of the arrangement, in Fermi's hand, was recorded in my notebook, shown here as Figure 11.

The experiment measures the total number of neutrons emitted by a source. The radon-beryllium source is inserted into the flask containing the uranium oxide. The rhodium foils which are activated by slow neutrons and therefore measure their intensity, are placed at various distances from the source. The water tank has to be big enough to stop all the neutrons. All the neutrons ultimately slow down and reach thermal energies inside the tank. The total number is obtained by doing the integral. Fermi liked to use rhodium foils. Rhodium has a 44 second half-life. So anyone who wants to measure neutrons with rhodium you have to run like hell. I mean you have to run from the place where you do the irradiation to the counter, some distance away, to be able to start counting the activity before much of it has died away. Fermi set up a precise schedule. He gave very careful instructions. He said, "You put in the source. After 60 seconds

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Fermi Suggestion for Expt:-

*① Measure life time by cutting off oscillator by blocking grids.

② Can U be split by γ rays?
How get rid of neutrons in this.

③ Remove Uranium from chamber

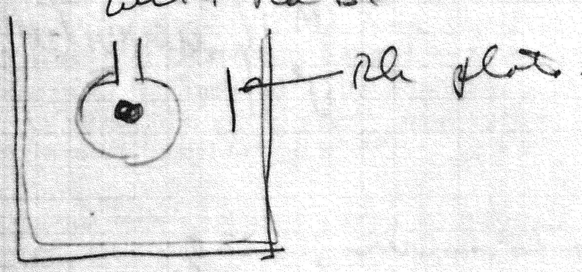
④ Possibility of collecting the splitters on a metal plate and then test activity of this plate

Possible put a metal foil in between to separate the splitters if they are different

⑤ Measure Range

⑥ U^{239} may emit electron with mean life of here and then split
do this if mean life is appreciable.

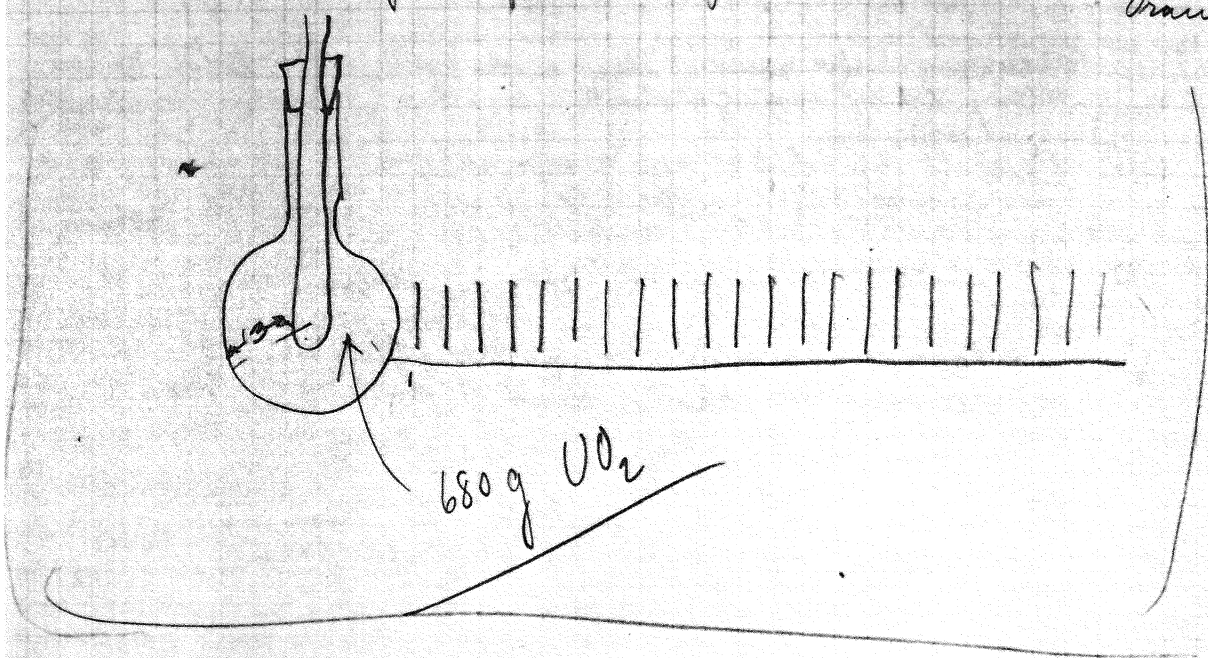
⑦ after splitting emission of neutrons?
Will U explode.
with Ra Be



*⑧ Run the first without nothing, then Th, Pb, etc

Figure 10

Activity in water as function of distance with & without Uranium



~~[illegible] 30 g~~

- 0^s Start irradiation
- 60^s End "
- 90^s Start reading
- 150^s End "

N without U

U with U

N^*, U^* } inverted detector

Rh Detectors 2 × 4 cm ; 1.22 g

Figure 11

remove the foil and hand it to me. I'll run with the foil, get it under the counter and start the measurement within 90 seconds. You put away the source and then come after me." We did a lot of running.

Further on in my notebook is a summary of absorption measurements made with boron and cadmium. The page shown in Figure 12 has various notations in Fermi's hand. The results were reported in a paper on the fission of uranium we published a short time later.

Physics moved very rapidly in those days. Notice that the date that experiment was done was February 11. We sent the paper on the fission of uranium(8) to the Physical Review on February 16. The publication date was March 1, 1939.

Fermi made every effort to put the experiment on a quantitative basis. You don't just describe the results, you give numbers. You need numbers for proper understanding. Well, the numbers weren't terribly good, but they were the best you could get at the time. We had a cross section for thermal neutron fission at 2×10^{-24} cm^2. The fast neutron fission cross was given as 0.1×10^{-24} cm^2. The paper(8) was signed by all the members of the cyclotron group.

Szilard's Neutrons

After that initial effort, Szilard came back into the picture. He was keenly interested in the chain reaction and realized that with Fermi there, Columbia was the place to be. All of a sudden he showed up. He persuaded Pegram, who was the chairman of the Physics Department at the time, to give him an appointment as guest scientist.

He had been looking for a process that would emit more neutrons than were absorbed. When fission was discovered, he saw immediately that this might turn the trick. He was already in the U.S. He was at Rochester at the time but early in January he went to Princeton to visit Wigner, an old friend. Wigner told him about Hahn's discovery. It seemed urgent to set up experiments which would show whether neutrons were emitted in fission. If this were the case it would be important to keep the information from the Germans. Szilard was very anxious to obtain Fermi's cooperation. At Columbia, Walter Zinn had some equipment which was suitable for the experiment Szilard had in mind. Enlisting Zinn's cooperation, the experiment was quickly mounted and soon produced a positive result.(9)

Szilard was very anxious to work with Fermi, or at least to have discussions with him. It turned out to be not so easy. Fermi didn't like the way Szilard worked. Their styles were completely different. There was one occasion in which Szilard and Fermi did work together. I was involved and a paper reporting the results was published under the names Anderson, Fermi, and Szilard(10). I have to say that the contact between Fermi and Szilard during that experiment was almost zero. I played the role of intermediary. It was published in the Physical Review in August 1939. The idea was to see if you could possibly get a reproduction factor high enough for a chain reaction with ordinary uranium and water. The result of the experiment was not encouraging. That paper was sent off in July and Fermi immediately went off for the summer. In those days, there was a popular summer school for physicists at Michigan. Fermi liked to go there. It was a reunion of physicists who came each year to tell what they had been up to and to exchange ideas. Fermi always gave a series of lectures, and he would listen to those given by others.

Absorbtion Measurements on Uranium Chamber
Summary

Data of	No	Boron	Cadmium
2/10/39	45.6 ± 1.2	22.25 ± .3	25.8 ± .5
2/11/39	32.64 ± .48	15.31 ± 0.16	17.79 ± .15

$$\text{Finally} :- 100\frac{Cd - B}{No - B} = 15.9 \pm 2.5$$

$$14.3 \pm 1.2\%$$

$$14.6 \pm 1.1$$

For boron chamber the same ratio is (2-11-39)

13.4 ± 1.3

Repeat!!

Cross section

Decay Const from A O Nier Phys Rev 55 p 150 (1939)

		Range
U_I	1.520×10^{-10} years^{-1}	2.63
U_{II}	2.6×10^{-6} years^{-1}	~~[illegible]~~ 3.18
AcU	9.72×10^{-10} years^{-1}	2.9?

Seconds in a year 3.18×10^7

Decay Const in sec^{-1}

U_I 4.8×10^{-18}

~~At~~ Atoms of U^{238} in 1 gm $= 2.53 \times 10^{21}$

1.21×10^4 disintegrations/sec

Figure 12

I didn't see much of Szilard that summer either. He didn't go very far away. The letters he sent off were from the King's Crown Hotel. A place near the Columbia campus. It was his favorite place to think.

I have to tell you how Szilard managed to interject himself into our experiment. 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, emits 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 that the effect was an important one but since the results would be less open to question, we repeated the experiment 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.

None of the neutron emission experiments were very conclusive, and Szilard urged a larger scale experiment. The three of us joined forces to carry it out. A great deal of physical work was involved. There were long thin cans to pack with uranium oxide powder and seal; we had to mix a huge solution of manganese sulfate after each irradiation; and to measure the radioactivity induced in it we had to stay up most of the night.

Fermi's idea of doing an experiment was that everyone worked. He generally worked harder than anyone else, but he expected everyone else to work hard. However, Szilard liked to spend his time thinking. He didn't want to stuff uranium into 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 could do these things better than he could. With this arrangement, everything went very smoothly. Krewer was very competent and did everything very well. But it was the last experiment in which Fermi and Szilard worked together. After that, a mutually satisfactory arrangement developed. Fermi did the experiments and Szilard worked behind the scenes to make them possible.

Out of Szilard's thinking came the idea of using graphite instead of water to slow down the neutrons. The trouble with water was that it absorbed too many neutrons. There was reason to believe that graphite would have a much lower absorption. Well, Szilard decided that we needed graphite and he wrote a letter to Fermi to that effect. Fermi actually responded. I have copies of those letters. Fermi had also been thinking about graphite.

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 until the evidence was clear; but that his idea of being conservative was to assume it would work and then take all the necessary precautions.

Wigner's Victory

The problem was where to get the money for the graphite. It was the kind of a problem Szilard liked. The route he chose, from Einstein to Sachs to Roosevelt, has been told many times. The point here is that Roosevelt appointed a uranium committee and it was to members of this committee that Szilard appealed. When he appeared before them, he brought two other Hungarians, Edward Teller and Eugene Wigner, with him. There were two ordnance specialists on the committee, Colonel Keith R. Adamson of the Army and Commander Gilbert C. Hoover of the Navy. After the case for graphite had been presented, the question of money arose. I quote Szilard's recollection(1):

> At this point the representative of the Army started a rather long tirade. He told us that it was naive to believe that we could make a significant contribution to defense by creating a new explosive. He said that if a new weapon was created, it usually took two wars before one knew whether the weapon was any good or not. And then he explained rather laboriously that in the end, it is not weapons which win the wars, but the morale of the troops. He went on in this vein for a long time, until suddenly Wigner, the most polite of us, 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 was what cost money, that is why the Army needed such a large appropriation. But he was very interested to hear that he was wrong. If this was correct, perhaps one should have a second look at the budget of the Army, maybe the budget could be cut. Colonel Adamson of the Army wheeled around to look at Mr. Wigner and said, "Well, as far as those $2000 are concerned, you can have it." This is how the first money promise was made by the government.

The meeting took place on October 21, 1939. Already in January the graphite began to arrive on the 7th floor of Pupin. Szilard had gotten the money, ordered the graphite, and had it delivered to Fermi. When Fermi saw the graphite coming in, he came to me and said, "Herbert, what are you doing on your thesis?" I told him that I had been making measurements and that each time I finished one I thought of three more to do to check up on one point or another. He then asked me to review for him what I had already done. When I went through and explained it all, he said, "That's fine, you've done enough. Why don't you stop now and help me with the graphite?" So I finished my thesis and started stacking graphite with Fermi. The $2000 had become $6000 but that was only a foot in the door. It wasn't long before we were requesting $100,000 and then more and more and more.

Move to Chicago

Having taken so much time to tell you how it all began, I have to bring you rather abruptly to Chicago. All that I have recounted so far took place at Columbia and you may wonder how we came to Chicago. There were a number of reasons. Number one was that Fermi himself was an enemy alien. He was not in a position to be the head of a wartime activity involving government money. An American was needed. However, the most eligible persons for that, John Dunning and Harold Urey, were already deeply involved in different aspects of the same enterprise.

Urey was interested in routes to the chain reaction using uranium enriched in uranium 235 by isotope separation. This seemed natural since Urey had made his fame and fortune separating the isotopes of deuterium. Dunning also got interested in uranium 235 enrichment. He had done an experiment using the cyclotron with Alfred O. C. Nier of the University of Minnesota. Nier had separated small amounts of the isotopes uranium 238 and uranium 235, brought his samples to Columbia, and with Dunning and Eugene T. Booth carried out an experiment at the cyclotron that showed that slow neutron fission took place in uranium 235 but not in uranium 238. This is what Bohr and

Wheeler had predicted but it decided Dunning and Booth to get into the isotope separation business. As a result, Columbia already had two major projects having to do with uranium.

At this point, Arthur H. Compton of the University of Chicago came on the scene. He was the chairman of a National Academy of Sciences Committee chosen to review the uranium projects and to judge their military importance. The Committee decided that our work using natural uranium was important. Important enough for Compton to feel he ought to head the project himself. Once in charge it was natural to centralize the work in Chicago. Who is to say this wasn't a good decision?

I have to confess that in a personal way I was pleased with the decision. I was a New Yorker. I had been born, bred, and educated in New York, and the idea of going west seemed very appealing. A visit to Chicago was arranged and I fell in love with the campus of the University of Chicago. "Enrico," I said, "we really ought to go to Chicago." He was more than a little reluctant. After all, he had just come west a good deal and had settled with his family very nicely and comfortably in Leonia, New Jersey. But in the end he decided he would have to come. For a chain reaction using natural uranium, Chicago was the only game going.

We came to Chicago in February 1942. It was only a few months after Pearl Harbor, December 7, 1941. The Metallurgical Laboratory had been established only one day before with Compton as its scientific head in Chicago. By that time, it had been recognized that a nuclear explosion could be made not only using uranium 235 obtained through isotope separation, but also by using plutonium made in a nuclear reactor from uranium 238.

That was the route Fermi wanted to take. He called me to his office one day to persuade me to go with him. "Herbert," he said, "if you stick with me we'll get the chain reaction first. The other guys will have to separate those isotopes first, but we'll make it work with ordinary uranium." As an added inducement, because he knew something about my personality and my ambition to make a lot of money, he said, "Someday uranium will become very important and you'll become the president of the Uranium Corporation of America." Well, that didn't happen. But we did go to Chicago. Under Compton's leadership a large number of people came too. Among them there was Szilard who worked hard getting the graphite free from neutron absorbing impurities, and Norman Hilberry, who did a marvelous job procuring what was needed. Soon large quantities of graphite began to appear for us to test. Equally strenuous efforts were expended getting uranium in forms sufficiently pure. First we worked with uranium oxide. Then various people worked to produce uranium metal. Outstanding among those was Frank Spedding from Iowa State University. He was an expert in the production of rare earth metals. When he was told how important it was to produce pure uranium metal he got to work immediately. Spedding's uranium was an important component of the first chain reaction.

Testing Materials

With all this activity in motion, our problem was to design a chain reacting pile that would work. As each batch of graphite came in we measured its properties, especially its absorption of neutrons. Fermi had worked out two important measuring techniques. For the graphite we stacked the graphite bricks into what we called the sigma pile. The Greek letter "sigma" stands for cross section and we were measuring an absorption cross section. These piles were 4 feet by 4 feet by 8 feet high. A neutron source was placed near the bottom and indium foils were exposed at various points on the vertical axis above the source. From the radioactivity induced in these foils, we could deduce the absorption cross section of graphite. These measurements were carried out mainly under the supervision of George Weil.

For the uranium we constructed what we called exponential piles, so named because of the exponential decrease of the neutron intensity with distance from the source. These piles were much larger than the sigma piles. They were 8 feet by 8 feet by 12 feet high. The uranium was placed among the graphite bricks in a lattice array. Again, measurements were made with a neutron source near the bottom and indium foils exposed at various distances from it on the vertical axis. In the beginning, the exponential experiments were carried out in the West Stands of Stagg Field on the campus under the direction of Martin Whittaker, then under Zinn, and in the end by Zinn and me jointly. The record shows that the groups of Anderson and Zinn working together built and measured 16 exponential piles in the two-month period between September 15 and November 15.

While this was going on, preparations were being made to construct the first chain reacting pile at Argonne, a Cook County Park District site outside of Chicago, some 25 miles from the University. Martin Whittaker had been appointed director of the Argonne site. He would have been in charge of the whole thing. Except for a twist of fate this story might have been quite different. I can't tell you what my role would have been if Whittaker had remained in charge.

Compton's Decision

However, around October 15, the workers who were constructing the buildings at Argonne went on strike and it became clear that there would be a serious delay in our schedule. Fermi didn't see why we just didn't go ahead with the construction of the pile where we were already working, on campus in the West Stands of Stagg Field. We went to see Compton. Compton's response is recorded in the book he wrote, "Atomic Quest".[8] He listened to Fermi's argument. It made good sense to him and he told Fermi to go ahead. On the other hand, he wrote, "As a responsible officer of the University, 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 judge 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."

General Groves, when informed somewhat later what was going on, became very upset. He visualized what would happen to his Army career if there were an explosion and a piece of the City of Chicago suddenly disappeared. But then he decided it wouldn't be wise to intervene.

Building the Pile

So it happened that on the 15th of November we started to build the pile in the West Stands. We set up a small factory to machine the graphite to the right size and to drill holes for the uranium in the right place. Fermi wanted to build the pile with a shape as close to spherical as possible. This would minimize the surface/volume ratio and make the best use of the material which would become available. I then had the job of procuring a lot of wood. How do you erect a sphere on a flat floor? You get a lot of wood and cut it to form a spherical cavity as a base for the graphite and uranium lattice. I was the buyer for a lot of lumber. I remember the Sterling Lumber Company, how amazed they were by the orders I gave them, all with double X priority. But they delivered the lumber with no questions asked. There was almost no constraint on money and priority to get what we wanted.

To avoid the absorption of neutrons by the nitrogen in the air within the pile, we wanted to be in a position to remove the air and replace it with carbon dioxide. It was my idea to build the pile inside an envelope made of balloon cloth. Then the air could be pumped out and replaced by CO_2. I was thinking of the blimps and dirigibles that were made by the Goodyear Company. When I went to Goodyear they thought my request for a square balloon was a bit peculiar. But again money and priority proved persuasive and they did the job on schedule with no questions asked. It was wartime.

Very few pictures were taken. There is a sketch that shows these features very well. I show it here as Figure 13. The sketch has been published before; many of you may have already seen it. You can see the wood I talked about; you can see where the graphite was stacked in pseudo-spherical shape. The balloon cloth is clearly evident as is the elevator that was used to lift the graphite bricks to the level on the pile at which we were doing the stacking.

We had a crew of undergraduate physics students who did the job. The graphite bricks slipped easily on the surface of the completed layer below. It was easy to skid the bricks across the surface to be set in the right place. Some of the bricks had slots machined in them. The stacking had to be done with enough care to keep the slots lined up. These were for the cadmium strips we inserted within the pile. Cadmium is a strong neutron absorber and was used to control the reactivity of the chain reaction. Our cadmium control rods were simply strips of cadmium sheet nailed down on wood strips.

Every night when the quota of stacking had been reached, we removed all those cadmium control strips and inserted a boron-trifluoride counter to measure the neutron activity. Uranium has the nice property of spontaneous fission. It emits a few neutrons of its own, so it was not necessary to introduce a separate neutron source. From these measurements a plot was made to show the approach to criticality. I worked the night shift so I made the measurement every night and would bring it to Fermi in his office promptly at 8:00 AM the next morning. He would add it to his curve showing how close to criticality we were. He would then spend the day calculating the best way to place the material to be stacked next. A major change in design came when we had news that Spedding would be sending some of his high purity uranium metal. The best place for this was as close to the center as possible. As a result, the shape of the pile was changed as we went along. The spherical shape we started with got squashed somewhat as we went along because the purity of the material we were getting was better than we had anticipated.

Figure 18 shows another view of the pile. This shows another aspect. It shows the control rod that was used in the test of the chain reaction. There were other more sophisticated control rods designed by the electronics group. They were designed to be operated remotely with various kinds of mechanisms. They worked well but Fermi preferred the hand-operated controls. There was one control rod called "Zip" designed by Zinn. He wanted a control rod that he'd have to hold on to. It had a number of weights on the other end. The idea was that if something happened to him the rope holding the control rod would release and the weights would pull the rod in automatically and stop the reaction. Precautions of this sort were taken but none were needed. The sketch is supposed to portray the scene on December 2, 1942, when Crawford Greenewalt arrived

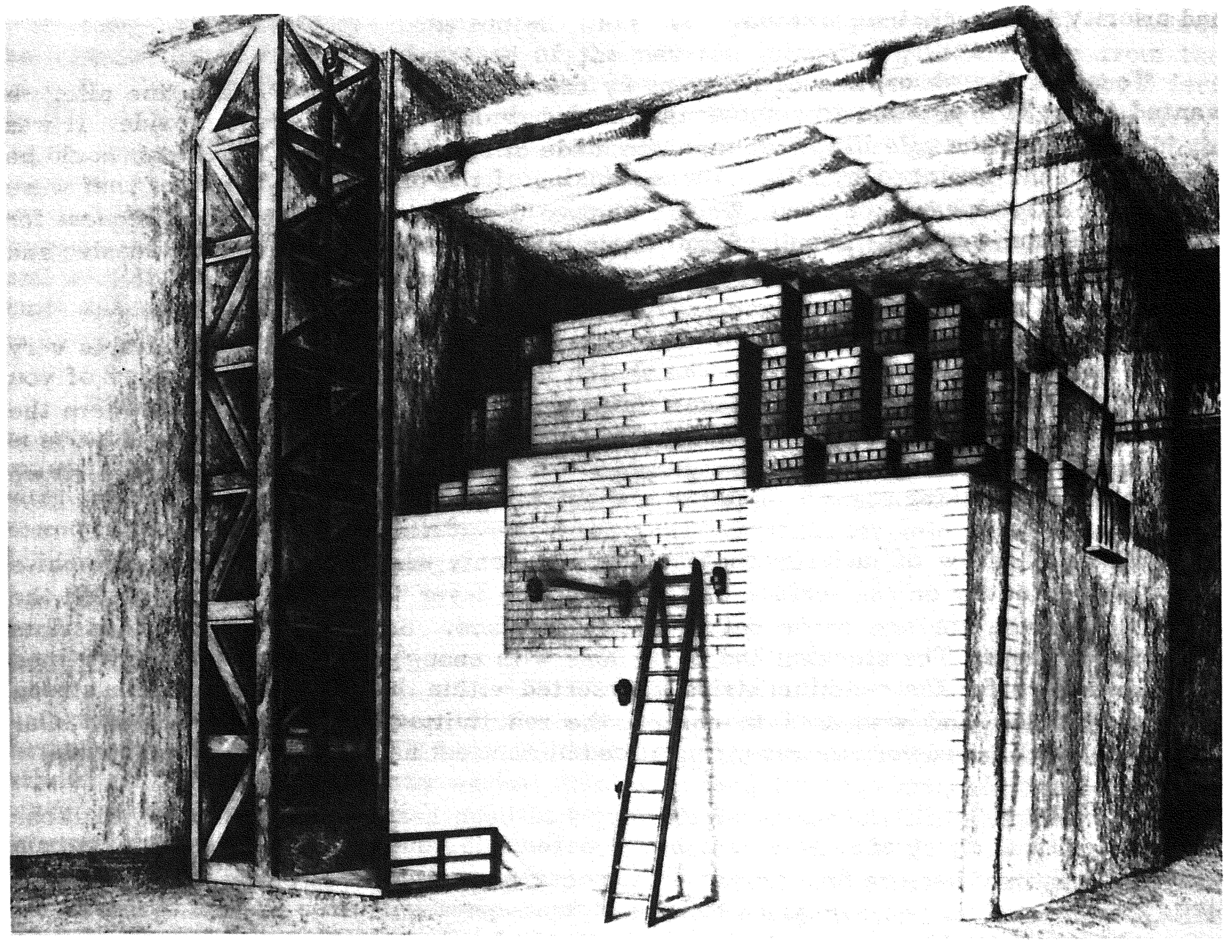


Figure 13

on the University of Chicago campus. He's with us now and I expect he'll tell you a little more about it tonight. I'd like to lead up to his part of the story.

The Chain Reaction Works

As it happened, on December 2, a group from DuPont arrived in Chicago, as part of a review they were conducting, to see where they could do the most good among the various activities of the Manhattan District. When they arrived Compton told them that Fermi was about to carry out his test of the first chain reaction. There were quite a few people already there. It was getting kind of crowded. There were the people who put it together and there were others who wanted to be present and had enough clout to get in. They can be seen on the balcony on the left of Figure 14. The DuPont group was invited to select one of their number to witness the performance. They chose Crawford Greenewalt. It was quite a show! I had very little to do at that time. Fermi was in charge. He soon began to issue instructions to George Weil who was down on the floor where he could manipulate one of the cadmium control rods. To register the neutron intensity, we had a boron-trifluoride counter. It was connected to a scalar which operated a mechanical counter. The counter made a loud sound every time it registered a count. It went clack! And after the next 16 pulses from the boron-trifluoride counter, it would go clack again. Just by listening you could tell what the neutron intensity was.

When he began all the control rods were in the pile. Fermi ordered all removed except the one operated by George Weil. He then asked George to pull that rod out a foot. Fermi recorded the activity as indicated by the counter, so many clacks per minute. The rod was pulled out another foot and a new measurement was made. Fermi would put each measurement on a graph and then, with a little slide rule, he would calculate where the next point ought to go. He had done his homework and knew what to expect. Each data point was analyzed on the spot.

These preliminary measurements went on for a while and in due course it became lunch time. It was Fermi's habit to go to lunch at noon and this occasion was no exception. It wasn't a good idea to do an important experiment on an empty stomach.

The serious work began after lunch. Fermi had calculated that the system would become critical by removing 8 feet of the cadmium strip. He called for the strip to be pulled one foot at a time. The increase in intensity was obvious to everyone on the balcony. You could hear those clacks and each time the strip was removed further the clacks came faster and faster. At each step Fermi would record the result, make a calculation, and announce something like, "The next time we pull out the strip by one foot, the rate will go from 600 to 1200 a minute." Then the rod would be pulled out and everybody could tell by the sound that the predictions were in the right ball park. They weren't exactly on but each time he got closer. You got the feeling that Fermi really knew what he was doing, that he had everything under control.

At a certain point he announced that by pulling out the cadmium strip a final foot and one-half, the pile would go critical. Instead of leveling off as had been the case before, the intensity would continue to rise indefinitely in an exponential fashion.

The rod was pulled out the specified amount and you could hear the counters clicking away—clickety-clack, clackity-click. They went faster and faster and then at a certain point suddenly there was silence. The rate had become too great for the counters to follow. It was a dramatic moment. An important threshold had been passed. Attention turned to the chart recorder. It was silent but could record much higher levels

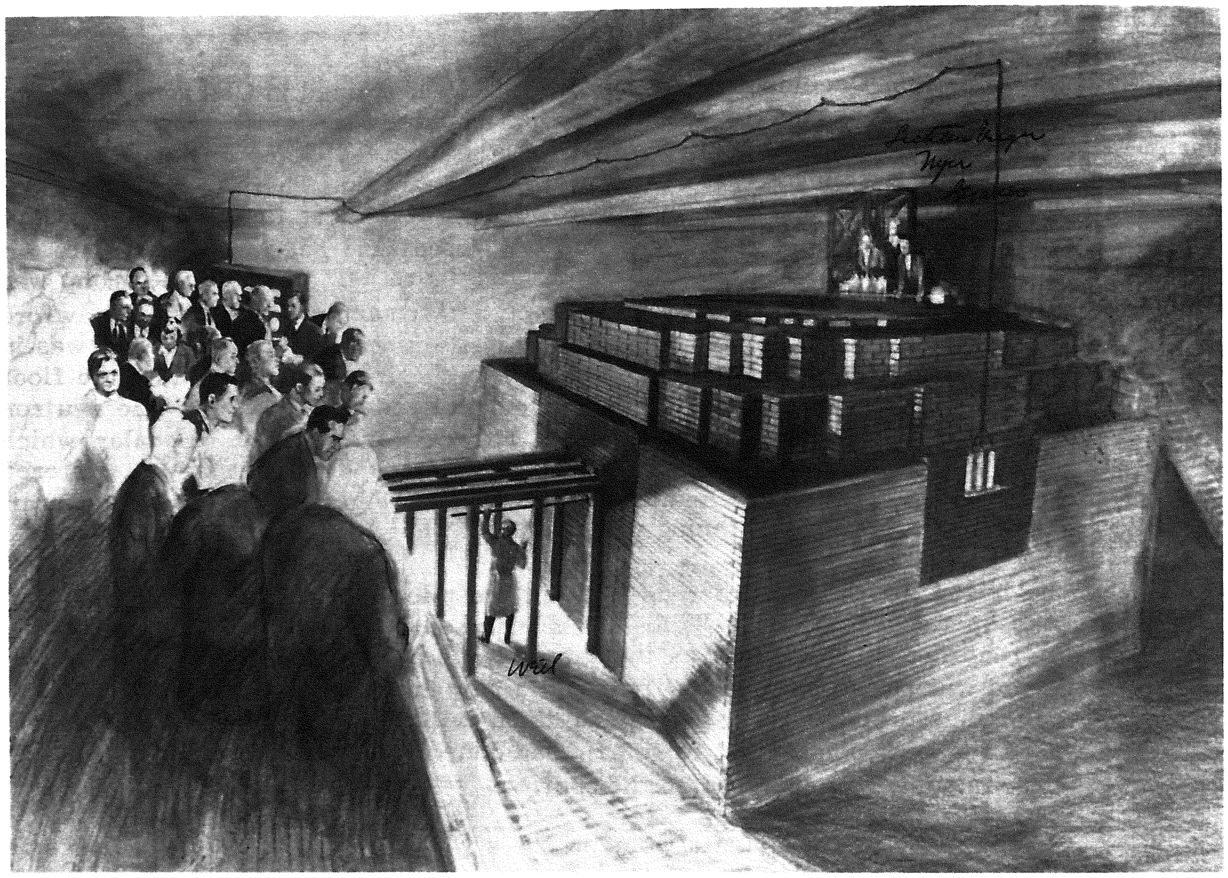


Figure 14

of intensity. You watched a pen moving across the scale as the chart advanced. It produced the record, now famous, shown in the figure on page of Appendix C.

The intensity kept rising and soon the pen was off-scale. So the scale was changed, the pen returned to a point near zero and then began to move across the scale again. The rise in intensity was exponential as the record shows. After a change in scale by a factor of 10, it was understandable that some of the onlookers might become a little nervous. They didn't hear anything, they didn't feel anything, but they knew that a dangerous activity was mounting rapidly. Everyone's eyes were on Fermi. It was up to him to call a halt. But he was very confident and very calm. He wanted the intensity to rise high enough to remove all possible doubt that the pile was critical. He kept it going until it seemed too much to bear. "Zip in," he called, and Zinn released his rope. The control rod he held went in with a bang and the intensity dropped abruptly to comfortable levels. Everyone sighed with relief. Then there was a small cheer. The experiment was a success.

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APPENDIX B

BEGINNING OF 1939 SCIENCE SERVICE NEWS RELEASE

APPENDIX B

Science Service Jan. 30, 1939

IS WORLD ON BRINK OF RELEASING ATOMIC POWER? THIS QUESTION ASKED AS BOMBARDMENT OF URANIUM RELEASES MILLIONS OF VOLTS OF ENERGY; EXPERIMENT MAY BE AS IMPORTANT AS DISCOVERY OF RADIOACTIVITY

By Watson Davis
Director, Science Service

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Washington — Is the world standing on the brink of the release of atomic power?

This question is paramount in scientific circles, following confirmation in several laboratories of the extraordinary release of atomic energy from the splitting of the uranium atom. Perhaps these experiments are more important than even the discovery of radioactivity itself.

First of all, the physicists are anxious that there be no public alarm over the possibility of the world being blown to bits by their experiments. Writers and dramatists (H. G. Wells' scientific fantasies, the play "Wings Over Europe," and J. B. Priestley's current novel, "Doomsday Men") have over-emphasized this idea. While they are proceeding with their experiments with proper caution, they feel that there is no real danger except perhaps in their own laboratories.

What is the new experiment that is so exciting?

Uranium, heaviest of stable elements and a sort of granddaddy among the radioactive elements that slowly disintegrate spontaneously, has been split with great release of energy. And the atom-splitting agency is the neutron, the electrically neutral particle discovered only seven years ago, itself a part of the hearts of atoms.

Bombard uranium with neutrons, even those with only a fraction of an electron-volt of energy, and its nucleus will split and give off millions upon millions of electron-volts of energy, up to 100,000,000 volts in actual experiments and some 200,000,000 volts theoretically.

This latest chapter of physics began its immediate phases with researches in Berlin by Prof. Otto Hahn. He observed the strange action of uranium under neutron bombardment but could not quite account for it. Dr. Liese Meitner, long associated with Prof. Hahn, and Dr. R. Frisch of Copenhagen, suggested the idea of uranium splitting into other elements, which although unheard of previously proved to be the case. Ironically, Dr. Meitner is now an intellectual refugee from Germany. She is temporarily working in Stockholm.

This work, just reported in Die Naturwissenschaften, German science journal, became known first by private communication from Prof. Hahn and later through publication. The Hahn-Meitner-Strassmann paper was the sensation of the theoretical physics conference in Washington last week under auspices of the Carnegie Institution of Washington and George Washington University.

Atom smashers were rushed into service to confirm or deny the German work. Long distance telephone and cables provided prompt communication. At least four independent confirmations have been obtained.

It was learned subsequent to the Washington conference that the experiment was confirmed in Copenhagen at Prof. Niels Bohr's laboratory two weeks ago (about Jan. 15). Prof. Bohr, world famed Nobelist, is himself in America visiting Princeton University.

Columbia University, whose research team consists of Prof. John Dunning, Dr. E. T. Booth, Dr. G. N. Glasoe, H. L. Anderson, Prof. S. G. Slack, Dr. George B. Pegram, and Prof. Enrico Fermi, confirmed uranium's energetic splitting on Wednesday (Jan. 25).

APPENDIX C

DECEMBER 2, 1942: THE EVENT AND THE PEOPLE

Albert Wattenberg

(Reprinted from The Bulletin of the Atomic Scientists, December, 1982.)

With the discovery of the neutron, the way was opened for a possible release of the energy locked up in the atomic nucleus. Otto Hahn and Fritz Strassman discovered nuclear fission in 1938, and the stage was set for the next great step—the man-made nuclear chain reaction.

ALBERT WATTENBERG

December 2, 1942: the event and the people

On Ellis Avenue between 56th and 57th Streets at the University of Chicago is a bronze plaque with the following inscription:

"ON DECEMBER 2, 1942, MAN ACHIEVED HERE THE FIRST SELF-SUSTAINING CHAIN REACTION AND THEREBY INITIATED THE CONTROLLED RELEASE OF NUCLEAR ENERGY."

If the event had been only the birth of a new era allowing the peaceful and safe use of nuclear energy or the development of a new and powerful research tool, it would have been wonderful. Unfortunately, it also made possible the production of sufficient plutonium to make nuclear fission bombs. This latter possibility and the threat of war had provided the motivation and support needed for the rapid development of the first nuclear reactor or first chain-reacting pile, CP-1.

Forty-two people were present at that event. I was one of about two dozen physicists in their twenties who had been helping to build the pile or the instrumentation for it. Enrico Fermi had asked me to join him at Columbia University 11 months earlier. Most of the others were also there because physics professors at their schools had asked them to help.

While a college student in the 1930s, I had helped organize meetings and demonstrations against both war and fascism. With Hitler's subjugation of most of Europe in 1940 and 1941, the fear of fascism in the United States became real and very personal. The announcement of the bombing of Pearl Harbor interrupted a concert to which I was listening; it was tremendous shock. In the days that followed, there was a great deal of patriotic fervor. However, my real fear was of the Nazis and not of the Japanese. Having decided to enlist as an ensign in the Navy, I asked for an application at the Columbia physics department, where they were available. But the head of the department insisted that I talk to Harold Urey, John Dunning, Lucy J. Hayner, and Enrico Fermi. They all knew me and asked me to join the projects they were running. Fermi had known me mainly as a student in the first quantum mechanics course he taught at Columbia in 1939, and we had talked several times outside of class. He certainly was my best teacher in graduate school. He had a very pleasant and easy-going style. He made everything look very logical and straightforward, and he never made any mistakes. (It took eight years before I found him making a mathematical mistake.) If I had an awe of Nobel-caliber physicists, I was not conscious of it since I had also taken courses from Urey and Rabi, who were not great teachers.

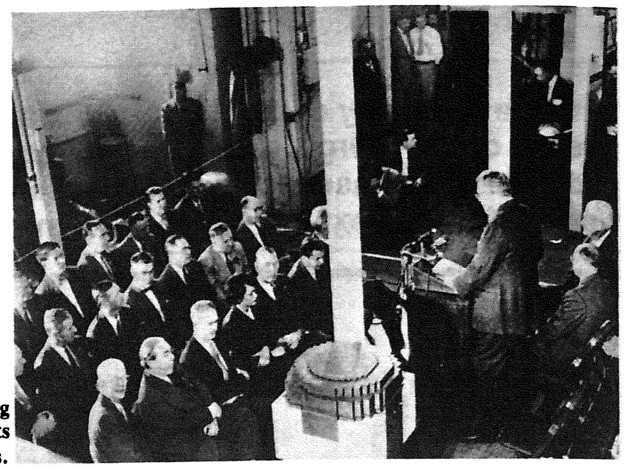
Exponential piles at Columbia University. I was very pleased that Fermi wanted me to work with him. When I joined the group in early January 1942, it consisted of Herb Anderson, Bernie Feld, Enrico Fermi, Leo Szilard, George Weil, and Walter Zinn. In February or March, Harold Agnew and John Marshall were sent from Chicago to help us complete a program of measurements before the group moved to Chicago. We all, except Leo Szilard, took part in making the measurements, in calculating the results, and in constructing the assemblies of graphite and uranium oxide.

Szilard quite correctly felt that his talents were better applied to things other than routine physics work. For example, he played a major role in persuading others to get financial support for the work from the U.S. government. It was Szilard who persuaded Einstein to write to President Roosevelt to alert the United States to the possible military applications of nuclear fission. Szilard also understood very early the importance of having very pure graphite and uranium, and he devoted a good deal of effort to procuring the test materials. Fermi, devoted to the theory and practice of physics, was very pleased to leave to Szilard the task of persuading industrial companies and other people to provide the needed materials. Szilard was an idea man, and it seemed to me that he and Fermi interacted rather well. Together, they were responsible for the very important idea of placing the uranium oxide in a lattice in the graphite instead of spreading it out uniformly.

Szilard was not discouraged by the fact that uranium metal had only been made in very small quantities. He was very persuasive in influencing laboratories and industries to produce larger amounts. High-purity graphite and some uranium metal did arrive in time to complete the project on schedule.

By contrast, Fermi acted as an equal in the laboratory. He did his share of building and measuring with the rest of us during the day. While we were making measurements, we worked day and night, and the younger men who were not married took the night shifts.

I recall that a good deal of my training in the use of Geiger counters was



Tenth anniversary reunion honoring the first self-sustaining nuclear chain reaction. The participants gathered once more under the West Stands.

by Bernie Feld; he and I both had an inclination to work at night. After a while, I was on the night shift alone, and the measuring routine was exceedingly rapid, with no rest. We measured the radioactivity in indium foils every five or ten minutes with Geiger counters. While accumulating the data, we carried through the calculations. The following morning, Fermi would check on all of the numbers that we had obtained to make sure that we had not made either numerical or clerical mistakes. He always made a personal summary of the measurements.

The indium foils had been made radioactive by being put in a neutron flux; so we were really measuring the neutron intensity. Fermi had a ritual: we made two measurements; then we ran a standard to check that the counters were reliable; then we measured without a foil or a radioactive standard in the Geiger counter. Fermi was always running checks on everything he did, especially on the reproducibility and reliability of the equipment with which he was working. This was a terrific training—when we encountered a new effect, we could be pretty sure that it was not due to our instrumentation.

The neutron intensity (flux) measurements were to ascertain whether a given lattice of uranium oxide lumps imbedded in graphite could give a self-sustaining chain reaction if the dimensions of the structure were sufficiently large. The results of the experiment could be expressed as a determination of a multiplication constant k, or reproduction coefficient. It is the fundamental property of the lattice structure and the neutron interaction probability of the material. It can be thought of as the average number of neutrons produced in the first generation by a primary neutron in a lattice of this structure if it had infinite extension. The Columbia group had developed this "exponential pile" technique the year before.

First they had studied a cubic lattice with the uranium oxide in the form of loose powder. When I joned them, they were starting to press the uranium oxide, and they had changed the lattice size. With a neutron source near the bottom, the neutron distribution decreased exponentially toward the top. The exponent, or the rate at which the exponential decay occurred, depended on the size of the structure, a measure of the properties of the graphite, and the multiplication constant k. The size dependence arises because of the neutrons lost by leaking out the sides of the structure. The exponential piles were not small—about eight feet by eight feet on the base and about 10 feet high. If you made a pile with just graphite and no uranium oxide, you could determine the properties of the graphite. From doing such measurements, we came to the conclusion that the graphite had serious amounts of impurity in it. By March 1942, we knew we needed purer graphite in order to obtain a self-sustaining reaction even in a pile of infinite size.

One of the last measurements we were trying to complete at Columbia was to determine the effect of gases other than air being in the interstices of the graphite. We were concerned that nitrogen acted as an impurity and that there was an appreciable amount of it in the porous graphite. We therefore wanted to put this large structure inside a sheet metal can, which required soldering together many strips of sheet metal. We were very fortunate in getting a sheet metal worker who made excellent solder joints. It was, however, quite a challenge to deal with him, since he could neither read nor speak English. We communicated with pictures, and somehow he did the job. We put a vacuum pump on this canned exponential pile, pulled the air out of it, and then filled it with carbon dioxide. From this it was established that one could increase the multiplication constant if one got rid of the nitrogen in the graphite. This result led to Anderson's obtaining a big cubical balloon to enclose the reactor that finally became self-sustaining.

Water in the graphite or uranium was an undesirable impurity. I have recollections of Bernie Feld building an oven that must have been 20 or 30 feet long to try to drive the water out of either the uranium oxide or the graphite bricks. I also remember spending the nights there to make sure that fires did not get started from all of the electrical heating rods.

After measurements of the pile in the tin can were completed, we put the graphite into numerous cardboard cartons. We glued the boxes closed, then shipped them, the Geiger counters, and the rest of our equipment to Chicago. At the outbreak of the war, Arthur Compton had been put in charge of the project, and after a few indecisive meetings, he decided that the project for "chain reacting piles" (subsequently and more appropriately called nuclear reactors) should be moved to the Chicago area.

Albert Wattenberg is professor of physics at the University of Illinois, Urbana-Champaign (61801), and a fellow of the American Physical Society. In addition to collaborating on the first chain reaction, Wattenberg helped design, assemble and test the first enriched uranium heavy water reactor in 1949.

Fermi, Zinn, and Anderson. In April 1942, we left New York, and Bernie Feld, Enrico Fermi, and I started living at the International House of the University of Chicago. Enrico Fermi was there because Laura and the children were still in New York. Fermi and I frequently ate dinner and spent the evening together. He told me that his work habits were better than mine—he stopped working at about 5:00, relaxed after dinner, and went to bed early. He could beat me at chess, but I beat him at ping pong. There was a continuous game of a student-teacher relationship when some of us were with him. If we saw something, he would ask us to explain quantitatively what was happening. For example, the fire in a fireplace led to his making us try to calculate the amount of vacuum above the fire; seeing a dirty window, he asked us how thick can the dirt on a windowpane get? To play the game you only had to know the fundamental constants of nature and have some idea as to how things might vary with one another. If you got off to a poor start, he would help you by asking you to go to some extreme condition where the answer was obviously ridiculous. He was very quick in thinking up such tests which he used when he made his own formulations. He gave us the wonderful feeling that we also could and should know all physics.

Fermi enjoyed teaching and wanted those working with him to understand what was going on. This led him to give two sets of courses in neutron physics at Chicago, which prepared many of us for the events that took place on December 2. What made him a wonderful teacher was that he would avoid proofs that were too esoteric or lengthy. Instead, he would develop a plausibility argument so that we felt it was almost obvious.

When we went to Chicago, the original New York group was split into two main groups—one under Zinn and another under Anderson—and an appreciable number of additional people were brought in to work with us. Another group under Volney Wilson was in charge of the controls and instrumentation development. Before the war, Wilson understood what nuclear bombs would do, and he had been unwilling to work on the project. However, in the fervor created by the war, he decided he should work on the project.

As well as measuring exponential piles, Zinn took charge of the machining of the graphite and the pressing of the uranium oxide. I feel that Wally Zinn's enormous contribution and cleverness in this project have not been adequately appreciated. Although Fermi understood the physics, could teach it to the rest of us, and had developed the techniques for making measurements, Zinn really had a full grasp of the steps and processes needed to build a pile. He was exceptional at devising straightforward, reliable, and efficient solutions to an enormous variety of physics and engineering problems. He arranged for the personnel and equipment needed to get the job done. With about half a dozen young physicists, one millwright, and about 30 kids who had dropped out of high school, he carried a major share of the physics measurements of the exponential pile program, and he machined all the graphite blocks and pressed the uranium oxide briquettes for those exponential piles and for the first self-sustaining pile. Zinn drove people hard; he obtained very high quality work from them and also gave them a great deal of satisfaction in accomplishment.

Zinn had no draftsmen until 1943 when he took charge of designing the first heavy water reactor, although I did some drafting for him a couple of times. The loose uranium oxide powder needed to be pressed to a density as high as possible. He designed a die that was built at Columbia. At Chicago he designed beautiful new dies that would make the lumps somewhat spherical in shape (we called them pseudo-spheres). He probably was assisted in decisions on the dies by a very bright and superior tool and die maker in the Physics Department shop, whose name, I believe, was Dicostanza. The oddly shaped die and all its parts had a fantastic polish, which meant that we could minimize the use of lubricants and avoid getting the lubricant on the uranium oxide lump. The lubricant would have absorbed neutrons. Zinn also had the foresight to have some spare dies made.

Prior to 1939, Zinn had been a professor at the City College in New York. He had had his own independent research program studying neutron interactions at Columbia University. After fission was discovered, he and Szilard began doing experiments together; sometime in 1939, they joined forces with Fermi and Anderson. A very interesting account of this period and the entire war period is given by Herb Anderson in the publication *All In Our Time.*[1]

Herb Anderson was already a very advanced and experienced graduate student in 1939 when Fermi arrived. He started working with Fermi almost immediately. Anderson had experience with experiments at the cyclotron, and this permitted them to study the very important question of the production of neutrons in the fission process. I gather that Anderson became a family friend; since he knew how things were done in America, he was helpful to the Fermis in their initial period in the United States. He also was very effective in arranging to get equipment and the other things needed to carry through the program. He was exceedingly conscientious about keeping up with the theoretical aspects of the physics with which he was involved.

Fermi involved Anderson in a very

In April 1942, we left New York and Bernie Feld, Enrico Fermi and I started living at the International House of the University of Chicago.

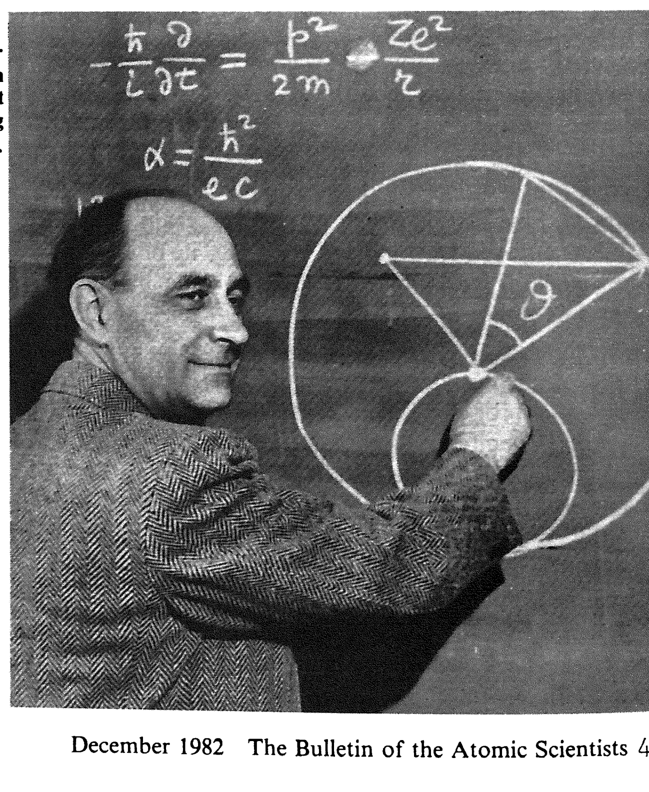
broad variety of experiments. As well as studying the fission process itself, they put a great deal of effort into establishing standards for neutron measurements. He studied the spectrum of neutrons from various sources. The neutron sources used in the exponential piles consisted of radium mixed with beryllium powder; Anderson was the expert on preparing these sources. Probably due to working with the beryllium powder, after the war he developed berylliosis, an illness which affected much of his later life. Bernie Feld and I were both trained by Herb in how to prepare these radium-beryllium sources. Anderson and Feld went to Los Alamos in 1944; since they could not travel, I ended up making neutron sources for the entire Manhattan Project.

The sources consisted, in many cases, of as much as a curie of radium. At 10 centimeters from a curie of radium in equilibrium with its decay products, a person would be exposed to the order of 100 roentgen per hour. The soldering in such work required great quickness and sureness. Actually, in making the radium-beryllium sources, we worked with radium which had had all of the radon driven out of it; a radium solution containing beryllium powder was boiled down to a dry powder which we transferred into brass containers which we sealed by soldering. We had several hours to work before the radioactivity built up to a very dangerous level. After the radium was sealed up for several days, however, it came into equilibrium with its radon decay products and then presented the hazard of a full curie of radioactivity. I repaired several sources of this type in which the solder joints had broken. Subsequently my white blood count dropped to a level of about 4,000, about half my normal count. From my work with the radium sources during the war and making up these neutron sources at other national laboratories, my blood count remained low for a number of years.

Exponential piles in Chicago. The exponential piles that we had built at Columbia had given results indicating that even an infinite amount of material would not lead to a self-sustaining structure. This was mainly because of the impurities in the graphite. During the spring, some new graphite arrived, and the exponential piles we built and measured in Chicago indicated that it would be feasible, with a very great deal of material, to build a chain-reacting structure; thus, it was very worthwhile to continue to get the better quality graphite and also to improve the quality of the uranium oxide that we were obtaining. As well as Szilard, Ed Creutz and Frank Spedding were also involved in this effort. Those of us doing the exponential pile work were not involved in discussions of the procurement of better materials, and I think that we didn't really appreciate the efforts that were going into this, both by people in the project and by those in the industrial companies. I do not remember these procurement problems being discussed at the general meetings that were held in the spring and summer.

My recollection is that general meetings took place in the evenings either every other week or once a month, and many of us attended. They were interesting because other groups discussed their experiments, calculations, or problems. The impressive array of people in the theo-

Enrico Fermi, leader of the scientific team that achieved the first controlled, self-sustaining nuclear chain reaction.



Fermi (far right) being admitted to the Italian Royal Academy, 1929.

Zinn drove people hard; he obtained very high quality work from them and gave them a great deal of satisfaction in accomplishment.

retical group under Eugene Wigner included Al Weinberg, Bob Christie, John Wheeler, Gale Young, and others; and a group using the Chicago Cyclotron under Snell was studying individual aspects of the fission process, such as the losses of fast neutrons.

The motivation for the project, to produce plutonium for a bomb, led to a very large effort on the chemistry and the separation of plutonium from uranium. Glenn Seaborg was the head of the nuclear chemistry division; James Franck joined that division in the fall. Quite a few engineers were studying the designs for various alternative large-scale chain reacting assemblies. A few of the engineers were good; but some of the engineering firms were very stupid. One engineering firm we got rid of had hired new people to work with us, and they seemed to have specially selected second-rate people for us and to have kept their first-rate people in their home office working on other projects. In the winter the DuPont Company joined the project, and they sent some of their best people. It was a real pleasure to train them.

Although we were committed to building a graphite-uranium pile, other possible systems were always being considered and some measurements of them had to be made. During the summer, according to my recollection, we tried to measure the multiplication constant of a beryllium-uranium oxide system. Beryllium metal certainly has very excellent characteristics as a neutron moderator in a reactor. However, it was not available in large quantities, and it would have been very difficult to machine. At the time, we did not know about the physiological hazard of handling beryllium; many people would have been hurt if we had tried to build a pile of beryllium metal.

From the middle of October until December 2, we were on a regime of about 90 hours of work a week. All we did was to work and sleep, and sometimes we didn't even get to eat meals. Sometimes we thought of why we were doing it; several times we discussed what we would do if the Nazis won—where we would try to hide in the United States. We were fairly certain we would be killed if we were caught. One morning very early Dr. Alvin Graves came in (he was slightly older than the rest of us) and wanted to take over what I was doing, although he wasn't due in until late that afternoon. He said he just couldn't sleep. He felt the Nazis were working, that they were pushing ahead to get there before us. We were in a real race, and he felt he shouldn't be taking a day off. But when you are working 90 hours a week, you listen to the news on the radio while dressing, only glance at newspapers, and spend little time discussing the world situation—you keep functioning and solving the problems that arise in your work.

In addition to running the factory, we shared the shifts in measuring the neutron fluxes in the exponential piles. Early in October we had machined enough of the new high density purer graphite, and we had pressed enough new uranium oxide pseudo-spheres to build exponential pile 18. This was to test whether the pile would be self-sustaining with the amount of new material that would arrive by December. When we had completed measurements, we knew that such a graphite-uranium pile would work. From Fermi's lectures, we understood the significance of what we had measured and how big the pile would need to be in order to be chain-reacting. Two additional improvements would make it a certainty. The big balloon-cloth bag purchased by Herb Anderson would allow us to get rid of the air in the graphite and therefore reduce the amount of neutrons absorbed by the nitrogen in the air. The other improvement was that some uranium metal, which is much better than uranium oxide, would be arriving and could substitute for the oxide, especially in the center of the reactor.

Until the end of September, the preparation of the materials and the construction of the exponential piles were carried on mostly by Zinn's group. Anderson's group had the responsibility for the standardization of neutron measurements. In October, in order to get all of the work done, the groups under Anderson and Zinn were combined, and other physicists joined us on a temporary basis.

The graphite and uranium oxide factory. The first self-sustaining pile was designated CP-1, Chicago Pile 1. For it to be self-sustaining, we needed to press about 22,000 pseudo-spheres of uranium oxide, and we had to machine about 400 tons of graphite. The graphite was received from the manufacturers in bars that were about four and a quarter by four and a quarter inches in cross-section and in lengths that varied, depending on the manufacturer, from 17 inches to 50 inches. Surfaces were quite rough, and therefore it was necessary to make them smooth and to cut the bricks to an accurate standard length. For the lengths we used a woodworking cut-off saw and it turned out that woodworking machines were the best thing to use. For the cross-section, two of the surfaces were made plane and accurately perpendicular in a jointer, and then the other two surfaces were brought to the size we wanted—four and an eighth by four and an eighth—by running them through the planer. These machines were set up and maintained very well by Gus Knuth, a millwright and carpenter with a great deal of ingenuity. The rest of us who worked with him most of the time were physicists and a motley

From the middle of October until December 2, we were on a regime of about 90 hours of work a week.

crew that had been rejected by war industries. The graphite machining produced black graphite dust all over the place. We breathed it, slipped on it, and it oozed out of our pores, even after we washed and showered. Everyone dressed for this work in coveralls, and a young professor could not be distinguished from the kids we hired from the area of Chicago known as Back of the Yards (the livestock yards).

One quarter of the graphite bricks needed to be accurately drilled to provide three and a quarter inch diameter holes shaped to fit the uranium oxide pseudo-spheres. Each hole was drilled in a single operation by using the lathe in an unorthodox fashion: we put the graphite bricks where the tools should be and a special homemade spade into the rotating chuck of the lathe. This rickety lathe wasn't even second-hand —it was probably fifth-hand. These tools required frequent sharpening, which proved to be time-consuming. We had tried carballoy bits but rejected them. Instead, we made the drilling tools from old files. Harold Lichtenberger, Bob Nobles and I took turns at shaping and re-sharpening these tools. Between 60 and 100 holes per hour could be drilled. After drilling 50 to 70 holes, however, a bit had to be resharpened, so about 30 bits a day had to be reground. We did not have a jig, so we did it by eye.

We handled the 400 tons of graphite a number of times, unloading trucks, storing it, machining it. Then we either used it in an exponential pile or stored it again until we put it into the pile. One day we received a telephone call saying that a shipment of graphite was in Chicago at a railroad yard, but that they could not get it unloaded. This provided an interesting sociological insight. The truckers who unload freight cars use itinerant labor, mainly picked up at the flop houses on West Madison Street in Chicago. Apparently during the war there were more jobs than men, or they had enough money to sleep several days and nights, so the truckers couldn't get any itinerant labor to work for them. When we learned this, we of course went down and unloaded the freight car ourselves. A carload contained around 50 tons of graphite—in this case about 2,000 bricks weighing about 50 pounds each. Four of us unloaded the freight car in less than a day and then went back to work.

The uranium in the pile was mostly in the form of uranium dioxide lumps, the remainder in the form of about six tons of uranium metal which we simply had to put into the holes that we had already drilled. We originally planned to press 22,000 uranium dioxide lumps from loose powder. We had an old press and the dies designed by Wally Zinn. On a good day, working three shifts and using two dies, we could press about 1,200 lumps in 24 hours. At the beginning, a team consisted of three Back-of-the-Yards high school kids and usually one young physicist like Lichtenberger, Nobles, Warren Nyer, myself, or others. At the end of October, the Back-of-the-Yards kids were drafted, and more physicists were used to help us. The work in the press room was fast and monotonous. To keep up a fast pace, we frequently sang. We made mistakes only a couple of times during the whole period.

When we had completed about three quarters of the pressings needed, one of the poles of the press cracked. The press was another example of the third- or fourth-hand equipment that we were using. We reduced the pressure a little bit so that we didn't break the press completely. We sang a little louder and kept our fingers crossed.

After we had been doing this for quite a stretch, some physicians walked in with cages full of mice. They were putting them in the room because of the uncertain toxicological effect of breathing and eating uranium oxide. The medical experiment was to see how long the mice would live breathing uranium oxide dust. The mice would be there 24 hours a day; we would be there only 12. The physicians also brought dust masks for us to wear. It is very difficult to get people to start wearing dust masks, especially if they are smokers who like to sing while they work! The Back-of-the-Yards kids refused to wear the masks after a short while; and the physicists did not set a very good example.

The University Commons was the most convenient place to eat, but it was an appreciable walk, especially on a cold and snowy day. There was only one—very miserable—lunch counter close to where we worked. The only edible thing the little old lady could make was mashed potatoes with gravy; even her hamburgers came out like shoe-leather. In that period, for many of us, smoking was a way to avoid getting hungry so that we could skip meals. However, the cigarettes available during the war were terrible. A pack of Fatimas today would be a collector's item.

Preparing for December 2. Harold Lichtenberger came from Decatur, Illinois and had just received his B.S. in physics. He was observant and quick to grasp new concepts in physics. His previous work experiences were very different from the rest of ours: he had worked in the repair shop of a locomotive yard and knew how to handle large, heavy equipment. The rest of us learned a great deal from him.

The floor level of the squash court in which we were going to build the pile was below the level where we were doing all of the machining of the graphite. We had to be able to bring skid loads of graphite bricks down to that floor. A portable elevator was obtained (I assume by Zinn), and was

The big balloon-cloth bag purchased by Herb Anderson would get rid of the air in the graphite and reduce the amount of neutrons absorbed by the nitrogen in the air.

delivered to the door of the West Stands of Stagg Field. Harold Lichtenberger played a major role in getting that elevator moved the hundred or so feet from the entrance to the squash court. Then we had to get it down onto the squash court floor and arrange to get it right side up. We enjoyed new challenges like rigging.

Prior to the arrival of the elevator, we had passed the graphite blocks by hand from the floor up to physicists who were on a wooden scaffolding. We would work 8 to 12 feet off the floor on 2 by 12 inch planks. General Leslie Groves walked in on us while we were building an exponential pile, and he was upset. I suspect that we got the elevator to avoid his criticism. After a while, most of us were very much at ease walking out on the planks, although a few did not adjust to it. About sixteen of us helped build the exponential piles, then irradiated and measured the indium foils.[2] While we were at the West Stands, Anderson's group was responsible for the Geiger counters and the associated electronics.

Fermi's monthly report for October indicates that we built and measured seven exponential piles, as well as running the factory. Several of the exponential piles were to study the reproduction factor that could be obtained from using uranium metal produced by Westinghouse. We tried to find the optimum amount of metal to use in the graphite lattice. The results were very encouraging and showed an improvement in the reproduction factor of more than 3 percent over that obtained with uranium oxide. The arrival of additional uranium metal in time to be placed near the center of the reactor certainly reduced the size of the pile from what we had originally thought would be necessary; it also saved us from having to use the balloon bag to remove the nitrogen from the graphite.

Several of the other exponential pile experiments were not directly related to the building of CP-1, and they show that a large effort was already underway in planning the pilot and production reactors, on the assumption that CP-1 would succeed. In one exponential pile, we simulated the situation which would exist in a water-cooled reactor. The measurements were to provide data for Eugene Wigner, Gale Young, and the theoretical group who were studying systems for water cooling the production reactors. Another exponential pile was for studying the possibility of using liquid bismuth as a cooling agent, one of the possibilities on which Szilard wanted data. Years before, Szilard and Einstein had developed an electromagnetic pump for liquid metals. After the war, such pumps were used in prototype liquid metal cooling systems.

We had planned to move the material to a new laboratory at the Argonne Forest near Chicago. But when the construction of the building was halted due to labor problems, Fermi decided to proceed to build CP-1 in the squash court of the West Stands. On November 16, the balloon-cloth envelope was erected in the squash court and a circle was drawn to indicate where the first graphite bricks should be placed. My recollection is that Al Graves placed the first bricks. A reactor in the form of a sphere would lose fewer neutrons than one in the form of a cube. To make it spherical, a wooden understructure was built in which to place the graphite. In addition to everything else he was doing, Herb Anderson went around to the lumberyards to get the wood. Gus Knuth, the carpenter, cut the wood and built it into the shape that Fermi calculated would support the bottom of the pile.

I believe that we must have put on two layers per shift, one with the uranium oxide in the bricks and one without it. The day shift consisted of those of us in Zinn's group, and the night shift consisted of Herb Anderson's group, with some additional people. During the construction, Fermi had a meeting in his office every morning with Zinn and Anderson to plan each layer. We had different types of material, and for each type we had measured the reproductive properties in the exponential pile experiments.

The West Stands of Stagg Field at the University of Chicago, site of the first reactor.

The machining produced black graphite dust all over the place. We breathed it, slipped on it and it oozed out our pores.

There was a very great advantage in putting the material with the best reproductive factor in the center of the pile. I provided Zinn and Fermi with the inventory of all this material. For each layer, Fermi specified which types of material should be toward the outside of the layers and which types should be toward the inside. He used uranium metal instead of uranium oxide in the central area of the pile, making optimal use of the material we had.

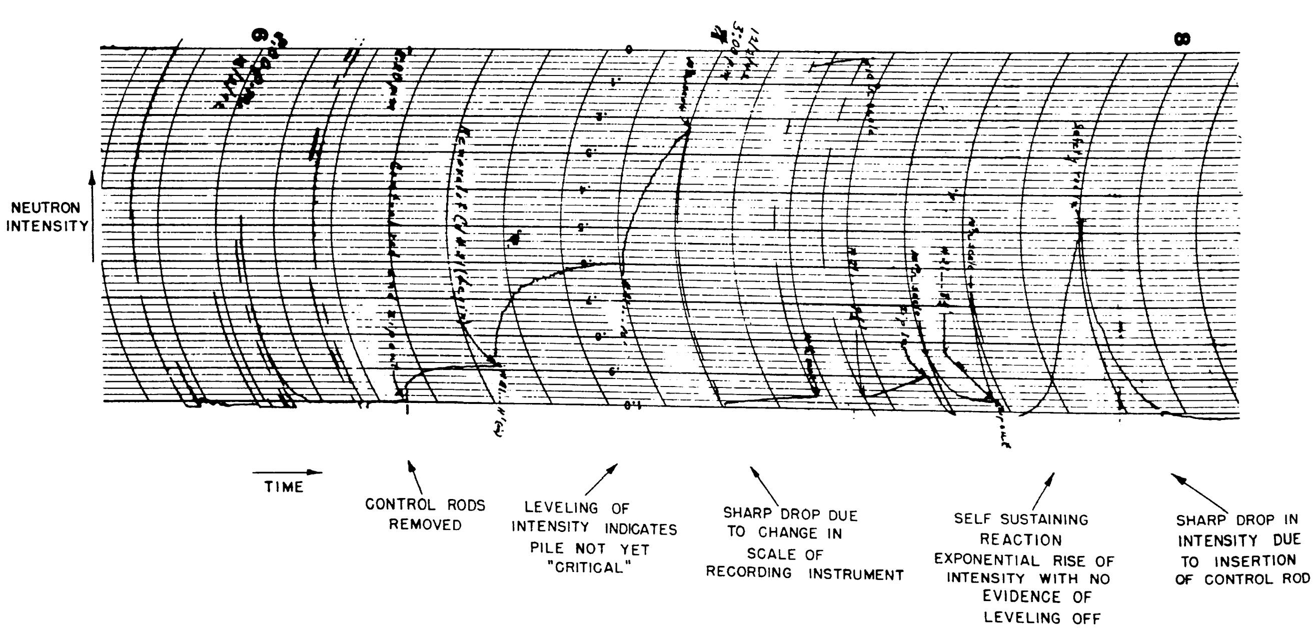
About eight weeks prior to this, Fermi had started a series of weekly lectures which continued until November 20. Two of the lectures described the measurements which can ascertain when a pile will be critical. Another covered the time-dependence to be observed. He showed that, approaching the critical point, the exponential rate of rise of neutron intensity becomes slower and slower. When the pile is subcritical, but approaching the critical point, the neutron intensity levels off at increasingly larger values; the intensity approaches infinity nearing the critical point. The approach to the critical point could be effected by adding layers to the pile, or, after the pile is constructed, by pulling out a cadmium rod which has prevented the pile from reacting. Those of us involved in running the factory and exponential measurements would stop for Fermi's evening lectures, which were a beautiful, simplified treatment of reactor theory. Even someone unsophisticated in mathematics could still understand what was known, what was happening, and what to expect.

The actual steps he followed on December 2 in taking the pile up to and then above criticality were based on the formulas given in these lectures.[3] They clearly demonstrate that there was good understanding of pile kinetics, despite implications to the contrary in some melodramatic press accounts of the events that took place on December 2, 1942. A few of those press accounts claimed that we didn't know whether we were going to blow up Chicago, and other such misleading information.

The measurements Anderson made every night used two types of neutron detectors, one a BF_3 counter built by Leona Woods, and the other the method of irradiating an indium foil overnight as closely as possible to the effective center of the partially built pile. Its activity was then measured the following morning on a Geiger counter, and results from the foil measurement were compared with the BF_3 readings. The neutrons multiply-

DECEMBER 2, 1942, Start-up of First Self-sustaining Chain Reaction

NEUTRON INTENSITY IN THE PILE AS RECORDED BY A GALVANOMETER



It was 11:30 a.m. and Fermi said 'I'm hungry. Let's go to lunch.' The other rods were put into the pile and locked.

ing in the structure were those that arose from the spontaneous fission of uranium-238. We now had sufficient uranium in the pile so that the natural neutrons were adequate in number, and we no longer needed to use a radium-beryllium source.

As a matter of precaution during construction, long before we were near the critical layer, some cadmium strips were inserted into some of the slots and were kept locked there. They were removed once every day with proper precautions to check the approach to the critical condition. I believe that we reached 52 layers November 30, and Fermi's extrapolations showed that the pile would be just critical when the fifty-sixth layer was added. Fermi decided to add the fifty-seventh layer so that it would be just one layer above critical. Anderson and his group added the fifty-seventh layer the night of December 1 with the agreement that they would not pull out the cadmium rods to see if we had been successful. He adhered to the agreement, even though it was very tempting not to.

We were all informed about the results of these measurements, so we knew on December 1 that Fermi would be withdrawing the control rods and making critical measurements on December 2. The construction of the reactor had been completed about a week earlier than Compton, the director of the project, had anticipated. The pile was somewhat smaller than the October estimates, partly because more uranium metal had arrived during November; therefore it was not a spherical structure but somewhat flattened at the top. Also we did not need to remove the nitrogen from the graphite, so the balloon did not need to be closed up.

The group under Volney Wilson, who had been working on instrumentation and controls for monitoring and controlling the first pile, was also alerted to the plans for December 2.[4] They had built redundant boron triflouride counters and ion chambers; the former are good for lower neutron intensities, the latter for higher neutron intensities. Most of the equipment used 110-volt power supplies; a few operated off batteries. Overbeck had built some automatic control rods that could be operated remotely with small motors. On December 2, these were kept out of the reactor.

We also had some cadmium rods which could be slid in and out of the pile by hand; one of these was used on December 2. The pile was at one end of the large squash court, and a balcony was at the other end. The slots in which the control rods and the safety rods slid were extended outside the pile on a framework that reached almost to the balcony or the wall below it. We had several different types of safety rods. One of them, called ZIP, which was designed by Zinn, operated by gravity. A solenoid held the rod out of the reactor, while the other end of the rod was tied to a rope which went over a pulley and was attached to a weight. The solenoid was attached to a relay operated from one of the detecting devices so that if the electricity failed or if the intensity became sufficiently high, the solenoid would release ZIP. Wilson's group had built a series of safety rods which could be activated remotely by pushing a button. An additional gravitational safety rod similar to ZIP was tied out with just a rope on the railing of the balcony where we stood. Norman Hilberry had an axe to cut that rope if human intervention were required to shut the pile down in a hurry.

December 2, 1942. On December 2, I arrived at about nine o'clock. I checked out some supplementary electronics and detection equipment that Wally Zinn and I had set up in a little tunnel on the west side of the squash court containing the pile.

Sam Allison had arranged for three large jugs of cadmium sulfate solution to be brought over and put on the elevator near the top of the pile. He thought that the jugs could be carried onto the pile and poured onto it in case of unforeseen disaster. Several of us were very upset with this since an accidental breakage of the jugs near the pile could have destroyed the usefulness of the material in the reactor. In fact, one cadmium rod pushed into the center would have been equally effective. Because he was such a nice guy, however, and a big shot, we did as he asked. I do not remember which three people were on the elevator in the morning with the jugs of cadmium sulfate. The rest of us gathered on the narrow balcony at the other end of the squash court. At one end of the balcony were the electronics, scalers, and a pen recorder attached to detectors on the pile. Some of the younger physicists would read and plot the readings from the scalers; Fermi could look at their data or at the pen recorder; they were independent measurements of the neutron intensity.

On December 2, we began by checking that the neutron intensity was the same as Herb Anderson had measured the previous night, when all except one of the cadmium rods in that pile had been removed. The rates on some of the other instruments were checked and some adjustments were made in anticipation of the neutron intensity's increasing as we proceeded in the morning.

Fermi planned to use the last cadmium rod in the pile as a control rod. It would be set by hand at various positions so that we could measure neutron intensity for those positions. He had calculated in advance the intensity that he expected the pile to reach when it saturated at each of these various positions. George Weil was in the squash court in a position to be able to move the last rod. After

The pile was functioning exactly as expected. Fermi broke into a big, cheerful smile. He put away his slide rule and announced, 'The reaction is self-sustaining.'

the checks on the instrumentation were completed, Fermi instructed Weil to move the cadmium rod to a position which was about half-way out. It was well below the critical condition. The intensity rose, the scalers increased their rates of clicking for a short while, and then the rate became steady, as it was supposed to.

While it was rising, Fermi periodically read some numbers and did a quick calculation on his little slide rule of the exponential rate of rise of the neutron intensity in the pile. After the intensity had leveled off, he then told Weil to move the cadmium rod another six inches. The neutron intensity in the pile rose further and then again leveled off. The pile was still subcritical. Fermi had been busy noting the values on the back of his slide rule and calculating the rate of rise. After it had stabilized, Fermi told Weil to move the rod out another six inches. Again the neutron intensity increased and leveled off. The pile was still subcritical. Fermi had again been busy with his little slide rule and seemed very pleased with the results of his calculations. Every time the intensity leveled off, it was at the values he had anticipated for that position of the control rod. He moved the rod out another six inches. After it had stabilized this time, the neutron intensity in the pile had reached an intensity that was too high for some of the instruments, and, as in other experiments, a few of the instruments were no longer in their linear range. We wanted to take some time to rectify the situation and to modify the operating range of some of the instruments.

After the instrumentation was reset, Fermi told Weil to remove the rod another six inches. The pile was still subcritical. The intensity was increasing slowly—when suddenly there was a very loud crash! The safety rod, ZIP, had been automatically released. Its relay had been activated by an ionization chamber because the intensity had exceeded the arbitrary level at which it had been set. It was 11:30 am, and Fermi said, "I'm hungry. Let's go to lunch." The other rods were put into the pile and locked.

I ate at the cafeteria at the University Commons. After all these months, we had become adjusted to not discussing our work outside the laboratory, so there could be no discussion over lunch of the morning's events.

It is important to understand what Fermi was doing in the morning by making these measurements. The most important thing was to establish the position of the control rod at which the pile would become self-sustaining, that is, the critical point. The next thing was to establish how fast the intensity would rise if he moved the rod beyond that point. He could establish the critical point by two methods: to extrapolate from the intensity measurements; or to note that the exponential rate of rise had become zero, indicating the critical condition. The rate of rise would become longer and longer approach-

Norman Hilberry (left) and Leo Szilard at West Stands.

Eugene Wigner took out a bottle of Chianti and presented it to Fermi. We each had a small amount in a paper cup.

ing critical, then after passing through the critical it would start to get shorter again. Fermi was determining that both these methods were giving the same result. There had been some uncertainty in the value of one of the constants used in the formulas for the exponential rate of rise. From the first couple of measurements, he obtained the value of the constant, which was what he had hoped it would be. Continuing measurements had been to establish that he could predict precisely what would happen when he moved the control rod a fixed distance. That is, he could predict both the rate at which the intensity would increase with time and the intensity at which the pile would stabilize. Being able to make reliable predictions indicates not only a quantitative understanding of the physics, but also the reliability of the detectors and instrumentation.

So, in the morning Fermi had established that he had control, that he could predict the reaction of the pile, and that the instrumentation was reliable at the intensities at which he needed to make measurements. When we returned after lunch, Compton, who had not been there in the morning, joined us, bringing Crawford Greenwalt of the DuPont company with him. Compton had been meeting with an important committee that had just stopped over in Chicago for a few hours. Volney Wilson had phoned him in the morning to say that there was very little room on the balcony.

In the afternoon there were changes in what some of the younger people were doing. Herb Anderson, Bill Sturm, and Leona Woods were recording the readings from instruments. Somehow we got a public address system and Bill Overbeck was set up to call out the neutron counts. Fermi was set up to watch the pen on the recording chart which was attached to a neutron detector. I took my turn on the elevator.

Except for the one hand-controlled rod, all the other rods were again removed. Fermi asked for the last hand-controlled rod to be set at one of the positions where it had been in the morning. He checked the intensity and the rate of rise and the functioning of the instruments. The values were the same as they had been during the morning when the control rod was at that same position. He then asked George Weil to set the rod where it had been before we went to lunch.

The trace on the paper on which the neutron intensity was being recorded showed the intensity rising slowly, at the rate that Fermi expected. The intensity would have levelled off after an appreciable length of time. The pile was getting close to critical. Fermi measured the changes in the rate of rise for a while, then asked that ZIP be put in to bring down the intensity. He told George Weil, "This time, take the control rod out twelve inches." After the control rod was set, the ZIP rod was removed from the pile, and Fermi said to Compton, who was standing at his side, "This is going to do it. Now it will become self-sustaining. The trace will climb and continue to climb; it will not level off." Fermi computed the rate of rise after a minute. After another minute, he computed it again. After three minutes, he calculated the rate of rise again, and it was staying the same. The pile was functioning exactly as he had expected. I have heard that at this point he broke into a big, cheerful smile. He put away his slide rule and announced, "The reaction is self-sustaining."

Fermi let the activity of the pile increase and watched the pen. It continued to rise as it should, and the intensity was not leveling off. At 3:53, Fermi told Zinn to put ZIP in. The radiation and the neutron intensity and the counting rates all decreased almost instantaneously. We had built the pile, and Fermi had established that we could get a self-sustaining nuclear reaction that we could control in a very predictable manner.

Eugene Wigner had a paper bag with him that I had not noticed. He took out a bottle of Chianti and presented it to Fermi. We each had a small amount in a paper cup and drank silently, looking at Fermi. Someone told Fermi to sign the wrapping on the bottle. After he did so, he passed it around, and we all signed it, except Wigner.

People turned off the electric power on the instruments and slowly left. I think Bob Nobles and I got the cadmium sulfate bottles as far away from the pile as we could.

After all the others had left, I stood there just looking at the pile. My mind was wandering over all the machining, pressing, stacking that the gang of us had done. I recalled some of the minor incidents that could have turned into major delays or disasters. I had a tremendous feeling of accomplishment.

Then my mind wandered in the wrong direction—I started thinking about the work that lay ahead. So I went around and checked that all of the rods were locked in place, that all the power supplies were turned off. I hung up the Chianti bottle on the wall and threw away the cups. I then went home to my room to sleep for twelve hours. ☐

1. Herbert L. Anderson, "Assisting Fermi," in *All in Our Time* (Chicago: Educational Foundation for Nuclear Science, 1974).

2. As well as Anderson and Zinn, those involved were: H. Agnew, A.C. Graves, D.L. Hill, P.G. Koontz, H. Kubitschek, H. Lichtenberger, G. Miller, R. Nobles, W. Nyer, L. Sayvetz, L. Seven, W. Sturm, A. Wattenberg, and G. Weil.

3. Enrico Fermi, *Collected Papers*, 2 vol., Emilio Segre, ed. (Chicago: University of Chicago Press, 1965).

4. Those involved were: H.M. Barton, Jr., T. Brill, R. J. Fox, S. Fox, D. Froman, W. H. Hinch, W.P. Overbeck, J.H. Parsons, G.S. Pawlicki, L. Slotin, R.J. Watts, M.H. Wilkening, and V.C. Wilson.

THE POLITICS OF CONTROL - THE ROLE OF CHICAGO SCIENTISTS

Alice Kimball Smith

My remarks today will be an exercise in nostalgia for those of you in the audience from whom I learned much of what I shall say. For others I hope they may provide perspective on questions of immediate concern.

Scientists at Oak Ridge, where huge plants produced fissionable materials, and at Los Alamos, where the bombs were designed and built, certainly foresaw the revolutionary impact of the release of atomic energy. But only here at the Manhattan Project's Metallurgical Laboratory was much time spent in planning for the future and a concerted effort made to bring about Japan's surrender by a non-military demonstration.

Why was Chicago different? After the breakthrough of December 2, projects and staff gradually moved to other laboratories. Met Lab people agreed that thereafter they worked under less pressure and had more time to think and talk. But the real answer lies in people. In the mid-1930s Leo Szilard, working in Berlin, then in England, was convinced that atomic energy would soon be available and drew up plans for a foundation to control its use. He was in the United States when news of uranium fission arrived in January 1939 and commenced experiments at Columbia University leading to a chain reaction. Appalled at the lack of official interest in a weapon toward which Germany might well have a headstart, Szilard and Eugene Wigner persuaded Einstein to write the letter to Roosevelt that launched the bomb project. In 1942 when chain reaction work was added to the research already being done in Chicago under Arthur H. Compton's direction, Szilard too joined the Met Lab staff. He once identified his favorite hobby as baiting brass hats and complained constantly about the rigid compartmentalization of information that General Leslie R. Groves imposed when he took over the Manhattan Project in September 1942. Nor was Szilard always an easy scientific colleague, for he spawned ideas faster than more conventional minds could absorb them, but his prodding to think about the future had enormous effect upon some young Met Lab associates.

Others responded more readily to the quiet stimulus of James Franck. In 1933 when Hitler dismissed Jews from university posts Franck, though part Jewish, was allowed to retain his professorship of physics at Gottingen because of his eminence and a Nobel prize. However, he resigned in protest. He joined the Chicago faculty in 1938. Franck later told me that Niels Bohr was indirectly responsible both for his resignation from Gottingen and for his role in Met Lab discussions. After World War I the two men had become fast friends, but Bohr was critical of Franck for having ignored the rise of militarism under the Kaiser and then joined the army. The individual, Bohr said, is responsible for what society does. Because he was someone who had practiced what he preached, Franck's influence in getting his young friends to think about the results of their work was very great.

And the young made eager disciples. In the summer of 1943 when word reached Chicago about the scale of industrial installations at Oak Ridge a group of them drew up a manifesto warning against postwar commitment to private enterprise. Security officers confiscated the document and banned their meetings with mutterings about transfer to Guadalcanal. But it was not so much this prospect, promptly discounted by Compton, that halted overt action as an appeal from Met Lab director Samuel K. Allison, who said that placating the authorities was taking too much of his time. Allison had taken charge of the Met Lab earlier in 1943 when Compton re-organized his far flung research command into the Metallurgical Project and appointed directors of its several components.

By the spring of 1944 senior Met Lab scientists were also becoming restive, disturbed by rumors that the laboratory might close. This was shocking news to the physicians and biologists in the busy health care division that Compton had established as well as to those engaged in basic atomic research applicable to medicine, agriculture, and power. To allay this concern and to back up his own case for Met Lab continuance, Compton appointed a committee chaired by industrial metallurgist Zay Jeffries to explore peacetime applications. Topics were assigned to subcommittees of group leaders at the Met Lab and Oak Ridge, and the findings were embodied in the "Prospectus for Nucleonics," known as the Jeffries report. Compton sent it to Washington on November 18, 1944.

The "Prospectus" did not become a chart for the atomic age. There were as yet too many imponderables. But two sections added to the original outline by Eugene Rabinowitch and the committee's secretary, Robert S. Mulliken, were an early draft of the more famous Franck report of the following June. Work on the bomb must continue lest our strong hand be covered by a stronger, but in the long run we had two alternatives — to stop work on nucleonics and lose all potential benefits or to alert the public and mobilize support for an international agreement to control atomic weapons. Until a control authority existed only fear of retaliation would deter a nuclear attack — an ominous foreshadowing of a now familiar doctrine. It was essential therefore to enlist the cooperation of our allies, especially Russia, at an early stage.

The compartmentalization of information that prevented Met Lab ferment from spreading to other Manhattan Project sites also interfered with vertical communication. When Compton forwarded successive exhortations to Washington he was authorized to return only vague assurances that adequate planning was under way. He could not say that for the past six months the scientific administrators of the Project, Vannevar Bush and James Conant, had been deciding what to say about international control when the time seemed ripe to discuss it with Secretary of War Stimson and the president. Six weeks before Compton turned in the Jeffries report they had sent Stimson a long memorandum to use in talking with Roosevelt proposing an international control agency with supporting arguments exactly like those in the report's final chapters. Stimson became a firm supporter of international control and before he retired in September 1945 he had helped to make it a plank in U.S. atomic policy.

The Bush-Conant memorandum suggested demonstrating the bomb in an uninhabited area in the hope of inducing Japan's surrender though they abandoned this idea as technical uncertainties in bomb design persisted. President Roosevelt received a similar poposal in November 1944 — a rehearsal demonstration before internationally recognized scientists and religious leaders — from economist Alexander Sachs whose help Szilard had enlisted to deliver Einstein's letter in 1939. Did Szilard have a hand in Sachs' proposal? His published documents contain no evidence that he did, but a non-military demonstration was widely discussed at the Met Lab in the early weeks of 1945. By this time it was clear that the bomb would not be needed to defeat Germany. But what would be the effect upon the postwar international climate of using it not as a defensive weapon against Germany but as an offensive one against Japan? The realization that most of the statesmen meeting in April to plan the United Nations Organization did not know about the bomb was deeply disturbing to the Chicago scientists who had contributed to the final chapters of the Jeffries report. So while young people at the Met Lab held seminars to discuss alternatives to military use and details of a control plan, Szilard and Franck both took independent action.

Szilard prepared a memorandum for President Roosevelt and had obtained an appointment for May 2, but the president died on April 12. Truman was too busy to see Szilard and referred him to his personal adviser James F. Byrnes, after July his secretary of state. For all Szilard's self-confidence in intellectual matters, he was aware that his manner was not always ingratiating or his accented English an advantage in talking to American officials. So he persuaded Harold Urey and Chicago mathematician Walter Bartky to accompany him to Byrnes' home in Spartanburg, South Carolina on May 28. Byrnes knew about the bomb, but his three visitors were shocked by his apparent failure to understand its significance. Byrnes for his part was antagonized by Szilard's presumption in suggesting that he and other scientists talk to the Cabinet. Szilard later recalled how depressed he was as he walked to the railroad station with Bartky and Urey. "I thought to myself how much better off the world might be had I been born in America and become influential in American politics, and had Byrnes been born in Hungary and studied physics. In all probability there would then have been no atomic bomb and no danger of an arms race between America and Russia."

Years later James Franck told me of the background of his characteristically lower keyed undertaking. In 1942, when Compton asked him to head the chemistry division at the Met Lab, Franck consented on one condition, that when the time came to use the bomb, if no other country had developed it, he might present his views on its use to someone at the highest level of decision making. "I didn't always agree with Compton," Franck said, "but he was an honest man and a gentleman." In early April 1945 Compton said it was time to arrange a meeting, and Franck prepared a memorandum. Franck's spoken English was very effective, but he never wrote it with ease and turned for help to his friend Rabinowitch who, as a Russian refugee student in Germany in the 1920s had done writing and editing on the side. Whoever held the pen, the words had an authentic Franckian ring: Scientists had willingly obeyed rules about secrecy so long as the inconvenience was personal, but these regulations become intolerable when they conflict with our consciences as citizens and human beings. "How is it possible," Franck asked, "that the statesmen are not informed that the aspect of the world and its future is entirely changed..., and how is it possible that the men who know these facts are prevented from informing the statesmen about the situation?" Franck did not comment on the use of the bomb. On or about April 21 Compton accompanied him to Washington with the memorandum. It was a busy time for those at the highest level of decision making with whom Franck hoped to talk. Truman's sudden elevation had given him much to learn, including the very existence of the Manhattan Project. Secretary of War Stimson was preparing to give Truman his first full briefing on the bomb, and Bush was briefing Stimson. Only Henry A. Wallace, at this point the least influential member of the Cabinet, was available to receive Franck and his statement. Actually the memorandum that Stimson used for his talk with Truman said much the same things. Had Chicago scientists known this, they could no longer have complained about Washington's failure to think ahead. But Franck, as respected and trusted as anyone on the Project, returned to Chicago without this assurance.

Early in May Stimson appointed a War Department Interim Committee to decide what should be said after a bomb was dropped, to draft domestic legislation, and make recommendations regarding international control. The departments of state, war and navy were represented, as was the Office of Scientific Research and Development by Bush, Conant, and Karl T. Compton, president of MIT on leave. On the advice of Bush and Conant, Stimson appointed an advisory panel of four Project scientists: Arthur Compton, Enrico Fermi, Ernest Lawrence and Robert Oppenheimer, and by May 31 the committee was ready to consult them. Use of the bomb was not on the agenda, but at lunch Arthur Compton mentioned the strong Met Lab interest in a demonstration. In the afternoon the possibility was raised, and dismissed, as part of a discussion of the effect of bombing on the Japanese people and their will to fight. The panel was not present the next day when the Interim Committee voted unanimously that a bomb should be used against Japan as soon as possible, without prior warning, on a combined military-civilian target selected by the Secretary of War.

In response to a question from Compton panel members were authorized to tell their colleagues about the Interim Committee and its function. Back in Chicago Compton immediately called a meeting of Met Lab leaders to convey this information. The panel would meet again soon, and he would take to it any proposals that were ready by June 14. Some of those present were interested in research, production, controls, the organization of an atomic authority, and public education. Before the meeting broke up committees were appointed to explore these five topics. In due course their conclusions were combined by Norman Hilberry into a single report to the Interim Committee. To deal with the more pressing issue of a demonstration Compton also appointed a committee on social and political implications with James Franck as chairman. The other members were Donald Hughes, J.J. Nickson, Eugene Rabinowitch, Glenn Seaborg, Joyce Stearns, and Leo Szilard. Drafting and re-drafting. Rabinowitch achieved "A Report to the Secretary of War" that all members were willing to sign. To earlier Chicago statements about the impact of the bomb he added a plea for a demonstration which said in part: "If the United States were to be the first to release this new means of indiscriminate destruction upon mankind she would sacrifice public support throughout the world, precipitate the race for armaments, and prejudice the possibility of reaching an international agreement on the future control of such weapons." If a demonstration in an uninhabited area did not lead to surrender, Japan could be given an ultimatum to evacuate certain regions as an alternative to their total destruction. On June 11 Franck, again with Compton's help, left for Washington and deposited the report next day with Stimson's assistant, George Harrison. At Harrison's request, panel members discussed the proposed demonstration when they met at Los Alamos on June 15 and concluded unanimously that they could propose no demonstration likely to end the war.

The Franck report offered only political arguments for a demonstration. We were all deeply moved by moral considerations, Rabinowitch later explained, but we knew that in the necessarily a-moral climate in which wartime decisions are made the moral argument would not be effective. Szilard wanted the record to show scientists' concern with the moral issue. Early in July he began circulating drafts of a petitiion to President Truman. The final version, dated July 17, did not mention a demonstration, but it urged that atomic bombs not be used unless Japan, fully understanding the terms to be imposed, still refused to surrender. The petition added: "Such a step ought not to be made at any time without seriously considering the moral responsibilities involved." The seventy signers included all levels of the Met Lab staff and some very distinguished names.

Compton was unhappy about petition signing as a measure of opinion. So he asked Farrington Daniels, the new Met Lab director, to take a confidential poll. Five alternatives were offered in writing to the 150 staff members at work on July 12. They ranged from all out military use of the bomb to none at all in the current war. Eighty-three per cent of the respondents chose one of the other three alternatives which offered some form of demonstration. The one that received 42% of the total vote was so ambiguously phrased that some participants understood it to mean using the bomb as requested in the petition that Szilard was circulating. But that was not the way Compton interpreted the poll in 1956 when he published Atomic Quest, his personal account of the Manhattan Project, in which it was described as supporting the bombing of Japan.

That account is responsible for my presence here today, a connection I shall amplify less as autobiography than as historiography. Rabinowitch, by this time editor of

The Bulletin of the Atomic Scientists, was disturbed by some of Compton's statements relating to the decision to use the bomb, especially those about the poll. One day he said to me, "Why don't you find out what Met Lab people tried to do about the bomb and write an article for the Bulletin? You're a historian; you know some of those people." I didn't think much of these qualifications. I had been trained to believe that a respectable historian specialized, and what I knew most about was clerical incomes in seventeenth century England. As for friends, what did they have to do with research? But Rabinowitch was a hard man to refuse when he wanted help, so I went to work. There were as yet no general histories of the war and postwar periods from which to start. Journalists' accounts varied greatly in quality, and most documents were classified or, like the version of the Franck report which the Bulletin was allowed to publish in 1946, liberally censored. So, quite unwittingly, I became something of a pioneer in oral history, asking questions, getting a lot of interesting information that spilled over into the postwar period, linking up with aspects of the scientists' movement that I had but vaguely understood during two years as assistant editor of the Bulletin, especially with some of Szilard's conspiratorial maneuvers conducted via the office telephone. I decided to carry on the Met Lab Story. Robert Rosenthal, head of the University Library's Special Collections, had had the foresight to house back files of the Bulletin and the Atomic Scientists of Chicago. As word of my interest got around other files arrived from Los Alamos, Cambridge, and Oak Ridge. Several cartons were deposited in our Kenwood Avenue living room one weekend with a message of gratitude from an Oak Ridge wife whose closet shelves had at last been liberated.

To return to July 1945 — No replies came to the pleas for a demonstration. On July 6, Truman, Stimson, and other officials sailed for Europe to attend the conference with Churchill and Stalin that opened in Potsdam on July 15. In Chicago a new committee on social and political implications — Arthur Jaffey, Robert Maurer, J.J. Nickson, and John Simpson — circumvented the continuing ban on meetings by interviewing their colleagues individually while security turned a deaf ear to the talk in an anteroom among those waiting to be interviewed. Of memoranda that survive from these hearings by far the most significant was submitted by Eugene Rabinowitch on July 12. Secrecy should be relaxed to permit discussion with policy makers and to inform the American people about the problems. Project scientists who agreed on basic questions should form an organization which could be extended to scientists at large as soon as the existence of the bomb was revealed. This action committee should work for international control of nuclear power and for research in nucleonics for the common good of mankind. Here in essence was the charter of the postwar scientists' movement though the memorandum itself disappeared into classified files. On August 6 Hiroshima was bombed. On August 12 Atomic Energy for Military Purposes by Henry de Wolfe Smyth was published.

Surprise at the prompt release of so much information was matched by outrage when Project employees were told not to make public statements until the president announced official policy. However, it was not one of the Young Turks who first challenged this ban but Samuel K. Allison, since mid-1944 an associate director at Los Alamos. On September 1, the University invited the local press to lunch at the Shoreland Hotel to hear about its three new scientific research institutes. As director of the Institute for Nuclear Studies, Allison was the principal speaker. Some of you will remember how deceptive was his outwardly placid manner and how he could rouse himself from apparent somnolence at meeting or party to say something incisive or funny. What quickly entered Chicago annals as Sam's butterfly speech was reported on the front page of next day's Chicago Tribune with the headline "Scientist drops A-Bomb; Blasts Army Shackles." Allison was quoted as saying that if exchange of scientific information was prohibited scientists would abandon nuclear research and turn to the study of butterfly wings. He went on to speak of the tragedy of the second bomb on Nagasaki, the first public expression by a Project scientist, I believe, of what was to be a common reaction.

Calls from Washington summoned Allison, Urey, Fermi, and Thorfin Hogness to another Shoreland lunch next day with Groves' assistant, Colonel Kenneth D. Nichols who, officially at least, was not amused by Allison's explanation that lunch the day before was late and sheer hunger had made him fractious. Nichols said that a domestic atomic energy bill would soon be ready and irresponsible talk might jeopardize its passage. Allison was astonished to learn that his spontaneous outburst was regarded as the opening gun in a scientists' campaign though such indeed it proved to be, for mimeographed statements were already circulating freely between Chicago, Los Alamos, and Oak Ridge. Smaller Project centers soon joined the communications network to form the Federation of Atomic Scientists.

On October 3 Truman sent a highly reassuring message to the Congress, describing atomic energy as "a force too revolutionary to be considered within the framework of old ideas," He spoke of eventual renunciation of military use and of research directed to humanitarian ends. But next day there was introduced into both houses of Congress the May-Johnson bill which left a loophole for military membership on a domestic commission and contained drastic security provisions. With great pleasure I leave further comment on this bill, and on the one that eventually took its place, to the next speaker and mention here only its energizing effect upon the embryonic scientists' movement. Predictably it was Leo Szilard and Harold Urey who explained to reporters that the bill would endanger national security by stifling research and would prevent the exchange of data essential to any international control scheme. And it was at the instigation of Szilard, ensconced at the Wardman Park Hotel, that young men from Project labs converged upon Washington, told their stories to senators, congressmen, and legislative aides, and testified about atomic energy at committee hearings officially scheduled to deal with bills to set up a national science foundation.

Raymond Swing's broadcast to his national audience on Friday, October 19, was ecstatic about what he called science week in the capitol. The young scientists, he said, were as impressive a group as ever came to modern Washington. "Their faces are open and clear, their eyes look steadily, and as witnesses before committees and in newspaper conferences they were quiet, modest, lucid, and impellingly convincing." Subsequent events, including the organization of the Federation of Atomic Scientists and the chaos in its tiny office from which rotating site representatives lobbied and lectured were chronicled in a November issue of Newsweek with the title "The Reluctant Lobby." Scientists at non-Project labs, especially those working on radar in Cambridge, also reacted so strongly to the security provisions of the May-Johnson bill that the broader organization suggested by Rabinowitch coalesced without need for recruitment as the Federation of American Scientists. Delegates to its first meeting in mid-November met with representatives of some fifty national organizations — religious groups, labor unions, women's clubs — with a total membership of around ten million to form the National Committee on Atomic Information which distributed vast quantities of literature about international control and related domestic issues over the next two years.

Prior to the first United Nations General Assembly meeting in January 1946, Britain, Russia, and the United States agreed to sponsor a UN Atomic Energy Commission. Preparation of a U.S. proposal was entrusted to a committee chaired by Under Secretary of State Dean Acheson. In April its board of consultants, headed by TVA chairman David Lilienthal, produced a document known as the Acheson-Lilienthal report. With a few modifications, this was submitted to the UN Atomic Energy Commission in June 1946 as the U.S. plan and accepted as the basis of negotiations. Three months later a subcommittee of Soviet and Western scientists reported that "we do not find any basis in the available facts for supposing that effective control is not technically feasible. Whether or not it is politically feasible is not discussed in this report." The answer was no. Negotiations remained deadlocked. In 1951 they merged with those of the UN Disarmament Commission and quietly died.

Against this very sketchy background I again concentrate on developments in Chicago, though with a sense of injustice to their colleagues elsewhere. Young scientists at Oak Ridge promptly wrote to legislators and called upon editors, commentators, and congressman in New York and Washington. The Clinton Labs group leaked its statement of purpose to the New York Herald Tribune ten days ahead of the president's message. It was fully represented in Newsweek's "Reluctant Lobby" and in the on-going affairs of the Federation.

At Los Alamos Oppenheimer no longer discouraged talk of the impact of the bomb, as he had felt compelled to do until it was completed. In fact, he became the principal disseminator of the comforting message distilled from his wartime talks with Niels Bohr that war was now impossible and that in an inevitable revolution in international relations the world community of science would have an important part to play. In the mountains of New Mexico, as in the hills of Tennessee, audiences with whom to share this message were few and far between, but postwar migration, especially rapid from Los Alamos, added manpower to other centers of activity and helped to keep international control at the top of the agenda. And I doubt that the national federation would have survived without the dedicated services of William Higinbotham of Los Alamos and Joseph Rush of Oak Ridge in the Washington office.

Yet Chicago was the principal nerve center of the postwar movement, thanks in part to familiarity with the isuues that had occupied Franck, Szilard, and their disciples. And there were the disciples, suddenly transformed into leaders, taking the initiative in political action and risking the public exposure that many older scientists tended to avoid, partly from early conditioning but also because they were responsible for resumption of normal teaching and research. As for manpower, Chicago lost very little after the war. Met Lab people who moved to the new Argonne Laboratory could still take part in the local organization while the new research institutes brought young activists from Oak Ridge as well as more senior ones, including Maria Mayer and Harold Urey from New York and Edward Teller from Los Alamos.

Vitally important also was the support of the University. From special funds Chancellor Hutchins produced $10,000 for local activities and to launch a national office, making the Atomic Scientists of Chicago the rich relation. On September 19, 1945 he convened a conference on atomic energy, the first of its kind, at which distinguished economists, political scientists, government officials and public relations experts heard Szilard talk about the international impact of the bomb and Franck about the evils of continued secrecy. Six weeks after Hiroshima the guests had some trenchant thoughts of their own to contribute, and contacts were made that enhanced Chicago influence on international and domestic policy. Political scientists Quincy Wright and Hans Morgenthau willingly assisted the scientists' efforts at self-education, a cardinal point in Rabinowitch's original platform. So did Robert Redfield and Edward Shils who organized the Office of Enquiry into the Social Aspects of Atomic Energy which undertook research on such topics as the cost of atomic power and dispersal of urban populations. And I trust that Edward Levi's reminiscences will do justice to the important links with the Chicago law faculty.

The atomic Scientists of Chicago began to organize the day after Hiroshima. Rabinowitch chaired a committee to summarize collective thinking. Szilard's contribution, which included a section on birth control, was rejected in favor of Rabinowitch's more sharply focused draft. A temporary executive committee, typical in make-up of those that followed, included four signers of the Franck report: Nickson, Rabinowitch, Seaborg, and Szilard, Simpson who had chaired the successor committee, Austin Brues of the health division, and physicist David Hill. The theme of the ASC's first press release the day after the President's message to Congress was that a new dimension of power demanded some truly radical countermeasure and the only one with a chance of success was international control. If this thesis seems tiresomely familiar at this point in my talk, remember that it was new to readers of Chicago newspapers.

The release also noted that not all scientists had reservations about the way the bomb had been used or felt responsible for future developments, but it claimed that the new organization had the support of 95% per cent of those working on the Project at Chicago. Following cautious revision a week later to "over 90%", John Simpson commented that the 5-10% per cent included people we respected and that bothered us. One of those he had in mind was Enrico Fermi, and it is appropriate on this occasion to say something about Fermi's attitude. Unable to attend the September conference, he had written to Hutchins that the U.S. must remain militarily strong but "the possiblility of an honest international agreement should be explored energetically and hopefully." His wife later explained that experience under dictatorship convinced him that international control was impossible so long as Russia remained a closed country, a view that many of its proponents, including Robert Oppenheimer, rather quickly came to share. But one of my vivid Chicago memories is of evenings at the Fermi's, or in other Hyde Park Living rooms, when Fermi sat, often on the floor, surrounded by young people discussing politics including those of the bomb. I do not remember ever hearing him belittle views that differed from his own or squelch those who expressed them.

Chicago's first press release was but the tip of the iceberg of minutes, outlines, and correspondence left by committees and subcommittees over the next two years of intensive, then declining, activity. They are not lively reading; yet collectively they convey the excitement and urgency with which the educational campaign was launched. An article in the October 29 issue of <u>Life</u> entitled "The Atomic Scientists Speak up" was signed by Hill, Rabinowitch, and Simpson. No need to tell you what it said, only that it employed the slogans of no secret, no monopoly, no defense, therefore international control, that would become the staple of speeches and articles for months to come. <u>The Atomic Bomb</u>, a primer of technical information directed to members of Congress, was assembled by Leonard Katzin's editorial group. Katherine Way and Gale Young solicited articles by leading scientists and other public figurs for <u>One World Or None</u> published by McGraw Hill. Nearly 100,000 copies were sold.

The warning that a revival of prewar Midwest isolationism could fuel opposition to international control gave added impetus to the work of a speakers' bureau headed by Alex Langsdorf. Big names were assigned to big audiences like the one that came to an Orchestra Hall rally. Others had a choice of suburban service club luncheons, pastors' institutes, and high school science clubs from Fond du Lac to Flint. Langsdorf filled an average of one request a day during the autumn and winter of 1945-46, and his files show how successfully some speakers transferred attention-holding skills from classroom to platform. "Dr. Teller did a magnificent job," wrote the pastor of a large church in suburban Cleveland. "With superb skill he put us through a course in nuclear physics and then with a force of conviction that is rare indeed told us what needs to be done about it." Less eloquent speakers projected the image of someone wrenched from the laboratory by the exigencies of the moment and did the cause no harm.

Chicago also had easy access to radio audiences and to broadcasters. ASC members talked to national audiences on the University of Chicago Round Table programs. But what counted more, in the long run, was the slant regularly given to atomic energy news by commentators and columnists. One of the most influential Midwest radio newsmen of the period was Clifton Utley, former poliical science instructor at the University who publicized the scientists' views and gave sound advice on politics and public relations.

Szilard maintained that the whole educational campaign was not worth a few well-placed contacts in Washington. Rabinowitch argued that only the long slow process of education could produce the changed climate of opinion needed for a significant international agreement (though he surely hoped for a time scale of less than forty years). The campaign for the McMahon bill would demonstrate the merit of both approaches and also of a middle course adopted with University backing — a series of conferences to which clergymen, then labor leaders, business executives, and radio commentators were invited. The exchange of wisdom did not always live up to expectations, but continuing committees of these groups sponsored meetings in other towns and cities and were responsible for the dispatch of hundreds of telegrams in support of the McMahon bill at critical points.

Individuals made important contributions. Arthur Jaffey and John Simpson noted that congressmen and consultants on international control kept asking for evidence that inspection of atomic installations was technically feasible. Some answers required access to classified data, so Jaffey and Simpson organized a series of feasibilty studies, assigning topics to specialists at Project laboratories and collecting the reports. These studies lost their identity, though not their usefulness, as part of the technical information supplied to the framers of the U.S. international control plan. When a Senate Special Committee opened new hearings on domestic atomic energy legislation in late November, Simpson served as the national federation's liaison, preparing testimony and telling local groups when and where to apply pressure.

For many ASC members evenings and weekends, once spent in the laboratory, were now taken up with meetings and speech writing. Yet as I talked to participants years later it was not so much the changed pattern of time they remembered as the upheaval in accustomed attitudes and the mounting sense of urgency. But time was an element in the tapering off of activity, as Simpson pointed out in the November 1946 issue of Chicago Magazine: "The constant stream of interruptions day and night, phone calls, speeches and strange decisions," he wrote, "have made it impossible to think about science for any extended periods.... At the University of Chicago alone over twenty four hundred hours per month have been devoted by scientists ... to education and political action.... For many of us it has meant the postponement or complete loss of a year of valuable research time out of the productive part of our lives." Another factor in the gradual reduction of political activity was that of diminishing returns as the stalemate in international control negotiations developed.

Yet Simpson and some of his young colleagues continued to support the one offshoot of Met Lab political and social concern that has yielded neither to time constraints nor to apparent failure, The Bulletin of the Atomic Scientists. Rabinowitch liked to say that The Bulletin was born in the Stineway Drug Store at 57th and Kenwood where he, with physicist H.H. Goldsmith and sociologist Edward Shils, drank coffee and talked about the need for a publication that would document the new age and explore its problems. The first six-page issue of The Bulletin of the Atomic Scientists appeared on December 10, 1945, with Rabinowitch and Goldsmith as editors.

When Goldsmith died in a swimming accident in 1949, Rabinowitch lost the one associate whose dedication was as singleminded as his own though invaluable help came from Jaffey, Langsdorf, Simpson, and, as they moved from Oak Ridge, Harrison Brown, Harrison Davies, and Clyde Hutchison. The Bulletin's rapid transformation from local newsletter to the prime source of key documents and of scholarly discussion relating to science and public affairs prompted an early takeover attempt by the national federation which the editors successfully resisted though they did agree to drop "of Chicago" from the title. A generous offer of sponsorship and financial help from the University of Chicago was also turned down. Neither Rabinowitch in his editorials nor The Bulletin's unpaid authors made the stylistic concessions that attract a popular audience, but from the beginning reporters and commentators relied upon it for facts and for insight into how scientists were adjusting to their new prestige and responsibilities. Early issues now serve the same purpose for historians.

In the margin of my rough notes for this talk is frequently scribbled the words "then and now." An obvious link with the present were suggestions of a bomb holiday or moratorium so that UN negotiations might proceed in good faith. Other notes concerned fear as a spur to action. At the February 1946 conference with religious leaders here at the University, it was a scientist who raised the moral issue posed by the bomb to which the clergymen wearily replied that the Church had wrestled for centuries with man's propensity to use force and that they were thoroughly familiar with the scientists' current dilemma — how to alert people to danger without scaring them to the point of cynicism. Indeed, within a few months scientists were wondering whether their educational campaign, instead of frightening their listeners into rationality, was contributing to the talk of preventive war that began to surface. Federation leaders sought collective therapy through the American Psychological Association and were assured that while crippling, panicky fear was bad, action-goading fear, directed to constructive ends, was good. Today, scientists have created a potential hellfire that far outstrips the imagination of medieval poet or Calvinist preacher. Physicians describe it with clinical precision. And leading churchmen address the moral issue without worrying about cynicism.

But most of my then-and-now notes related to the dramatic change in scientists' own attitudes toward participation in public affairs. For young people the transition was quick and fairly painless. Commenting on Chicago's October 1945 Life article, Katherine Way told a reporter: "A year ago a scientist who sought publicity was regarded as a hack, advancing some personal vanity, but that article broke the ice. Nothing terrible happened, nobody's reputation was irretrievably lost." Yet it was not until 1960 that the American Association for the Advancement of Science officially subscribed to the proposition that "scientists bear a serious and immediate responsibility to help mediate the effects of scientific progress on human welfare."

Another legacy is today's organizational network. With the waning of hope for international control, the Federation of American Scientists lost members, influence, and its grassroots character as local affiliates disbanded. However, it remained a watchdog over issues more important to science, and in the 1960s membership rose again and new leaders concentrated on Szilard's technique of contacts with decision makers. Szilard himself promoted successor organizations which employ this strategy. Early after the war, with the help of Harrison Brown, he tried unsuccessfully to arrange meetings between U.S. and Soviet scientists so that they in turn might influence their governments, an idea that was realized after 1957 in the Pugwash Conferences on Science and World Affairs and again adopted in 1981 by the International Physicians Against Nuclear War. In 1962, Szilard and Bernard Feld launched another pressure-on-the-pulse-of-power scheme, the Council for a Livable World, to help elect senators who would vote yes on arms control treaties.

The Council still raises campaign funds and lobbies in Washington, but it recently joined the ranks of those who believe, as did Rabinowitch, in the importance of an informed public and now sponsors seminars and conferences. Two highly visible educational groups have their roots, not in the postwar period, but in the environmental movement of the 1960s. In 1981, the Union of Concerned Scientists, which had focused the attention of its large membership on the dangers of nuclear power, held meetings on campuses across the country to stimulate student interest in arms escalation and a nuclear weapons freeze. Members of Physicians for Social Responsibility describe the results of nuclear warfare in vividly horrifying detail with which the Atomic Scientists of Chicago did not dare confront their audiences, aware though they were of what two primitive bombs had done to Hiroshima and Nagasaki. The nuclear weapons freeze petition originated in the tiny Brookline, Massachusetts office of the Institute for Defense and Disarmament Studies where its founder, Randall Forsberg, acting on another Rabinowitch article of faith that knowledge of the facts is a prerequisite of sound policy, assembles statistics on the world's growing arsenal of conventional and nuclear arms.

In Chicago, The Bulletin of the Atomic Scientists remains the most enduring symbol of the postwar scientists' movement. Discussions of the arms race now employ a vocabulary far more sophisticated and involve concepts far more complex than those in use in 1945, but the nature of the problem that these discussions address was fully understood and clearly expressed in the offices and corridors of this university four decades ago.

MEMORIES ABOUT THE MCMAHON ACT

Edward Levi

(Mr. Levi presented a very informal talk and he did not wish to undertake the complete rewriting that he felt would be necessary for this publication. However, since this presentation provided an important historical perspective complementing the preceding offering by Mrs. Smith, the Editor, with Mr. Levi's approval, has undertaken to paraphrase his comments.)

Mr. Levi reminisced about the period following the introduction in Congress of the May-Johnson bill which was the original proposal for management of the atomic energy enterprise, and was strongly opposed by a large number of scientists because they viewed it as a proposal for military control of atomic energy.

From his position as an outsider (i.e., one who is not a scientist and had not been involved in the work of the Manhattan District), Mr. Levi observed that this campaign of the scientists against the May-Johnson bill took the form of an extraordinary and unique movement. The scientists were deeply concerned and very knowledgeable, at least on the technical side, about the implications of the new technology. The overwhelming public demonstration of their knowledge and commitment was like "a flash of light," making an incredible impression on public opinion makers, colleagues from other areas and congressmen.

The movement had spontaneity, a sense of great alarm, great faith and great excitement. At the same time it was not confrontational. The participants realized that this great public policy matter should be understood and discussed by the public.

The campaign was helped by having a well defined "wicked witch" to attack, the May-Johnson bill, and took on the character of a crusade. Mr. Levi suggested that in some sense it was a "children's crusade" because some of the characters, like Leo Szilard who was a principal leader of the movement, were "children at heart" who viewed their political efforts as an experiment designed to achieve the "correct" political controls and order, much as a scientist's experiment may achieve great (and hopefully correct) scientific results. The emphasis was on "civilian control of atomic energy" which was a way of saying that it was a crusade pointed to better international control, peaceful uses and freedom of research in general.

Mr. Levi's involvement began with a request from Robert Hutchins (then President of the University of Chicago) for an analysis of the May-Johnson bill. Mr. Hutchins apparently wanted to understand why his faculty members were stirring up such a fuss. Mr. Levi found in retrospect that the memorandum responding to Mr. Hutchin's request was "very clear and calm and describes the legislation. Hutchins took the unusual steps of having it sent to all members of the Academic Council."

This brought Mr. Levi into close contact with some of the scientists who were very active in the movement, including especially Leo Szilard who played a central role in the crusade. Mr. Levi told an anecdote that captures the manner (and manners) of Szilard:

"Leo Szilard began a relationship with me which continued over several years. When it began I had not met my wife. I first met her at an evening at which Francis

Friedman and I were discussing the proposed McMahon bill and the evil May-Johnson bill. My constant honeymoon or whatever it was with Leo Szilard continued further on into the first pregnancy of my wife. And when he had me over to his rooms, as he was constantly doing on whatever project it was (I avoided getting involved in his design for a new monetary system) I said to him, 'Leo, my wife is pregnant and she's home in bed and I really have to leave.' And he looked at me, as he often did, like a canary, and he said, 'If this continues, wouldn't it be better if she were in a hospital?' And I said, 'Better for whom?' And so, cocking his head like a canary, he said, 'For her???'"

As a result of the relationship that Mr. Levi developed with scientists active in the movement, Byron Miller, who along with Thomas Emerson had been assigned by the White House the task of funneling the ideas of scientists into the legislative process, asked him to serve as an unofficial representative between them and the scientists. Then Mr. Levi said, "There was then this very close relationship, particularly after the McMahon Committee was established and James Neuman was the general counsel there. There was a great deal of drafting of proposed legislation which went on in the basement of the old law school building here. With scientists and some lawyers sitting around the table taking down and trying to put in usable language what the scientists were saying. There was constant communication between those proposals, the reception of them in Washington through Byron Miller or Jim Neuman. Then a call back and a statement to the group, either that night or the next night or the next day, whatever, that no this won't do but how about this? How about that?"

Although it cannot be said that the bill was drafted in Chicago, it "can be said that there was this extraordinary arranged stream of input and with the statements coming back as to what was acceptable or apparently was acceptable or was not, and the whole thing was argued out with everybody trying to feel what was responsive to what 'scientists' wanted." It was an effort by the national administration and the scientists to come together on something and, in this respect, it was a good demonstration of the nonconfrontational aspect of the movement.

Mr. Levi spoke of those "extraordinary, delightful, wonderful meetings in Washington where scientists would amaze congressmen and newspapermen" and of the key role of Thorstin Hogness who got Eisenhower to persuade Senator Vandenberg to water down the Vandenberg amendment. He spoke of the network of relationships among the scientists who "were all over the place" and the fact it was a phenomenon hardly likely to be replicated. Then he raised the question, was it all worthwhile?

The theme or the slogan of the crusade became opposition to military control as represented by the May-Johnson bill. The May-Johnson bill, which was written by very good people, didn't specify military control but would have made it possible. And "the setting with which it was produced, the speed with which it was supposed to be adopted, the aura of being pushed through without discussion and having been prepared by the military, the belief that perhaps some elder statesmen from the scientists had agreed to it but not really knowing what it was, and that it could or would give control to the military, with all that that might mean as the continuation of the restrictions on research and communication, all of that gave rise to a strong almost accidental theme of the importance of civilian control over atomic energy." The Vandenberg Amendment to the McMahon bill was symbolic of the issue and, after it was watered down, its consequence was to establish a principle of full time civilian control.

Mr. Levi then went on to say that although people may have some doubts as to the effect of a short lived act such as this (it was amended within seven or eight years) "...it was an extraordinary achievement which really accomplished all that could have been expected of it at the time. It did set up a structure. It could not control the future. This wonderful event of this coming together, this enlightenment for that short period, was succeeded by all kinds of nasty events and the gradual loss of trust in the United States during the next few years. So that perhaps we have not done so badly when we look at the extraordinary difficulties which are around us today. That is, we have not done so badly in the sense of the crusade for the McMahon bill. There's always a time to take stock and this is a good time. I don't think we have the cheerfulness, the freshness, the sense of possible triumph. And I don't know that that can be recreated for this time. But at least that is the way it was then and it was worthwhile."