August 1941

Background of The Paper "Divergent Chain Reaction in Systems Composed of Uranium and Carbon" of February 14, 1940.

In May, 1939, Anderson, Fermi, and I carried out an experiment[12] in which neutrons were slowed down by water and reacted with uranium. This experiment showed that more neutrons are emitted than absorbed by uranium under the particular conditions in which the experiment was performed.

Our system contained 17 H atoms per U atom and the thermal neutron absorption of hydrogen rules out the possibility of a chain reaction at such concentrations. When we started this experiment we had hoped, however, that we might perhaps be able to draw from it the conclusions that a chain reaction could be maintained at a lower hydrogen-uranium ratio.

That the possibility of a chain reaction can not be proved in such a way had been first realized by G. Placzek. He pointed out that with increasing uranium concentrations the fraction of neutrons captured by uranium at resonanc increases so rapidly that a chain reaction might be very well impossible at any hydrogen-uranium ratio. Placzek put forward the suggestion of using helium in place of hydrogen for slowing down the neutrons. He emphasized that the absence of a thermal neutron absorption in helium would make it possible to use very high helium uranium ratios with the result that a high fraction of the neutrons would be slowed down to thermal energies without being captured by uranium at resonance.

In the experiment of Anderson, Fermi, and Szilard[12] tubes filled with uranium oxide were immersed in a water tank. Adjacent tubes were separated by a few centimeters of water in order to keep these distances within the range of thermal neutrons in this medium. For the interpretation of this experiment it was necessary to calculate the fraction of the neutrons absorbed at resonance in these uranium-oxide filled tubes. Fermi carried out such a calculation and noticed that the arrangement which we had used was more favorable with respect to resonance absorption than a homogeneous mixture of uranium oxide and water. Assuming a sharp absorption line in uranium it is indeed quite easy to see a posteriori that most of the resonance absorption will take place in a thin outside layer of the uranium oxide, whereas the inside of the cylinder, which will not appreciably contribute to the resonance neutron absorption, will still materially contribute to the useful thermal neutron absorption. Thus the use of uranium oxide layers of finite thickness has an advantage over the use of infinitely thin uranium oxide layers which would be equivalent to a homogeneous mixture of uranium oxide and water. Fermi calculated in detail the balance of

neutron absorption and emission for systems built up from alternating layers of uranium oxide and water. These calculations of Fermi showed that there was a much better chance for obtaining a chain reaction in such an inhomogeneous system of water and uranium oxide than in a homogeneous system of these two substances. For the purposes of such calculations it might be sufficient to consider only the lowest resonance absorption line of uranium but the width and intensity of this line enter into these calculations as major determining factors, and these quantities were known only within rather wide limits of experimental error. For this reason it was difficult to say whether or not a chain reaction can be maintained in the heterogeneous uranium oxide water systems which were considered by Fermi. (June, 1939)

Meanwhile it occurred to me that from the point of view of practical applications it would be very much better to use carbon in place of hydrogen for slowing down the neutrons. Led by this point of view, I became interested in this possibility in spite of the fact that carbon, on account of its larger atomic weight and smaller scattering cross-section, is very much less efficient in slowing down neutrons than hydrogen. (June, 1939)

The rate at which a nuclear chain reaction can be maintained will obviously be determined by the rate at which the heat which is developed in the reaction can be dissipated. Since carbon can withstand high temperatures it is much more efficient in this respect than water or paraffin. Led by these considerations, I made inquiries about carbon and found that it can be obtained in the form of graphite bricks, very pure, and at a moderate price. A structure composed of alternate layers of graphite bricks and uranium oxide appeared to be the simplest from a purely practical point of view and I attempted to form an estimate of the chances of a chain reaction in such a structure. As soon as this problem was seriously considered it became evident that these chances were appreciable even if one assumed that the uranium layers would absorb all the neutrons which reach them while the energy of the neutron is between 10 and 100 volts. (July, 1939)

In this respect, a system in which the neutrons are slowed down by carbon is very different from a system in which the neutrons are slowed down by water. Of the two, carbon is very much superior and this is mainly due to the fact that the range of thermal neutrons in carbon, is considerably larger than the range of the resonance neutrons of uranium in carbon, whereas the ranges of these two categories of neutrons are about equal in water.

Further simple considerations showed that the lattice of uranium spheres

embedded in graphite appeared even more favorable from the point of view of a chain reaction than the system of plane uranium layers which was initially considered. The efficiency of such a system was calculated and it was found that if small spheres of uranium metal were embedded in graphite there would be quite a good chance of obtaining a chain reaction in an experiment performed on a large but entirely practicable scale. There appeared to be an appreciable chance for success even if uranium oxide had to be used in place of uranium metal In the circumstances, the conclusion was reached that it would be better to perform an experiment on a large scale rather than to wait for measurements to be performed for the purpose of determining all the nuclear constants involved. (July 9, 1939)

Note for Memorandum:

Tentative steps were thereupon taken in this direction and among others, E. P. Wigner, and E. Teller were informed of these considerations. They shared the opinion that no time must be lost in following up this line of development and in the discussion that followed, the opinion crystallized that an attempt ought to be made to enlist the support of the Government rather than that of private industry. Dr. Wigner, in particular, urged very strongly that the Government of the United States be advised of certain possible consequences of this as well as some other lines of work connected with uranium. With this in mind, we approached Professor Albert Einstein and Dr. Alexander Sachs, and after a number of consultations, Dr. Einstein wrote a letter to the President of the United States recommending that a personal committee be appointed to act as a permanent liason between the Government and the physicists who are working on uranium. (August 2, 1939)

Einstein's letter and a memorandum which I was asked to write were submitted by Dr. Sachs to the President and Dr. Lyman J. Briggs was appointed as chairman of a Government committee. Dr. Sachs, Dr. Wigner, Dr. Teller and I were given an opportunity to explain to this committee why we believed that the work which is being done on uranium deserved the attention and the support of the Government. (October 21, 1939)

On this occasion a plea was made for the Government's support either financial or moral both for the work on uranium in general and for the work along the lines indicated in the paper in particular. It was stated that a lattice of uranium metal spheres embedded in graphite appeared to offer the greatest chance for immediate success; that about 100 tons of graphite and 10 to 20 tons of uranium would have to be used in a large scale experiment in order to produce a divergent chain reaction. A recommendation was made that steps be taken to prepare for the performance of such a large scale experiment and that methods of producing uranium metal from uranium oxide be explored. It was emphasized that before starting a large scale experiement the capture cross-section of carbon would be measured and that a few tons of graphite were required for this purpose. A memorandum summarizing these statements and recommendations was submitted to Dr. Briggs (October, 1939)

The value of the capture cross-section of carbon for thermal neutrons was not known at that time; only an upper limit for this magnitude of 0.01×10^{-24} cm^2 was published by Frisch, v. Halban, and Koch. To measure this value appeared, therefore, to be an urgent task, indeed. The usual methods for measuring small absorption cross-sections did not seem to be adequate for this purpose and so a method was devised which called for the study of the spatial distribution of the thermal neutron density in a large mass of graphite. The thermal neutron

density in graphite obeys a diffusion equation which contains the mean free path for scattering and the ratio of the capture and scattering cross-sections. Since the thermal neutrons are produced in the graphite through the slowing down of the fast neutrons emitted from the neutron source, this diffusion equation is not homogeneous. By introducing screens which are black for thermal neutrons one can obtain, however, experimental values which obey the homogeneous diffusion equation. The first experiment of this type which was originally planned (July 5, 1939) is described in the paper.

Due to various circumstances, experimentation along this line was halted between July, 1939, and March, 1940. While no information on the capture cross-section of carbon was as yet forth-coming, a increasingly optimistic view on the chances of a chain reaction in a uranium - graphite system appeared to be justified when it was realized that the neutron absorption in carbon could be materially reduced by allowing the bulk of the graphite to heat up to high temperatures. Thus having finally reached the conclusion that we may expect a divergent chain reaction in such a system, the paper was submitted in February, 1941, to the Physical Review for publication. After its acceptance, and after consultations with a number of my colleagues and Dr. Lyman J. Briggs, its publication was deferred at my request.