6. *America 1938 through November 1940 (1)*

During the Munich Crisis, I happened to be in Urbana, Illinois, and I spent one week listening to the news over the radio. When it was all over, I was convinced that within one year there would be war. I resigned my position at Oxford.

In January, 1939, I learned of the discovery of the fission of uranium by Otto Hahn. I saw at once that if neutrons are emitted in the fission process it might be possible to set up a chain reaction. "H.G. Wells, here we come!" I said to myself. Immediately I was obsessed with two thoughts: To do, as quickly as possible, an experiment to discover whether or not neutrons are in fact emitted in the fission process, and to contact those laboratories in America, England and France where such an experiment conceivably could be thought of, and performed. This I thought had to be done with a view to reaching an agreement that if neutrons were in fact emitted, this fact should remain a secret of the three countries involved, lest the Germans developed the atomic bomb first and used it in the impending war. I was not affiliated at the time with any university, but after scouting around I borrowed $2,000.00, rented ~~a radio~~ Radium radium, and teamed up with Dr. Walter Zinn, at that time instructor at City College. The experiment was actually set up at the Physics Department of Columbia University, and was performed on March 3, 1939. When I saw the neutrons emitted in the fission of uranium, I knew that the world was headed for trouble.

At that point I thought that from this point on there should be no difficulty about obtaining financial support for this work. But in this I was quite mistaken.

(cont'd)

6. *America 1938 through November 1940 (2)*

My attempt to keep the neutron emission of uranium secret ran into difficulties, and it collapsed when Joliot in Paris published his results and declined to cooperate. The circumstances surrounding this collapse are not without human interest.

Fermi and I teamed up and performed an experiment which we thought would show that a chain reaction could be maintained in a system composed of water and uranium. We actually thought that we had shown this when the experiment was completed, but then George Placzek dropped in for a visit and showed us that we were in error.

In July, when I was left alone in New York, I recognized that a graphite uranium system would have a much better chance to sustain a chain reaction, and that accordingly the liberation of nuclear energy on an industrial scale was at hand. My first concern was to warn the Belgian Government of this possibility, lest being unaware of it, they make available uranium to Germany from the Belgian Congo. It was this consideration which brought me into contact with Einstein, a contact which resulted in Einstein's historic letter to President Roosevelt.

(

(cont'd)

5/23/60

In response to Einstein's letter, the President appointed a committee which met for the first time on October 21, 1939. We did not look to the Federal Government for funds but rather for official recognition that would have enabled us to obtain private funds. During the meeting, through what might be described as a comedy of errors, the issue of funds came up and the committee promised to provide us with $6,000.00 in order to enable us to buy a few tons of graphite. By February, 1940, we had not heard anything further from the Government, and in the period from June 1939 to April 1940, not a single experiment was under way in the U.S. that was concerned with the possibility of setting up a chain reaction.

In February, 1940, I decided to take some drastic action. I sent a paper to the Physical Review, describing how a chain reaction may be set up in a graphite-uranium system. And I took a copy of this paper to Einstein in Princeton. Einstein wrote a letter saying that if the Government was not interested in pursuing this matter, my paper would be published in due course of time.

This provoked another meeting of the Uranium Committee at which I was asked to defer publication of my paper. In the mean time, the $6,000.00 promised to us were received by Columbia University, and some of the most urgent expenditures got under way, but nothing remotely resembling the scale that was needed.

L. A. [May 27 1940] [note]

At this point I received a letter from Turner in Princeton, who pointed out that in the chain reaction which I hoped to be able to set up there would be formed a new element which might be capable of undergoing fission. As we now know, this is in fact the case, and the element formed in the chain reaction is now called plutonium. Neither Fermi nor I had thought of this possibility, which was obviously of the utmost importance, and this realization increased my sense of urgency.

(cont'd)

On Rabi's advice, I enlisted the help of H. C. Urey, who prevailed on the Chairman of the Uranium Committee to appoint those of us who were actively interested in this problem to serve as a technical subcommittee of the Uranium Committee. We thought this would put us in a position to approach various laboratories in the U.S. and to enlist their cooperation in pursuing the various aspects of the problem, including the possibility raised by Turner's suggestion.

The Committee, having been duly appointed, met in Washington and ~~xxxx~~ when the meeting was opened by the chairman, he told us that the committee would be dissolved upon termination of the current meeting, because if the government were to spend a substantial amount of money--we were discussing sums of the order of a half million dollars--~~xx~~ and subsequently it would turn out that it is not possible to set up a chain reaction based on uranium, there might be a Congressional Investigation. If this were the case in such a situation, it would be awkward if the Government had made available funds on the recommendation of a committee whose membership comprised men other than American citizens of long standing. Fermi and I were not American citizens. Though Wigner was an American citizen, he was not one of long standing. Thus the work on uranium in the United States was brought to a standstill for the next six months. Mr. Wigner wrote a very polite letter to the Chairman of the Uranium Committee saying that he would hold himself in readiness to work for the Government on all matters related to defense, with the exception of uranium.

After reorganization in Washington, which put the Uranium Committee under Dr. Vannevar Bush's committee, Columbia University was given a contract in the amount of $40,000.00 to develop the Fermi-Szilard system. On November 1, 1940, I was put on the payroll of Columbia University under this contract. Since I was instrumental in inducing the Government to assume expenditures for exploring the possibility of setting up a chain reaction, and with a view to the possibility that our efforts might come to nothing, it was deemed advisable to set my salary at a low figure, i.e., $4,000.00 a year.

(cont'd)
Folder 7.

6. America 1938 – Nov 1940

Stenorette 1963 Tape
Summarized by L. S.

The fission of uranium was discovered by Hahn and Strassman late in 1938 and when I heard of it in January of 1939, it occurred to me that if neutrons were emitted in the fission process of uranium, uranium might sustain a chain reaction.

In order to induce the fission of uranium, uranium has to be bombarded with neutrons and the problem was how to distinugish the neutrons which may be emitted in the fission process, from the neutrons which are used to induce fission in uranium. I thought ~~It seemed to me~~ that the right way of solving this problem was to use photo-neutrons from beryllium, which are slow, to induce fission in uranium and then to look for fast neutrons which might be emitted in the fission process. This is what Dr. Walter Zinn and I have actually done on March 3rd, 1939, working as guests of the Physics Department of Columbia University. The experiment showed that about two neutrons are emitted in the fission of uranium for each neutron which is absorbed, and this meant that it might be possible to set up a chain reaction in uranium. Anderson and Fermi, as well as Halban, Joliot and Kowarski have independently reached the same conclusion, just about the same time.

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6. America 1938 through Nov 1940

Stenorette I
May 6, 1960
Interview

14 14

I came over to America (on January 2, 1938) and

[Anderson
3-14-68
A 17

did nothing but loaf. I didn't look for a position, I just thought I would wait and see. Then came the Munich crisis, and I was at the time that the Munich crisis broke visiting Goldhaber in Urbana, Illinois; and I spent a week listening to the radio, giving news about Munich, and when it was all over I sat down and wrote a letter to Lindemann, later Lord Cherwell, who was director of the Clarendon Laboratory where I was employed. The letter said that I am now quite convinced that there will be war, and therefore there will be little point in my returning to England unless they would want to use me for war work. If as a foreigner I would not be used for war work, I would not want to return to England but rather stay in America. And so I resigned at Oxford and stayed here.

I was still intrigued with the possibility of a chain reaction, and for that reason I was interested in elements which became radioactive when they were bombarded by neutrons and where there were more radioactive isotopes than there should have been. In particular, I was interested in indium. I went up to Rochester and stayed there for two weeks and made some experiments on indium, which finally cleared up this mystery. And it turned out that indium is not instable and that the phenomenon observed could be explained without assuming that indium is split by neutrons.

At that point I abandoned the idea of a chain reaction and of looking for elements which could sustain a chain reaction, and I wrote a letter to the British Admiralty suggesting the patent which has been applied for should be withdrawn because I couldn't make the process work. Before that letter reached them, I learned of the discovery of fission. This was early in January when I visited

Munich crisis: Last weeks of September 1938, ending with the agreement of September 30th.

Telegram, Szilard to Lindemann. October (?) 1938

Letter, Szilard to Lindemann. January 13, 1939.

Letter to British Admiralty withdrawing patent application. December 21, 1938

Telegram to British Admiralty cancelling above. January 26, 1939 (not in tapes)
Letter " " " February 2, 1939 (" ")
Letter, Szilard to Strauss. January 25, 1939

Letter from Brit. Adm. March 26, 1936
" " Director of Artillery, Oct 8, 1935

Bohr arrived in New York January 16, 1939

Mr. Wigner in Princeton, who was ill with jaundice. Wigner told me of Hahn's discovery. Hahn found that uranium breaks into two parts when it absorbs the neutron and this is the process which we call fission. When I heard this I saw immediately that these fragments, being heavier than corresponds to their charge, must emit neutrons; and if enough neutrons are emitted in this fission process, then it should be, of course, possible to sustain a chain reaction, for all the things which H.G. Wells predicted appeared suddenly real to me.

At that time it was already clear, not only to me but to many other people, and certainly it was clear to Wigner, that we are at the threshold of another world war. And so it became, it seemed to us, urgent to set up experiments which would show whether, in fact, neutrons are emitted in the fission process of uranium. I thought that if neutrons are in fact emitted in fission, this fact should be kept secret, I mean, it should be kept secret from the Germans, and so I was very eager to contact Joliot and to contact Fermi, the two men who were most likely to think of this possibility. I was still in Princeton and staying at Wigner's apartment - Wigner was in the hospital with jaundice and I stayed at his apartment - and I got up in the morning and wanted to go out. It was raining cats and dogs, and I said, "My god, I am going to catch a cold!" because at that time, the first years I was in America, each time I got wet I invariably caught a bad cold. However, I had no rubbers with me, so I had no choice, I just had to go out. I got wet and I came home with a very high fever, so I was not able to contact Fermi; but I went home and I went back to New York and as I opened the drawer to take my things out I saw there were Wigner's rubbers standing. I could have taken Wigner's rubbers and avoided the cold. But as it was I was laid up with fever for about a week or ten days, I don't know precisely how much. In the meantime, Fermi also thought of the possibility of a neutron emission and the possibility of a chain reaction and he went to a private meeting in Washington and talked about these things. Since it was a private meeting, the cat was not entirely out of the bag, but its tail was sticking out; and when I recovered I went to see Rabi, and Rabi told me that Fermi had similar ideas and that he had talked about them in Washington. Fermi was not in, so I told Rabi to please talk to Fermi and say that these things ought to be kept secret because it's very likely that neutrons are emitted, because this may lead to a chain reaction, and this may lead to the construction of bombs. So Rabi said he would, and I went back home to bed and to the Kings Crown Hotel. A few days later I got up and to see Rabi and I said to Rabi "Did you talk to Fermi?" Rabi said "Yes, I did." I said "What did Fermi say?" and he said Fermi said "Nuts." So I said "Why did he say 'Nuts'?" and Rabi said, "Well, I don't know, but he is in and we can ask him." So we went over to Fermi's office, and Rabi said to Fermi, "Look, Fermi, I told you what Szilard thought and you said 'Nuts!' and Szilard wants to know why you said 'Nuts!'" So Fermi said, "Well, there is the <u>remote</u> possibility that neutrons may be emitted in the fission of uranium and then of course that a chain reaction can be made." So Rabi said, "What do you mean by 'remote possibility?'" and Fermi said, "Well, ten per cent." And Rabi said, "Ten per cent is not a remote possibility if it means that we may die of it. If I have pneumonia and the doctor tells me that there is a remote possibility that I might die, and that it's ten per cent, I get excited about it."

From the very beginning the line was drawn; the difference between Fermi's position throughout this and mine was marked on the first day when we talked about it. We both wanted to be conservative, but Fermi thought that the conservative thing is to play down the possibility that this may happen, and I thought the conservative thing is to assume that it will happen and take all the necessary precautions. I then went and wrote a letter to Joliot of which some time I can give you a copy, in which I had told Joliot that we were discussing here the possibility of neutron emission of uranium in the fission process and the possibility of a chain reaction, and that I personally felt that these things should be discussed privately among the physicists of England, France and America; and that there should be no publication on this topic, if it should turn out that neutrons are, in fact, emitted, and that a chain reaction may be possible. This letter was dated February 2nd, 1939. I sent a telegram to England asking them to send a block of beryllium which I had made in Germany (?), with the kind of experiments in mind which I now was actually going to perform.

This block of beryllium can be used to produce slow neutrons, because if you put in the middle of this beryllium block radium, under the influence of the gamma rays of radium the beryllium splits and gives off slow neutrons. If uranium in the process of fission, which can be caused by slow neutrons, emits fast neutrons, these fast neutrons can be distinguished from the neutrons of the source by virtue of their higher energy.

There was at Columbia University some equipment which was very suitable for these experiments. This equipment was built by Dr. Walter Zinn who was doing experiments with them. And all we needed to do is to get a gram of radium, get a block of beryllium,

... uranium to the neutrons which come from expose a piece of uranium to the neutrons which come from beryllium, and then see by means of the ionization chamber which Zinn has built whether fast neutrons are emitted in the process. Such an experiment need not take more than an hour or two to perform, once the equipment has been built and if you have the neutron source. But of course we had no radium.

So I first tried to talk to some of my wealthy friends, but they wanted to know just how sure I am that this would work; so finally I talked to one of my not-so-wealthy friends. He was an inventor; his name is Benjamin Liebowitz, and he had some income, he derived some income from royalties. And I told him what this was all about, and he said "How much money do you need?" and I said, "Well, I'd like to borrow two thousand dollars." He took out his checkbook, he wrote out a check, I cashed the check, I rented the gram of radium, and in the mean time the beryllium block arrived from England. And with this radium and beryllium I turned up at Columbia and said (having talked to Zinn), said to the head of the department, "I would like to have permission to do some experiments." I was given permission to do experiments for three months. I don't know what caused this caution, because they knew me quite well; but perhaps the idea is a little too fantastic to be entirely respectable. And once we had the radium and the beryllium it took us just one afternoon to see those neutrons. Mr. Zinn and I performed this experiment.

In the mean time Fermi, who had independently thought of this possibility, had set up an experiment. His did not at first work so well, because he used a neutron source which emitted fast neutrons,

Notes to p. 18

Letter from Liebowitz to G.W.S. Check was dated March 9, 1939

Letter, Pegram to Szilard. April 6, 1939. Guest at Columbia from March 1 to June 1, 1939.

Experiment with Zinn performed March 3, 1939. Physics papers Nos. 13, 14.

Telegram to Strauss re experiment with Zinn (undated) sbs bk 2

Columbia University
in the City of New York
DEPARTMENT OF PHYSICS

April 6, 1939

Dr. Leo Szilard,
Pupin Physics Laboratories,
Columbia University.

Dear Dr. Szilard:

I told you that I would write you a letter to put on record my invitation to you to be a guest of the Department of Physics until June 1, 1939 to work on certain researches with Dr. Zinn and to have the privileges that are appropriate for a guest in our laboratory. Laboratory keys have already been issued to you, and I enclose with this a card by the use of which you can obtain a key to the outer door of the building by calling at Room 111 Low Memorial Library so that you may have access to the laboratory at times when the outer door is closed. The key obtained with this card is to be returned on leaving the building.

Sincerely yours,

George B. Pegram

GBP:H

George B. Pegram

Without compensation
March 1939 – Nov 1, 1940
Nov 1, 1940 Appoint Staff
of Ntl. Def. Res. Dir.,
Columbia Univ. (Under U.S.
grant to Columbia)
at $4000/yr.
until Feb 1, 1942 (then moved to Chicago,
as Chief Physicist Metall. Lab.
(Not sent)

F

but then he borrowed our neutron source and his experiment, which was of completely different design, also showed the neutrons.

And now there came the question: shall we publish this? There were intensive discussions about this. (con't'd page 20)

and So, Zinn and I, and Fermi and Anderson, each sent a paper to the Physical Review, a Letter to the Editor. But we requested that publication be delayed for a little while until we can decide whether we want to keep this thing secret or whether we will permit them to be published. Throughout this time I kept close touch with Wigner and with Edward Teller, who was in Washington; and I went down to Washington. Fermi also went down to Washington on some other business, I forget what it was, and Teller and Fermi and I got together to discuss whether or not this thing should be published. Both Teller and I thought that it should not. Fermi thought that it should. But after a long discussion, Fermi took the position that after all this is a democracy, if the majority is against publication he will abide by the wish of the majority, and he said that he would go back to New York and advise the head of the department, Dean Pegram, to ask that publication of these papers be indefinitely delayed. While we were still in Washington, we learned that Joliot and his co-workers have sent a note to Nature reporting the discovery that neutrons are emitted in the fission of uranium, and indicating that this might lead to a chain reaction. At this point Fermi said that in this case we are going to publish now everything. I was not willing to do that, and I said that even though Joliot has published this, this is just the first step, and that if we persist in not publishing, Joliot will have to come around; otherwise, he will be at a disadvantage because we will know his results and he will not know of our results. From that moment on Fermi was adamant that withholding publication makes no sense. I still did not want to yield and so we agreed that we will put up

Notes to p. 20

Szilard & Zinn. Physics paper No. 12

Fermi & ~~Szilard.~~ Anderson (look up reference)

Joliot (look up reference)

this matter for a decision to the head of the department, the physics department, Professor Pegram. Pegram hesitated for a while to make this decision, and after a few weeks he finally came and said that he had decided that we should now publish everything. He later told me why he decided this, and so many decisions were based on the wrong premise. Rabi was concerned about my stand because he said that everybody is opposed to withholding publication, only I want it in the Columbia group. This will make my position difficult, in the end impossible, and he thought that I ought to yield on this. He visited Urbana, this is what Pegram told me, that Rabi had visited Urbana and found that Goldhaber in Urbana knew of our research at Columbia; and from this Rabi concluded that these results are already known as far as Urbana, Illinois, and there is no point in keeping them secret. The fact was that I was in constant communication with Goldhaber and I wrote him of these results, and Goldhaber was pledged to secrecy. He talked to Rabi, because of course Rabi was part of the Columbia operation. So on this false premise, the decision was made that we should publish.

(April 18, 1961)

Unedited Manuscript of Leo Szilard

(Next four pages edited by Leo Szilard)

In the following months Fermi and I teamed up in order to explore whether a uranium-water system would be capable of sustaining a chain reaction. The experiment was actually done by Anderson, Fermi, and myself. We worked very hard at this experiment and saw that under the conditions of this experiment more neutrons are emitted by uranium than absorbed by uranium. We were therefore inclined to conclude that this means that the water-uranium system would sustain a chain reaction. Whether finally we should have said that in print I do not know. However, the fact is that we believed it until George Placzek dropped in for a visit. Placzek said that our conclusion was wrong because in order to make a chain reaction go, we would have to reduce the absorption of water; that is, we would have to reduce the amount of water in the system, and if we reduced the water in the system we would increase the parasitic absorption of uranium and he recommended that we abandon the water-uranium system and use helium for slowing down the neutrons. To Fermi this sounded impractical, and therefore funny, and Fermi referred to helium thereafter as Placzek's helium. I took Placzek more seriously, and while I had, for purely practical reasons, no enthusiasm for helium, I dropped then and there my pursuit of the water-uranium system. Thus, while Fermi went on examining this system in detail and trying to see whether by changing the arrangements he could not improve it to the point where it would sustain a chain reaction, I started to think about the possibility ad 22

? Does this Placzek letter belong here

? 30/11/1939(?) ~~1934(?)~~

gestrichen

Bk.f.2 (40)

CORNELL UNIVERSITY
ITHACA, NEW YORK

30. Nov 1939

DEPARTMENT OF PHYSICS
ROCKEFELLER HALL

30/11 39.

Lieber Szilard,

Mein Geschmiere ist auf den Umstand zurückzuführen, dass meine Füllfeder verschwunden ist, u. zw., wie ich vermute, im Bette des Zimmers 315. Vielleicht können Sie so freundlich sein, diesbezügliche Recherchen anzustellen und im positiven Falle das gefundene Objekt mir nach Indiana zusenden zu lassen. Ich werde Ihnen dann einen leserlichen Dankbrief schreiben können.

Vorliegende Gelegenheit kann ich gleich benützen, um Ihnen eine Formel für das $\overline{r^2}$ mitzuteilen, die für jede Masse gültig bleibt. Es ist

LM

$$\overline{r^2} = \frac{2Nl^2}{1-\cos\vartheta} \qquad (1)$$

Nun ist $N = \frac{\ln\frac{E_0}{E}}{\overline{\ln\frac{E_0}{E_1}}} = \frac{\ln\frac{E_0}{E}}{1+\frac{1-\alpha}{\alpha}\ln(1-\alpha)}$ (2) ; $\alpha = \frac{4M}{(M+1)^2}$

(Mittelwert des $\ln\frac{E_0}{E}$ nach einem Stoss)

Maximaler Energieverlust.

Not sent

Weiters ist der mittlere cosinus des Ablenkungswinkels

$$\overline{\cos\vartheta} = \frac{2}{3M} \quad (3)$$

Somit hat man:

$$\overline{r^2} = \frac{6M}{(3M-2)\left\{1+\frac{1-\alpha}{\alpha}\ln(1-\alpha)\right\}} \cdot l^2 \ln\frac{E_0}{E} = f \cdot l^2 \ln\frac{E_0}{E} \quad (4)$$

Für Wasserstoff ($M=1$) wird der Faktor f

$f = 1$,

für $M \gg 1$ wiederum

$f = M$,

~~früher~~ wie es sein muss.

Für die Zwischenwerte folgende Tabelle:

	M	f
H	1	6
D	2	4,15
He	4	5,65
C	12	13,4
	$\gg 1$	M

Für C ist also der Grenzwert M schon eine gute Näherung. (13,4 gegen 12)

Je mehr ich mir übrigens das Uran überlege, desto wichtiger scheint mir es, die chombinierte <u>Bor-Selbstabsorptionskurve</u> zu machen, <u>bevor</u> man die Verlangsamung mit C oder D probiert, denn ~~auf~~ erst auf Grund dieser Kurve wird man imstande sein, die Versuchsbedingungen vernünftig zu wählen und so letzten Endes Zeit sparen.

Beste Grüsse Ihr G. Placzek

of using perhaps graphite instead of water. This brought us to the end of June. We wrote up our paper (Phys. 14), Fermi left for the summer to go to Ann Arbor, and I was left alone in New York. I still had no position at Columbia; my three months (March 1 - June 1, 1939) as a guest were up, but there were no experiments going on anyway and all I had to do was to think. Some very simple calculations which I made early in July showed that the graphite uranium system was indeed very promising, and when Wigner came to New York, I showed him what I had done. At this point, both Wigner and I began to worry what would happen if the Germans got hold of some of the vast quantities of the uranium which the Belgians had in the Congo. So we began to think through what channels we could approach the Belgian Government and warn them against selling any uranium to Germany.

It occurred to me then that Einstein knew the Queen of the Belgians, and I suggested to Wigner that we visit Einstein, tell him about the situation, and ask him whether he might not write to the Queen of the Belgians. We knew that Einstein was somewhere on Long Island but we didn't know precisely where, so I phoned his Princeton office and I was told he was staying at Dr. Moore's cabin at Peconic, Long Island. Wigner had a car and we drove out to Peconic and tried to find Dr. Moore's cabin. We drove around for about half an hour. We asked a number of people, but no one knew where Dr. Moore's cabin was. We were on the point of giving up and about to return to New York when I saw a boy of about seven or eight years of age standing at the curb. I leaned out of the window and I asked: "Say, do you by any chance know where Professor Einstein lives?" The boy knew and he offered to take us there, though he had never heard of Dr. Moore's cabin.

This was the first Einstein heard about the possibility of a chain reaction. He was very quick to see the implications and perfectly willing to do anything that needed to be done. He was reluctant to write to the Queen of the Belgians, but he thought he would write to one of the cabinet members of the Belgian government whom he knew, and he was about to do just that when Wigner said that we should not approach a foreign government without giving the State Department an opportunity to object. So Wigner proposed that Einstein write the letter and send a copy to the State Department with a covering letter. Einstein should say in that covering letter that if we don't hear from the State Department within two weeks, then he will send the letter to Belgium.

Having decided on this course, in principle, we returned to New York and Wigner left for California. (This goes to show how "green" we were. We did not know our way around in America, we did not know how to do business, and we certainly did not know how to deal with the government.) I had, however, an uneasy feeling about the approach we had decided upon and I felt that I would need to talk to somebody who knew a little bit better how things are done. I then thought of Gustav Stolper. He used to live in Berlin, where he had published a leading German economic journal and had been a member of the German parliament; now he was living as a refugee in New York. I went to see him and talked the situation over with him. He said that he

Notes to p. 24

Letter to Ambassador of Belgium (no date)

Draft of letter to Secretary of State (no date)

Also get "Einstein letter folder"

thought that Dr. Alexander Sachs, who was economic adviser to the Lehman Corporation and who had previously worked for the New Deal, might be able to give us advice on how to approach the American government, and whether we should approach the State Department or some other agency of the government. He telephoned Dr. Sachs and I went to see him and I told him my story. Sachs said that if Einstein were to write a letter to President Roosevelt, he would personally deliver it to the President, and that there was no use going to any of the agencies or departments of the government; this issue should go to the White House. This sounded like good advice, and I decided to follow it.

In the meantime, Teller arrived in New York and I asked Teller whether he would drive me out to Peconic. Teller and I went to see Einstein and on this occasion (second visit) we discussed with Einstein the possibility that he might write a letter to the President. Einstein was perfectly willing to do this. We discussed what should be in this letter and I said I would draft it. Subsequently, I sent Einstein two drafts to choose from, a longer one and a shorter one.

We did not know just how many words we could expect the President to read. How many words does the fission of uranium rate? So I sent Einstein a short version and the longer version; Einstein thought the longer version was better, and this is the version which he signed. The letter was dated August 2, 1939. I handed it to Dr. Sachs for delivery to the White House.

Letter, Szilard to Wigner. August 9, 1939.

Two letters, Einstein to Roosevelt. August 2, 1939.

Letter of transmittal for Einstein letter, Szilard to Sachs, August 15, 1939.
and Memorandum

I should perhaps say that this was not the first approach to the government. Soon after we had discovered the neutron emission of uranium, Wigner came to New York and we met, Fermi and I and Wigner met in the office of Dr. Pegram, head of the department; and Wigner said that this is such a serious business that we cannot assume the responsibility for handling it, we must contact the government and inform the government. Wigner said that "Well, we can do that," he would call the new - Charles Edison, who was, I think, the new Assistant Secretary of the Navy or Secretary of the Navy - and told Charles Edison that Fermi would be in Washington the next day and would be glad to meet with a committee and explain such matters which might be of interest to the Navy.

So Fermi went there. He was received by a committee. He told in his cautious way the story of uranium and what potential possibilities were involved. But there the matter ended. I got an echo of this through Merle Tuve. Ross Gunn, who was an adviser to the Navy and who attended this conference, telephoned Tuve and asked him, "Who is this man Fermi? What kind of a man is he? Is he a Fascist or what? What is he?" Nothing came of this.

In July, after I took a rather optimistic view of the possibility of setting up a chain reaction in graphite and uranium, I approached Ross Gunn and told him that the situation does not look too bad. The situation, as a matter of fact, looked so good that we ought to experiment at a faster rate than we have done before, that we had no money for this purpose, and I wondered if the Navy could make any funds available. Afterward I had a letter in reply, in which Ross

Gunn explained that there was almost no way in which the Navy could support this type of research, but that if we got any results which might be of interest to the Navy, they would appreciate it if we would keep them informed. This was the second approach to the government.

The letter of Einstein was dated August 2nd. August passed and nothing happened. September passed and nothing happened, and finally I got together with Teller and Wigner and we decided we'd give Sachs two more weeks, and if nothing happens we will use some other channel to the White House. However, suddenly Sachs began to bestir himself and we received a phone call from him in October saying that he had seen the President and transmitted Einstein's letter to him, and that the President has appointed a committee under the chairmanship of Lyman J. Briggs as Director of the National Bureau of Standards, and other members of the committee were Colonel Adamson of the Army and Admiral* Hoover from the Navy. The committee was to meet on October 21st, and Briggs wanted to know who else he should include. Well, I told Sachs that, apart from Wigner and me, I thought that Edward Teller ought to be invited because he lives in Washington and he could act as liaison between us and the committee. This was done. In addition, Briggs invited Dr. Tuve. Dr. Tuve had to go to New York and so he suggested that Dr. Roberts sit in for him.

* Correct title: Commander.

It was our general intention not to ask the government for money, but to ask only for the blessing of the government so that we then, with the blessing of the government, would go to foundations, raise the funds, and get some co-ordinated effort going. However, these things never go the way you have planned them.

After I presented the case, and Wigner spoke, Teller spoke; and Teller spoke in two capacities. In his own name he strongly supported what I had said and what Wigner had said. Then he said, now having spoken for himself, he will now speak for Dr. Tuve. Dr. Tuve could not attend the meeting, but he had visited New York and he had a discussion with Fermi; and it is Dr. Tuve's opinion that at this time it would not be advisable - "No," said Teller, "That's not what he said. He said it would not be possible to spend more money on this research than $15,000." We had no intention to ask for any money from the government at this point, but since the issue of money was injected , the representative of the Army asked "How much money do you need?" And I said that all we need money for at this time is to buy some graphite; and the amount of graphite which we would have to buy would cost about $2,000. Well, I don't know: maybe a few experiments which would follow would raise the sum to $6,000 - in this order of magnitude.

At this point the representative of the Army started a rather longish tirade, and he told us that it is naive to believe that we can make a significant contribution to defence by creating a new explosive. He said that if a new weapon is created, it usually takes

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Letter and Memorandum, Szilard to Briggs, October 26, 1939, setting forth results of October 21st meeting.

Letter, Szilard to Pegram, reporting on meeting. October 21, 1939 two wars before one can know whether the weapon is any good or not. And then he explained rather laboriously that it is in the end 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; and he said in his high-pitched voice that it was very interesting for him to hear this. He always thought that weapons were very important and that this is what costs money; and this is why the Army needs such a large appropriation. But he was very interested to hear that he was wrong - it's not weapons but the morale which wins the wars - and if this is correct, perhaps one should take a second look at the budget of the Army, maybe the budget could be cut. Colonel Adamson wheeled around to look at Mr. Wigner and said, "Well, as far as those two thousand dollars are concerned, you can have it." This is how the first money promise was made by the government.

I should mention that, until the government showed interest, and the first interest it showed was the appointment of this committee, I was undecided whether this development ought to be carried by industry, or whether it ought to be carried by the government. And so, just a week or two before the meeting in Washington, I had met with the Director of Research of the Union Carbon and Carbide Company, E.W.F.Barrett. The appointment was made by Strauss, and there was some mix-up about it, because they expected Fermi and it was I who turned up.

I saw five people sitting around the table, and I told them that the possibility of a chain reaction between uranium and graphite must be taken seriously; and at this point we cannot say very much about this possibility; and that we could talk about it with much greater assurance if we first measured the absorption of neutrons in

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Meeting with Barrett took place October 16, 1939.

graphite. It was for this purpose that we would need about two thousand dollars' worth of graphite, and I wondered whether they might lend or give us this amount of graphite on loan; the experiment would not damage the graphite and we could return it to them.

F.W. Barrett said, "You know, I'm a gambling man myself, but you are now asking me to gamble with the stockholders' money, and I'm not sure that I can do that. What would be the practical applications of such chain reaction?" And I said that I really cannot say what the practical applications would be at this point, that there is very little doubt in my mind that such a revolution is phenomenal, and will find its practical applications ultimately, but it is too early to say that. We had first to see whether we can get it going, and under what conditions it can be set up.

After I left the meeting I had an uneasy feeling that I did not convince anybody there. After all, I was a foreigner and my name was not so well known. I was not well known as a physicist, certainly not to these people. So I sat down and I wrote a letter, in which I wrote to Mr. Barrett that - in which I invited him to lunch with Dr. Pegram, who was head of the department and dean of the graduate school, and Dr. Fermi, who after all was a Nobel Prize winner and his name was quite well known, one day at his convenience the following week at Columbia University. He replied that the following week he would not be in town, he did not suggest an alternate date, and he wrote that they had decided that they would not be in a position to let us have any graphite except on a straight purchase basis. I remember that I was quite depressed by that letter, and showed it to

Letter, with memorandum, Szilard to Barrett, October 18, 1939.

Pegram, who thought that I was too easily discouraged. And maybe I was.

The Washington meeting was followed by the most curious period in my life. We heard nothing from Washington at all. By the first of February (1940) there was still no word from Washington - at least, none that reached me. I had assumed that once we had demonstrated that in the fission of uranium neutrons are emitted, there would be no difficulty in getting people interested, but I was wrong. Fermi didn't see any reason to do anything, since we had asked for our money to buy graphite and since we hadn't yet gotten the money, and was interested in working on cosmic rays. I myself waited for the developments in Washington, and amused myself by making some more detailed calculations on the chain reaction of the graphite-uranium system.

It is an incredible fact in retrospect that between the end of June 1939 and the spring of 1940, not a single experiment was under way in the United States which was aimed at exploring the possibilities of a chain reaction in natural uranium.

Late in January or early in February 1940, I received a reprint of a paper by Joliot in which Joliot investigated the possibilities of a chain reaction in a uranium-water system. In a sense this was a similar experiment to the one which Anderson, Fermi and I had carried out and published in June (1939). However, Joliot's experiment was made in a different set-up, and I was able to conclude from it what I was not able to conclude from our own experiment: namely, that the water-uranium system comes very close to being chain-reacting, even though

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Joliot (History Box)

Anderson, Fermi, Szilard. Physics paper No. 14

it does not quite reach this point. However, it seemed to come so close to being chain-reacting, that if we improved the system somewhat by replacing water with graphite, in my opinion we should have gotten over the hump.

I read Joliot's paper very carefully and made a number of small computations on it, and then I went to see Fermi, with whom I was no longer in daily contact because my function at Columbia had ceased. We had lunch together and Fermi told me that he was on the point of going to California. I asked him, "Did you read Joliot's paper?" And he said he did, and I asked him "What did you think of it?" and Fermi said "Not much." At this point I saw no reason to continue the conversation and went home.

I then went to see Einstein again in Princeton, and told him that things are not moving at all. And I said to Einstein that I think the best thing I can do now is to go definitely on record that a graphite-uranium system would be chain-reacting, by writing a paper on the subject and submitting it for publication to the *Physical Review*. I suggested that we reopen the matter with the government, and that we propose to take the position that I am going to publish my results unless the government asks me not to do so and unless the government is willing to take some action in this matter.

Accordingly, I wrote a paper for publication and sent it to *Physical Review* on February 16th (1940). And I brought the paper over to Pegram, who was somewhat embarrassed because Fermi was out of town and Pegram did not know what action he should take. However, he said that he must take some action, so he went to see Admiral Bowen in

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A-55

Letter, Szilard to Tate, Editor of Physical Review. February 14, 1940

Post-card. Receipt from Physical Review. February 19, 1940

Letter, Szilard to Tate, Feb 6, 1940 (c short version)

~~Correspondence Szilard - Sach -~~
~~Einstein - (Roosevelt?)~~

Washington, who Pegram thought might take some interest, because, after all, atomic energy might be used for driving submarines.

On the basis of the conversation which I had with him, Einstein wrote to Sachs, and Sachs wrote again to the President, and the President replied that he thought that the best way to continue research would be to have another meeting of the Uranium Committee. And now something most tragic and comic happened. Sachs called up Briggs, having received a letter from the White House, and suggested a meeting be called. And Briggs said he was on the point of calling a meeting, and that he wanted to invite Sachs and Dr. Pegram to attend. And Sachs said, "Well, what about Szilard and Fermi?" and Briggs said, "Well, you know, these matters are secret and we do not think that they should be included."

The fact of the matter is, Sachs blew up at this point, because this was after all his meeting, and why should the people who are doing the job and who produce the figures not be included? This, however, was a misunderstanding. Briggs did not want to call the meeting because he has heard from the White House; he wanted to call the meeting at the initiative of Admiral Bowen, whom Pegram had contacted; so that Sachs and Briggs talked to each other to cross purposes. They were talking not about the same meeting but about different meetings. However, somehow things got straightened out and the meeting was called which Fermi and I did in fact attend. April 27

I now have to go back to the summer of 1939, when in July I made the first beginnings to compute the uranium-graphite system, and relate something that I forgot to relate earlier. As soon as I saw

Letter, Einstein to Sachs, March 7, 1940. See page 2. (Photostatic copy)

Letter, Sachs to Roosevelt. March 15, 1940, transmitting above.

Roosevelt to Sachs, April 5, 1940.

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that the uranium-graphite system might work, I wrote a number of letters to Fermi telling him that I felt this is a matter of some urgency, and that we should not waste our time by making detailed physical measurements of the individual constants involved, but rather try to get a sufficient amount of graphite and uranium to approach the critical mass and build up a chain-reacting system. Fermi's response to this crash program was very cool. He said that he thought of the possibilities of using carbon instead of water, that he has computed how a homogeneous mixture of carbon and uranium would behave, and that he found that the absorption of carbon would have to be indeed exceedingly low in order to make such a system chain-reacting. I knew very well that Fermi must be aware of the fact that a homogeneous mixture of uranium and carbon is not as good as a heterogeneous uranium-carbon system; he computed the homogeneous mixture only because it was the easiest to compute. And this showed me that Fermi did not take this matter really seriously. And this was one of the factors which induced me to approach the government quite independently of whatever Fermi or Columbia University might feel.

In July, (1939) when I reported to Pegram my optimistic views about graphite, and told him why I thought the matter was urgent, he took the position that even though the matter appears to be rather urgent, this being summer and Fermi being away, there is really nothing that usefully could be done until the fall - September, or perhaps October. This was the second factor which induced me to disregard everything else and go to the government directly.

Letters, Szilard to Fermi.	July 3, 1939 July 5, 1939 July 8, 1939 July 11, 1939
Letters, Fermi to Szilard	July 9, 1939
Fermi to Pegram	July 11, 1939

Now, in the spring of 1940 we were advised that the money, $6,000 which the Committee promised us, was available. We bought some graphite, and Fermi started an experiment to measure the absorption of that graphite. When he finished his measurement, the question of secrecy again came up. I went to his office and said that "Now that we have this value, perhaps the value ought not to be made public." At this point Fermi really lost his temper, at this point he really thought that this was absurd. There was nothing much more I could say, but next time when I dropped in his office he told me that Pegram had come to see him, and Pegram thought that this value should not be published. From that point on, secrecy was on.

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Anderson
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SAN DIEGO: UNIVERSITY OF CALIFORNIA
LA JOLLA, CALIFORNIA 92038

Re-listened +
corrected p. 32-41

UNIVERSITY OF CALIFORNIA—(Letterhead for interdepartmental use)

I was still intrigued with the possibility of a chain reaction and for that reason I was interested in elements which became radioactive ~~and~~ when there were no positive ? bombarded by neutrons and where there are more radioactive isotopes than there should have been. In particular, I was interested in Indium. I went up to Rochester and stayed there for two weeks and made some experiments on indium, which finally cleared up this mystery. And it turned out that indium is not ~~xxxxxxxxx~~ unstable and that phenomena ~~x~~ observed could be explained without assuming that indium is ~~a~~ split by neutrons (?). At that point I ~~xxxxxxxxxxxxxxxxxxxx~~ abandoned the idea of a chain reaction and of looking for elements which could sustain a chain reaction and I wrote a letter to the British Admiralty suggesting the patent which I had applied for should be withdrawn because I couldn't make the process work. Before that letter reached them, I learned of the discovery of fission. This was early in January 1939 when I visited Mr. Wigner in Princeton, who was ill with jaundice. Wigner (?) told me of ~~xxxxx~~ Hahn's discovery. Hahn found that uranium breaks into two parts when it absorbs the neutron and this is the process which we call fission. When I heard this I saw immediately that these fragments, being heavier than corresponds to the charge (?) must emit neutrons, and if enough neutrons are emitted in this fission process, then it shall be, of course, possible to sustain a chain reaction, for all the things which

H. G. Wells predicted appeared suddenly real to me. At that time it was already clear, not only to me but to many other people, and certainly it was clear to Wigner(?) that we were at the threshold of another world war. And so, it became, it seemed to us, urgent to set up experiments which would show whether, in fact, neutrons are emitted in the ~~ø~~ fission process of uranium. I thought that if neutrons are in fact emitted in the fission process, this fact should be kept secret - I mean, it should be kept secret from the Germans - and so I was very eager to contact Joliot and to contact Fermi ~~Farimen(?)~~ ~~Faroney~~(?), the two men who were most likely to think of this possibility. I was still in Princeton and staying at Wigner's apartment - Wigner was in the hospital with jaundice so I stayed at his apartment - and I got up in the morning and wanted to go out -- it was raining cats and dogs, and I said: "My god, I am going to catch cold!" because at that time, the first years I was in America, each time I got wet I invariably caught a bad cold. However, I had no rubbers with me, so I had no choice - I just had to go out. I got wet and I came home with a very high fever, so I was not able to contact Fermi ~~Feminet(?)~~, but I went home and I went back to New York and as I opened the drawer to take my things out I saw Wigner's rubbers standing there; I could have taken them and avoided the cold. But as it was, I was laid up with fever for about a week or ten days, I don't know exactly how long. In the meantime, ~~Fermi~~ Fermi also thought of the possibility of a neutron emission and the possibility of a chain reaction and he went to a private meeting in Washington and talked about these things. Since it was a private meeting, the cat was not entirely out of the bag but its tail was sticking out and when I recovered I went to see Rabi and Rabi told me that Fermi had similar ideas and that he had talked about them in Washington; Fermi was not in, so I told Rabi to please talk to Fermi and say that these things ought to be kept secret because it's very likely that neutrons are emitted and because this may lead to a chain reaction and this may lead to the construction of a bomb. So Rabi said he would and I went back home to bed and . A Few days later I got up and went to see Rabi and I said to him: "Did you talk to Fermi?" Rabi said, "Yes, I did." I said, "What did Fermi say?" and he said Fermi said "Nuts!" So I said, "Why did he say 'Nuts!'?" and he said, "Well, I don't know, but he is in and we can ask him? So we went over to Fermi's office and Rabi said: "Look, Fermi, I told you what Szilard thought and you said, 'Nuts!' and Szilard wants to know why you said, 'Nuts!'" So Fermi said: "We-e-l-ll, there is the remote possibility that neutrons may be emitted in the fission of uranium and then of course that a chain reaction can be made."

So, Rabi said, what do you mean by 'remote possibility'?" and Fermi said, "Well, 10 per cent." And Rabi said: "Ten per cent is not a remote possibility if it means that we may die of it. If I have pneumonia and the doctor tells me that there is a remote possibility that I might die, and that it's ten per cent, I get excited about it."

From the very beginning the line was drawn; the difference between Fermi's position throughout this and mine was marked on the first day when we talked about it. We both wanted to be conservative but Fermi thought that the conservative thing was to play down the possibility that this may happen and I thought the conservative thing is to assume that it will happen and take all the necessary precautions. I then wrote a letter to Joliot (of which sometime I can give you a copy), in which I had told Joliot that we were discussing here the possibility of neutron emission of uranium in the fission process and the possibility of a chain reaction and that I personally decided that this thing should be discussed privately among the physicists of England, France and America and that there should be no publication on this topic, if it should turn out that neutrons are, in fact, emitted and that a chain reaction may be possible. This letter was dated February 2nd, 1939. I sent a telegram to England asking them to send a

? Birmingham

block of beryllium which I had made in Germany, with the kind of experiments in mind which I now was actually going to perform.

G. Amenece 1938 - Nov. 1940 (S.) Stenorette I
5.6.60 ER

This block of beryllium can be used to (distorted - several - about 3 - lines undecipherable here -)

(from about 90 to end of tape only a few disjointed words come through)

This block of beryllium can be used to produce slow neutrons, because if you put in the middle of this beryllium block radium, under the influence of the gamma rays of radium the beryllium splits and gives off slow neutrons. If uranium in the process of fission (which can be caused by slow neutrons) emits fast neutrons, these fast neutrons can be distinguished from the neutrons of the source by virtue of their higher energy.

There was at Columbia University some equipment which was very suitable for these experiments; this equipment was built by Dr. Walter Zinn (?) who was doing experiments with them. And all we needed to do is to get a gram of radium, get a block of beryllium, expose a piece of uranium to the neutrons which come from beryllium, and then see by means of the ionization chamber which Zinn had built whether fast neutrons are emitted in the process. Such an experiment need not take more than an hour or two to perform, once the equipment has been built and you have the neutron source. But of course we had no radium.

I first tried to talk to my wealthy friends, but they wanted to know just how sure I am that this would work, so finally I talked to one of my not-so-wealthy friends -- he was an inventor, he was not poor but he was not exactly wealthy. He was a successful inventor; his name is Benjamin ~~Liberich~~ Liebowitz, and he

6. America 1938 - Nov. 1940 (7)   Stenorette I
5.6.60 SR

had some income, mainly derived from royalties. I told him what this was all about, and he said "How much money do you need?" and I said 'Well, I'd ~~MM~~ like to borrow two thousand dollars." He took out his checkbook, he wrote out a check, I cashed the check, I rented the gram of radium, and in the meantime the beryllium block arrived from England -- and with this radium and beryllium I turned up at Columbia and said (having talked to Zinn) said to the head of the department "I would like to have permission to do some experiments." I was given permission to do experiments for three months. I don't know why was caused all this caution, because they knew me quite well; but perhaps the idea was a little too fantastic to be entirely respectable. And once we had the radium and the beryllium it took us just one afternoon to see those neutrons. Mr. Zinn and I performed this experiment. on March 3, 1939

In the meantime Fermi, who had independently thought of this possibility, set up an experiment; his did not at first work so well, because he used a neutron source which emitted fast neutrons, but then he borrowed our neutron source and he did an experiment which was of completely different design but ~~HHH~~ also showed the neutrons.

And now there came the question -- should we publish this? There were intensive discussions about this, and

(END OF TAPE # I).

So, , I, and Anderson, each sent a paper to________ ________, a letter to the editor. We requested that the publication ~~of thems~~ be delayed for a little while until we could decide whether we want to keep this thing secret or whether we would permit them to be published. Throughout ~~Bafaxa~~ this time I had kept in close touch with Wigner (?) and with Edward Teller, who was in Washington, and I went down to Washington. (Fermi ~~Sammy? Fammy?~~ also went down to Washington on some other business, I forget what it was) and Teller, Fermi ~~Fammy Sammy~~ and I got together to discoss whether or not this thing should be published. Both Teller and I thought that it ~~they~~ should not. Fermi ~~Sammy (Fammy)~~ thought that it ~~they~~ should. But after a long discussion, Fermi ~~Sammy?~~ took the position that after all this is a democracy, the majority is against publication, he will abide by the wish of the majority and he said that he would go back to New York and advise the head of the department, Dean Pegram ~~Bigram (?)~~ to ask that publication of these papers be indefinitely delayed. While we were still in Washington, we learned that ~~Zolier~~ (Joliot ~~(?)~~) and his co-workers have sent a note to reporting ~~that~~ the discovery that neutrons are emitted in a fission of uranium and indicating that this might lead to a chain reaction. At this point Fermi ~~Sammy (Fammy)~~ said that ~~th~~ in this case we are now going to publish everything. I was not willing to do that

6. 1938 - Nov. 1940 (10). Stenorette II 5.6.60 S.R.

and I said that even though Joliot (?) has published this, this is just the first step, and that if we persist in not publishing, Joliot will have to come around. Otherwise, he will be at a disadvantage because we will know his results and he will not know our results. From that moment on Fermi (?) was adamant that withholding publication made no sense. I still did not want to yield and so we agreed that we will put up this matter for a decision to the head of the department, the physics department, Professer Pegram. Pegram hesitated to make this decision and after a few weeks he finally came and said that he had decided that we should now publish everything. He later told me why he decided this, and so many decisions (?) were based on the wrong premise. Rabi ~~Robby~~ (?) was concerned about my stand because he said that everybody is opposed to withholding publication, only I   in the   group. This will make my position in the end very difficult, impossible, and he thought that I ought to yield on this.   this is what Pegram ~~Bigenam~~ (?) told me, that   visited Urbana and found that Goldhaber ~~Goldtraub (?)~~ in Urbana knew of our research at Columbia and from this Rabi ~~Robby~~ concluded that these results are already known as far as Urbana, Illinois, and there is no point in keeping them secret. The fact was that I was in constant communication with Goldhaber.

6 1938 - Nov 1940 (ir.)

Edited by L.S. from Stenotype II

April 18, 1961

Pages 25 to 29 of an Unedited Manuscript

of Leo Szilard

In the following months Fermi and I teamed up in order to explore whether a uranium water system would be capable of sustaining a chain reaction. The experiment was actually done by Anderson, Fermi and myself. We worked very hard at this experiment and we saw that under the conditions of this experiment more neutrons are emitted by uranium than absorbed by uranium. We were therefore inclined to conclude that this means that the water uranium system would sustain a chain reaction. Whether finally we should have said that in print I do not know. However, the fact is that we believed it until George Placzek dropped in for a visit. Placzek said that our conclusion was wrong because in order to make a chain reaction go, we would have to ~~eliminate~~ reduce the absorption of water; that is, we would have to reduce the amount of water in the syst

6 1938-Nov. 1940 (12.)

Edited by L.S. from Stenderette II 4-18-61

and if we reduced the water in the system we would increase the parasitic absorption of uranium and he recommended that we abandon the water uranium system and use helium for slowing down the neutrons. To Fermi this sounded impractical and therefore funny, and Fermi referred to helium thereafter as ~~Placzek~~ Placsek's helium. I took Placsek more seriously and, while I had, for purely practical reasons, no enthusiasm for helium, I dropped then and there my pursuit of the uranium water system; thus, while Fermi went on examining this system in detail and trying to see whether by changing the arrangements he could not improve it to the point where it would sustain a chain reaction, I started to think about the possibility of using ~~p~~ perhaps graphite instead of water. This brought us to the end of June. We wrote up our paper, (Phys. 14) Fermi left for the summer to go to Ann Arbor, and I was left alone in New York. I still had no position at Columbia; my three months as a guest (March-May 31 1939) were up, but there were no experiments going on anyway and all I had to do was to think. Some very simple calculations which I made early in July showed that the graphite uranium system was indeed very promising, and when Wigner came to New York, I showed him what I had done. At this point, both Wigner and I began to worry what would happen if the Germans ~~gmxkx~~ got hold of some of the vast quantities of the uranium which the Belgians had in the Congo. So we began to think through what channels we could approach the Belgian Government and warn them against selling any uranium to Germany.

It occurred to me then that Einstein knew the Queen of the Belgians, and I suggested to Wigner that we visit Einstein, tell him about the situation and ask him whether he might not write to the Queen of the Belgians. We knew that Einstein was somewhere on Long Island but we didn't know precisely where, so I phoned his Princeton office and I was told he was staying at Dr. Moore's cabin at Peconic, Long Island. Wigner had a car and we drove out to Peconic (before July 19, 1939) and tried to find Dr. Moore's cabin. We drove around for about half an hour. We asked a number of people, but no one knew where Dr. Moore's cabin was. We were on the point of giving up and about to return to New

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4.18.61

York when I saw a boy of about seven or eight years of age standing at the curb. I leaned out of the window and I asked: "Say, do you by any chance know where Professor Einstein lives?" The boy knew and he offered to take us there, though he had never heard of Dr. Moore/s cabin.

This was the first Einstein heard about the possibility of a chain reaction. He was very quick to see the implications and perfectly willing to do anything that needed to be done. He was reluctant to write to the Queen of the Belgians but he thought he would write to one of the cabinet members of the Belgian Government whom he knew, and he was about to do just that when Wigner said that we should not approach a foreign government without giving the State Department an opportunity to object. So Wigner proposed that Einstein write the letter and send a copy to the State Department with a covering letter. Einstein should say in that covering letter that if we don't hear from the State Department within two weeks,

6. 1938 - Nov. 1940 (15)

Edited by L.S. from Stenorette II 4-18-61

then he will send the letter to Belgium. Having decided on this course, in principle, we returned to New York and Wigner left for California. (This goes to show how "green" we were. We did not know our way around in America, we did not know how to do business, and we certainly did not know how to deal with the Government.) I had, however, an uneasy feeling about the approach we had decided upon and I felt that I would need to talk to somebody who knew a little bit better how things are done. I then thought of Gustav ~~Sk~~ Stolper. He used to live in Berlin, where he had published a leading German economic journal and had been a member of the German parliament; now he was living as a refugee in New York. I went to see him and talked the situation over with him. He said that he thought that Dr. Alexander Sachs, who was economic adviser to the Lehman Corporation and who had previously worked for the New Deal, might be able to give us advice on how to approach the American Government and whether we should approach the State Department or some other Agency of the Government. He telephoned Dr. Sachs and I went to see him and I told him my story. (Before July 19 - first visit) Sachs said that if Einstein were to write ~~x~~ a letter to President Roosevelt, he would personally deliver it to the President, and that there was no use going to any of the agencies or Departments of the Government; this issue should go to the White House. This sounded like good advice, and I decided to follow it.

In the meantime, Teller arrived in New York and I asked Teller whether he would drive me out to Peconic. Teller and I went (between July 19 and Aug 2) to see Einstein and on this occasion we discussed with Einstein the ~~pxxxx~~ possibility that he might write a letter to the President. Einstein was perfectly willing to do this. We discussed what should be in this letter and I said I would draft it. Subsequently, I sent Einstein two drafts to choose from, a longer one and a shorter one.

We did not know just how many words we ~~xx~~ could expect

6. 1938 - Nov. 1940 (17). Edited by L.S. from Steno II 4.18.61

the President to read. How many words does the fission of uranium rate? So I sent Einstein a short version and the longer version; Einstein thought the longer version was better, and this is the version which he signed. The letter was dated August 2, 1939. I handed it to Dr. Sachs for delivery to the White House. (Aug 15, 1939)

(March 1939) Play 12

¶ Soon after we had discovered the ~~nuclear~~ neutron emission of uranium, Wigner came through New York and we met -- Fermi and I and Wigner met in the office of Dr. ~~Bigram~~ Pegram, head of the department -- and Wigner said that this is such a serious business that we cannot assume the responsibility for handling it; we must contact the government and inform the government. Wigner said that "Well, we can do that" -- he would call the new -- Charles Edison, who was (I think) the new Assistant Secretary of the Navy or Secretary of the Navy -- and told Charles Edison that Fermi would be in Washington the next day and would be glad to meet with a committee and explain such matters as might be of interest to the Navy.

So Fermi went there. He was received by a committee. He told in his cautious way the story of uranium and what potential possibilities were involved; but there the matter ended. I got an echo of this through Tuve. Ross Gunn ~~Rothgen (?)~~, who was an adviser to the Navy and who attended this conference, telephoned Tuve and asked him "Who is this man Fermi? What kind of a man is he? Is he a Fascist or what? What is he?" Nothing came of this.

In July, 1939 after I took a rather optimistic view of the possibility of setting up a chain ~~HHHHHHH~~ reaction between graphite and uranium, I approached ~~Rothgen~~ Ross Gunn again and told him that the situation does not look too bad; the situation, as a matter of fact, looked so good that we ought to experiment at a faster rate than we have done before; that we had no money for this purpose, and I wondered if the Navy could make any funds available. Afterward I had a letter in reply, in which ~~Rothgen~~ Ross Gunn explained that there ~~was~~ almost no way in which the Navy could support this type of research, but that if we got any results which might be of interest to the Navy, they would appreciate it if we would keep them informed. This was the second approach to the government.

L, 8l
12-8-

The letter of Einstein was dated August 2. 1939 August passed and nothing happened. September passed and nothing happened, and finally I got together with Teller and Wigner and we decided we'd give Sachs two more weeks, and if nothing happened we would use some other ~~chance to write off.~~ 2. However suddenly Sachs began to bestir himself and (we) received a phone call from him in October 1939 saying that he had seen the President and transmitted (on Oct 11) Einstein's letter to him, and that the President has appointed a committee under the chairmanship of Lyman J. ~~Bricks~~ Briggs as Director of the National Bureau of Standards, and other members of the committee were Colonel Adamson of the Army and ~~Admiral~~ Commander Hoover from the Navy. The committee was to meet on October 21, 1939 and ~~Bricks~~ Briggs wanted to know who else he should include. Well, I told Sachs that, apart from Wigner and me, I thought that Edward Teller

(Letter dated Oct 17, 1939)

ought to be invited because he lives in Washington and he could act as a liaison between us and the committee. This was done. In addition, Bri~~cks~~ggs invited Dr. Tu~~e~~ve. Dr. Tu~~e~~ve had to ~~work in~~ go to New York and so he suggested that Dr. Roberts sit in for him.

12-8-67

It was our ~~generous~~ general intention not to ask the government for money, but to ask only for the blessing of the government so that we then (with the blessing of the government) would go to foundations, raise the funds, and get some co-ordinated effort going. However, these things never go the way you have planned them.

After I presented the case and Wigner spoke, Teller spoke; and Teller spoke in two capacities. In his own name he strongly supported what I had said and what Wigner had said; then he said ~~that, having now~~ having spoken for himself, he will now speak for Dr. Tu~~e~~ve. Dr. Tu~~e~~ve could not attend the meeting, but he had visited New York and he had a discuss~~ed it~~ion with Fermi; and it is Dr. Tu~~e~~ve's opinion that at this time it would not be advisable -- "No", said Teller, "that's not what he said." He said it would not be possible to spend more money on this research than $15,000. We had no intention to ask for any money from the government at this point, but since the issue of money was injected the representative of the Army asked "How much ~~was it you would~~ money do you need?" And I said that all we need money for at this time is to buy some graphite; and the amount of graphite we ~~would~~ have to buy would cost about $2000. Well, I don't know: maybe a few experiments which would follow would raise the sum to $6000 -- in this order of magnitude. 12-7-67

At this point the representative of the Army started a rather longish tirade, and he told us that it ~~was hard~~ is naive to believe that we can make a significant contribution to defense by creating a new explosive. He said that if a new weapon is created, it usually takes two wars before one can know whether the weapon is any good or not; and then he explained rather laboriously that it is in the end 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; and he said in his high-pitched voice that it was very interesting for him to hear this. He always thought that weapons were very important and that this is what costs money and this is why the Army needs such a large appropriation; but he was very interested to hear that he was wrong -- it's not weapons but the morale which wins the wars -- and if this is correct, perhaps one should take a second look at the budget of the Army, and maybe the budget could be cut. Colonel Adamson wheeled around to look at Mr. Wigner and said, "Well, as far as those two thousand dollars are concerned, you can have it." This is how the first money promise was made by the government. ~~I should mention that, until the government showed~~

I should mention that, until the government showed interest (and the first interest it showed was the appointment of this committee), I was undecided whether this development ought to be carried by industry or whether it ought to be carried by the government; and so, just a week or two before the meeting in Washington, I had met with the Director of Research of the Union Carbon and Carbide Company, ~~S. W. Bennett~~ F. W. Barrett. The appointment was made by Strauss and there was some mix-up about it because they expected Fermi and it was I who turned up.

~~There were~~ I saw five people sitting around the table, and I told them that the possibility of a chain reaction between uranium and graphite must be taken seriously; and at this point we ~~could~~ can not say very much about this possibility; and that we could talk about it with much greater assurance if we first measured the absorption of neutrons in graphite. It was for this purpose that we would need about two thousand dollars' worth of graphite, and I wondered whether they might lend -- or give us this amount of graphite on loan; the experiment would not damage the graphite and we could return it to them.

~~S. W. Bennett~~ F. W. Barrett said, "You know, I'm a gambling man myself; but you are now asking me to gamble with the stockholders' money, and I'm not sure that

I can do ~~HH~~ that. What would be the practical applications of such ~~this~~ chain reaction?" And I said that I really can ~~could~~ not say what the practical applications would be at this point, that there is ~~was~~ very little doubt in my mind that such a revolution is phenomenal and will find its practical applications ultimately, but it is ~~was~~ too early to say that; we had first to see whether we can ~~could~~ get it ~~this~~ going, and under what conditions it can ~~could~~ be set up.

After I left the meeting I had an uneasy feeling that I did not convince anybody there. After all, I was a foreigner and my name was not so ~~very~~ well known; I was not well known as a physicist, certainly not to these people. So I sat down and I wrote a letter, in which I wrote to Mr. Jarrett ~~Bennett~~ that -- in which I invited him to ~~HHHWHHHHHHHHHHHHHHHHHHHHHHHHHHHH~~ lunch with Dr. Pegram ~~Bigram (?)~~, who was head of the department and dean of the graduate school, and Dr. Fermi (who after all was ~~won~~ the Nobel Prize winner and ~~from that~~ his name was quite well known) one day at his convenience the following week at Columbia University. He replied that the following week he would not be in town; he did not suggest an alternate date; and he wrote that they had decided that they would not be in a position to let us have any graphite except on a straight purchase basis. I remember ~~admit~~ that I was quite depressed by that letter, and showed it to Pegram ~~Bigram~~, who thought that I was too easily discouraged; and maybe I was.

The Washington meeting was followed by the most curious period in my life. We heard nothing from Washington at all; by the first of February 1940 there was still no word from Washington -- at least, none that reached me. I had ~~a feeling~~ assumed that once we had demonstrated that ~~HHHHHH~~ in the fission of uranium neutrons are emitted, there would be no difficulty in getting people interested; but I was wrong. Fermi didn't see any reason to do anything since we had asked for our money to buy graphite and since we hadn't yet gotten the money, and was interested in working on cosmic rays. I ~~Y~~myself waited for the ~~government~~ developments in Washington, and amused myself by making some more detailed calculations on the chain reaction of the graphite-uranium system.

It is an incredible fact in retrospect that between the end of June 1939 and the spring of 1940, not a single experiment was under way in the United States which was aimed at exploring the possibilities of a chain reaction in natural uranium.

Late in January or early in February of 1940 I received a reprint of a paper by Joliot in which Joliot investigated the possibilities of a chain reaction in a uranium-water system. In a sense this was a similar experiment to the one which Anderson, Fermi and I had carried out and published in June, 1939.

(cf. Play 14)

however, Joliot's experiment was made in a different set-up, and I was able to conclude from it ~~HHHWHWHW~~ what I was not able to conclude from our own experiment, namely, that the water-uranium system comes very close to being chain-reacting, even though it does not quite reach this point. However, it seemed to come so close to being chain-reacting that if we improved the system somewhat by replacing water with graphite, in my opinion we should have gotten over the hump.

I read Joliot's paper very carefully and made a number of small computations on it, and then I went to see Fermi, with whom I was no longer in daily contact because my function at Columbia had ceased. We had lunch together and Fermi told me that he was on the point of going to California. I asked him, "Did you read Joliot's paper?" ~~He~~ and said he ~~had~~ did, and I asked him "What did you think of it?" and Fermi said "Not much." At this point I ~~found~~ saw no reason to continue the conversation, and went home.

I then went to see Einstein again in Princeton, and told him that things ~~were~~ are not moving at all; and I said to Einstein that I think ~~that~~ the best thing I ~~could~~ can do now is to go definitely on ~~the~~ record that a graphite-uranium system would be chain-reacting by writing a paper on the subject and submitting it for publication to the Physical Review. I suggested that we re-open the matter with the government, and that we ~~purport~~ propose to take the position that I am going to publish my results unless the government asks me not to do so and unless the government is willing to take some action in this matter. Accordingly, I wrote a paper for publication and sent it to <u>Physical Review</u> on February 16; 1940 and I brought the paper over to ~~Bigram~~ Pegram, who was somewhat embarrassed because Fermi was out of town and ~~Bigram~~ Pegram did not know what action he should take. However he said that he must take some action, so he went to see Admiral Bowen in Washington, who ~~Bigram~~ Pegram thought might take some interest (because after all, atomic energy might be used for driving submarines).

On the basis of the conversation which I had with him ~~I shot over~~ Einstein wrote to Sachs, and Sachs wrote again to the President and the President replied that he thought that the best way to continue research would be to have another meeting of the uranium committee. And now something most tragic and comic happened.

Sachs ~~talked to Bricks~~ called up Briggs, having received a letter from the White House, and suggested a meeting be called; and ~~Bricks~~ Briggs said he was on the point of calling a meeting, and that he wanted to invite Sachs and Dr. ~~Bigram~~ Pegram to attend. And Sachs said, "Well, what about Szilard and Fermi?" and ~~Bricks~~ Briggs said, Well "You know, these matters are secret and we do not think that they should be included."

The fact of the matter is, Sachs blew up at this point, because this was after all? ~~his~~ meeting, and why should the people who are doing the job and do produce the figures not be included? This, however, was a ~~bit of~~ misunderstanding. Briggs ~~Bricks~~ did not want to call the meeting because he had heard from the White House; he wanted to call the meeting at the initiative ~~aegis~~ of Admiral Bowen, whom Pegram ~~Bigram~~ had contacted, so that Sachs and Briggs ~~Bricks~~ talked to each other to cross-purposes, they were talking ~~reporting~~ not about the same meeting but about different meetings. However, somehow things got straightened out and the meeting was called which Fermi and I did in fact ~~expect to~~ attend.

I Now ~~I~~ have to go back to the summer of 1939, when in July I made the first beginnings to compute the uranium-graphite system, and relate ~~there is~~ something that I forgot to relate earlier. ~~tell here.~~ As soon as I saw that the uranium-graphite system might work, I wrote a number of letters to Fermi telling him that I felt this is a matter of some urgency and we should not waste our time by making detailed physical measurements of the individual constants ~~interval constancy~~ involved, but rather try to get a sufficient amount of graphite and uranium to approach the critical mass and build up a chain-reacting ~~chain-reactioning~~ system. Fermi's response to this crash program was very cool: he said that he had thought of the possibilities of using carbon instead of water, that he had computed how a homogeneous mixture of carbon and uranium would behave, and that he found that the absorption of carbon would have to be indeed exceedingly low in order to make such a system chain-reacting. I knew very well that Fermi must be aware ~~XHX~~ of the fact that a homogeneous mixture of uranium and carbon is not as good as a heterogeneous uranium-carbon system; he computed the homogeneous mixture only because it was the easiest to compute. and This showed me that Fermi did not take this matter really seriously; and this was one of the factors which induced me to approach the government quite independently of whatever Fermi or Columbia University might feel.

In July, 1939 when I reported to ~~Bigram~~ Pegram my optimistic views about graphite, and told him why I thought the matter was urgent, he took the position that even though the matter appears to be rather urgent, this being summer and Fermi being away, there is really nothing that usefully could be done until the fall -- September, or perhaps October. This was the second factor which induced me to disregard everything else and go to the government directly.

Now, in the spring of 1940 we were advised that the money ($6000 which the committee promised us) was available; we bought some graphite, and Fermi started an experiment to measure the absorption of ~~this~~ the graphite. When he finished his measurement, the question of ~~figures~~ secrecy again came up. I went to his office and said that "now that we have this value, perhaps the value ought not to be made public." At this point Fermi really lost his temper -- at this point he really thought that this was absurd. There was nothing much more I could say, but next time when I dropped in his office he told me that Pegram had come to see him and Pegram thought that this value should not be published. From that point on, secrecy was ~~all.~~ on

end of 2nd take 36 min

Letter to Goldhaber (Nov '68) s.b.s. bk fldr 2

Sept 1939 — about having clean

in materials heavier than hydrogen

CORNELL UNIVERSITY
ITHACA, NEW YORK

DEPARTMENT OF PHYSICS
ROCKEFELLER HALL

30. Nov 1939

30/11 39.

Lieber Freund,

Dieses Geschmier ist auf den Umstand zurückzuführen, dass meine Füllfeder verschwunden ist, u. zw., wie ich vermute, im Bette des Zimmers 315. Vielleicht können Sie so freundlich sein, diesbezügliche Recherchen anzustellen und im positiven Falle das gefundene Objekt mir nach Indiana zusenden zu lassen. Ich werde Ihnen dann einen leserlichen Dankbrief schreiben können.

Vorliegende Gelegenheit kann ich gleich benützen, um Ihnen eine Formel für das $\overline{r^2}$ mitzuteilen, die für jede Masse gültig bleibt. Es ist

LM

$$\overline{r^2} = \frac{2Nl^2}{1-\cos\vartheta} \qquad (1)$$

Nun ist

$$N = \frac{\ln\frac{E_0}{E}}{\overline{\ln\frac{E_0}{E_1}}} = \frac{\ln\frac{E_0}{E}}{1+\frac{1-\alpha}{\alpha}\ln(1-\alpha)} \qquad (2); \quad \alpha = \frac{4M}{(M+1)^2}$$

(Mittelwert des $\ln\frac{E_0}{E}$ nach einem Stoss)

Maximaler Energieverlust.

Weiters ist der mittlere cosinus des Ablenkungswinkels

$$\overline{\cos\vartheta} = \frac{2}{3M} \qquad (3)$$

Somit hat man:

$$\overline{r^2} = \frac{6M}{(3M-2)\left\{1+\frac{1-\alpha}{\alpha}\ln(1-\alpha)\right\}} \cdot l^2 \ln\frac{E_0}{E} = f \cdot l^2 \ln\frac{E_0}{E} \qquad (4)$$

Für Wasserstoff ($M=1$) wird der Faktor f

$$f = 1,$$

für $M \gg 1$ wiederum

$$f = M,$$

wie es sein muss.

Für die Zwischenwerte folgende Tabelle:

	M	f
H	1	6
D	2	4,15
He	4	5,65
C	12	13,4
	$\gg 1$	M

Für C ist also der Grenzwert M schon eine gute Näherung. (13,4 gegen 12)

Je mehr ich mir übrigens das Uran überlege, desto wichtiger scheint mir es, die kombinierte <u>Bor-Selbstabsorptionskurve</u> zu machen, <u>bevor</u> man die Verlangsamung mit C oder D probiert, denn ~~auf~~ erst auf Grund dieser Kurve wird man imstande sein, die Versuchsbedingungen vernünftig zu wählen und im letzten Ende Zeit sparen.

Beste Grüsse Ihr G. Placzek

sbs bk SA

CORNELL UNIVERSITY
ITHACA, NEW YORK

DEPARTMENT OF PHYSICS
ROCKEFELLER HALL

9. Mai 1940.

Lieber Szilard,

Besten Dank für das Manuskript. Sie kriegen das Zusatzprodukt, sobald es getippt ist. Das Resultat ist sehr einfach: Die Anzahl der von einem schwarzen Detektor pro sec und Flächeneinheit absorbierten Neutronen*) ist:

$$\underline{I} = \frac{1}{8\pi}\frac{Q\, l_f}{\rho^3}\left\{1-\varphi\left(\frac{\rho}{L}\right)\right\}$$

L ≈

Die Funktion $\varphi\left(\frac{\rho}{L}\right)$, für die Sie eine Kurve bekommen, ist Null für $\frac{\rho}{L} = 0$ und z. B. 0,6 für $\rho = L$, und geht dann schnell gegen 1. L ist etwa 1 km, l_f bekanntlich ca 140 m. Für Ihre Genauigkeit rechnen Sie also einfach für mäßige Abstände ρ mit

$$\underline{I \sim \frac{1}{8\pi}\frac{Q\, l_f}{\rho^3}}$$

*) erzeugt von Quelle über fressendem Boden.

Das ist Ihnen hoffentlich einfach genug. Viel scheint es ja nicht zu sein. Ich weiss nicht wie Ihre Mordpläne sind, aber Sie sehen dass das um einen Faktor $\frac{\ell t}{\rho}$ weniger ist als der Effekt einer Quelle im Vakuum ohne jede Diffusion.

Beste Grüsse

Ihr

G. Placzek

sbs bk 8A

CORNELL UNIVERSITY
ITHACA, NEW YORK

DEPARTMENT OF PHYSICS
ROCKEFELLER HALL

July 25, 1940.

Dear Szilard,

Enclosed with my thanks your undamaged MS. I do not need it at present, but it may be practical if I could refer to it at several passages of one of my future papers. {You know how bad my memory is. Therefore I would be grateful if I could dispose of your paper again when I come to the writing up of the passages in question. In order to avoid extralegal procedure, we may consider it as this time not as paper any longer, but as „aide-memoire."}

Please let me know about Teller.

Best regards Yours G.P.

(*Reprinted from* NATURE, *Vol.* 143, *page* 471, *March* 18, 1939.)

Products of the Fission of the Uranium Nucleus

O. Hahn and F. Strassmann[1] have discovered a new type of nuclear reaction, the splitting into two smaller nuclei of the nuclei of uranium and thorium under neutron bombardment. Thus they demonstrated the production of nuclei of barium, lanthanum, strontium, yttrium, and, more recently, of xenon and cæsium.

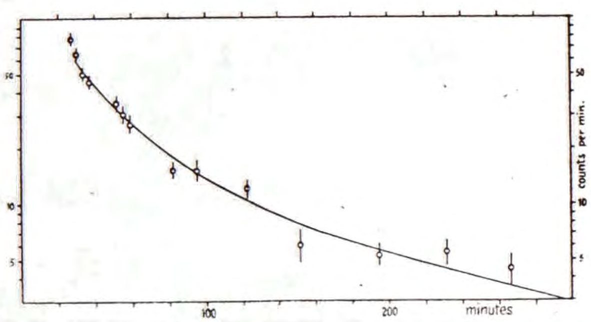
It can be shown by simple considerations that this type of nuclear reaction may be described in an essentially classical way like the fission of a liquid drop, and that the fission products must fly apart with kinetic energies of the order of hundred million electron-volts each[2]. Evidence for these high energies was first given by O. R. Frisch[3] and almost simultaneously by a number of other investigators[4].

The possibility of making use of these high energies in order to collect the fission products in the same way as one collects the active deposit from alpha-recoil has been pointed out by L. Meitner (see ref. 3). In the meantime, F. Joliot has independently made experiments of this type[5]. We have now carried out some experiments, using the recently completed high-tension equipment of the Institute of Theoretical Physics, Copenhagen.

A thin layer of uranium hydroxide, placed at a distance of 1 mm. from a collecting surface, was exposed to neutron bombardment. The neutrons were produced by bombarding lithium or beryllium targets with deuterons of energies up to 800 kilovolts. In the first experiments, a piece of paper was used as a collecting surface (after making sure that the paper did not get active by itself under neutron bombardment). About two minutes after interrupting the irradiation, the paper was placed near a Geiger-Müller counter with aluminium walls of 0·1 mm. thickness. We found a well-measurable activity which decayed first quickly (about two minutes half-value period) and then more slowly. No attempt was made to analyse the slow decay in view of the large number of periods to be expected.

The considerable intensity, however, of the collected activity encouraged us to try to get further information by chemical separations. The simplest experiment was to apply the chemical methods which have been developed in order to separate the 'transuranium' elements from uranium and elements immediately below it[6]. The methods had to be slightly modified on account of the absence of uranium in our samples and in view of the light element activities discovered by Hahn and Strassmann[1].

In these experiments, the collecting surface was water, contained in a shallow trough of paraffin wax. After irradiation (of about one hour) a small sample of the water was evaporated on a piece of aluminium foil; its activity was found to decay to zero. It was checked in other ways, too, that the water was not contaminated by uranium. To the rest of the water we added 150 mgm. barium chloride, 15 mgm. lanthanum nitrate, 15 mgm. platinum chloride and enough hydrochloric acid to get an acid concentration of 7 per cent. Then the platinum was precipitated with hydrogen sulphide, in the usual way; the precipitate was carefully rinsed and dried and then placed near our counter.



The results of three such experiments were found to be in mutual agreement. The decay of the activity was in one case followed for 28 hours. For comparison, a sample of uranium irradiated for one hour was treated chemically in the same way. The two decay curves were in perfect agreement with one another and with an old curve obtained by Hahn, Meitner and Strassmann under the same conditions. In the accompanying diagram the circles represent our recoil experiment while the full line represents the uranium precipitate. A comparison of the activity (within the first hour after irradiation) of the precipitate and of the evaporated sample showed that the precipitate contained about two thirds of the total activity collected in the water. After about two hours, however, the evaporated sample was found to decay considerably more slowly than the precipitate, presumably on account of the more long-lived fission products found by Hahn and Strassmann[1].

From these results, it can be concluded that the 'transuranium' nuclei originate by fission of the uranium nucleus. Mere capture of a neutron would give so little kinetic energy to the nucleus that only a vanishing fraction of these nuclei could reach the water surface. So it appears that the 'transuranium' periods, too, will have to be ascribed to elements considerably lighter than uranium.

In conclusion, we wish to thank Dr. T. Bjerge, Dr. J. Koch and K. J. Brostrøm for putting the high-tension plant at our disposal and for kind help with the irradiations. We are also grateful to Prof. N. Bohr for the hospitality extended to us at the Institute of Theoretical Physics, Copenhagen.

LISE MEITNER.

Physical Institute,
Academy of Sciences,
Stockholm.

O. R. FRISCH.

Institute of Theoretical Physics,
University,
Copenhagen.

March 6.

[1] Hahn, O., and Strassmann, F., *Naturwiss.*, **27**, 11, 89, and 163 (1939).
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SUR LA CAPTURE SIMPLE DES NEUTRONS THERMIQUES ET DES NEUTRONS DE RÉSONANCE PAR L'URANIUM

PAR

MM. Hans von HALBAN jun., Lew KOWARSKI
et Paul SAVITCH

(Extrait des *Comptes rendus des séances de l'Académie des Sciences*,
séance du 1er mai 1939.)

PHYSIQUE NUCLÉAIRE. — *Sur la capture simple des neutrons thermiques et des neutrons de résonance par l'uranium.* Note [1] de MM. **Hans von Halban** jun., **Lew Kowarski** et **Paul Savitch**.

Les travaux récents sur la partition nucléaire de l'uranium [2] et sur la libération de neutrons qui en résulte [3] ont fourni des renseignements quantitatifs dont la discussion exige la possession de données précises sur la capture simple, par l'uranium, des neutrons thermiques et des neutrons de résonance (d'énergie voisine de 25 volts) [4]. Nous rappelons que ce processus, tout à fait distinct de la capture conduisant à la partition, donne lieu à la formation d'un isotope de l'uranium (probablement ${}^{239}_{92}U$), qui se désintègre avec une période de 25 minutes en émettant des rayons β.

Nous avons cherché à déterminer la section efficace de l'uranium pour ce mode de capture des neutrons *thermiques*, en comparant l'activité produite dans l'uranium par une source déterminée de neutrons lents avec l'activité produite dans une feuille d'or par la même source et dans les mêmes conditions géométriques. On en déduit la valeur correspondante pour l'uranium d'après l'équation

$$\sigma_U^{th} = \sigma_{Au}^{th} \frac{I_U}{I_{Au}} \frac{\varepsilon_{Au}}{\varepsilon_U} \frac{A_U}{A_{Au}} \frac{m_{Au}}{m_U}$$

(I, activité d'une couche *infiniment épaisse* par rapport à la pénétration des rayons β, mesurée au compteur Geiger-Müller et ramenée à la fin

I

(1) Séance du 24 avril 1939.

(2) I. Curie et P. Savitch, *Journ. de Phys. et Radium*, 9, 1938, p. 355; O. Hahn et F. Strassmann, *Naturwiss.*, 27, 1939, p. 11; F. Joliot, *Comptes rendus*, 208, 1939, p. 341, et *Journ. de Phys. et Radium*, 10, 1939, p. 159; L. Meitner et O. Frisch, *Nature*, 143, 1939, p. 239; O. Frisch, *ibid.*, p. 276.

(3) H. von Halban jun., F. Joliot, L. Kowarski, *Nature*, 143, 1939, p. 470 et 680; M. Dodé, H. von Halban jun., F. Joliot, L. Kowarski, *Comptes rendus*, 208, 1939, p. 995; R. Roberts, R. Meyer, P. Wang, *Phys. Rev.*, 55, 1939, p. 510.

(4) L. Meitner, O. Hahn, F. Strassmann, *Z. f. Phys.*, 106, 1937, p. 249.

Det Kgl. Danske Videnskabernes Selskab.
Mathematisk-fysiske Meddelelser. **XV**, 10.

ON THE SLOWING DOWN AND CAPTURE OF NEUTRONS IN HYDROGENOUS SUBSTANCES

BY

O. R. FRISCH, H. v. HALBAN JUN.
AND JØRGEN KOCH

KØBENHAVN
LEVIN & MUNKSGAARD
EJNAR MUNKSGAARD
1938

MISE EN ÉVIDENCE D'UNE RÉACTION NUCLÉAIRE EN CHAINE AU SEIN D'UNE MASSE URANIFÈRE

PAR

H. HALBAN Jr, F. JOLIOT, L. KOWARSKI et F. PERRIN

Laboratoire de Chimie nucléaire (Collège de France).

3

EXTRAIT DE
« LE JOURNAL DE PHYSIQUE ET LE RADIUM »
OCTOBRE 1939. Série VII, T. X, N° 10, pp. 428-429.

MISE EN ÉVIDENCE D'UNE RÉACTION NUCLEAIRE EN CHAÎNE AU SEIN D'UNE MASSE URANIFÈRE

Par H. HALBAN Jr, F. JOLIOT, L. KOWARSKI et F. PERRIN.
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Sommaire. — Les expériences décrites dans cette Note donnent le nombre de neutrons produits dans une sphère d'oxyde d'uranium humide de 50 cm. de diamètre irradiée par une source de photoneutrons primaires. Une discussion de la valeur de ce nombre permet de conclure que les neutrons produits sont d'origine secondaire, tertiaire, etc., mettant en évidence, dans le système utilisé, la production de réactions en chaîne convergente.

Poursuivant l'étude de la libération des neutrons au cours de la rupture nucléaire de l'uranium [1], nous avons étudié la distribution de la densité des neutrons thermiques présents dans une masse à haute teneur d'uranium, irradiée par une source de photoneutrons (1).

Une sphère en cuivre (diamètre 50 cm), immergée dans un réservoir plein d'eau, peut être remplie d'eau ou de 300 kg de poudre de U^3O^8 (sèche ou additionnée d'une quantité variable d'eau). Une source de photoneutrons (1 g Ra + 160 g Be) est placée au centre de la sphère. La densité des neutrons présents dans la sphère est mesurée par l'activité I induite dans des détecteurs en dysprosium placés à l'intérieur de la sphère ou dans l'eau environnante. La courbe $Ir^2 = f(r)$, r étant la distance entre la source et l'un des détecteurs, a été tracée pour les cas suivants :

— sphère pleine d'eau;
— sphère pleine d'oxyde sec d'uranium;
— sphère pleine d'oxyde mouillé de façon à contenir 1 H pour 1 U;
— sphère pleine d'oxyde mouillé à 2 H;
— sphère pleine d'oxyde mouillé à 3 H.

Nous nous sommes assurés que la distribution de l'eau dans la poudre mouillée était suffisamment uniforme.

La forme des courbes obtenues présente des particularités intéressantes, notamment dans le cas 2 H, où deux maxima distincts sont visibles (pour $r = 14$ cm et $r = 28$ cm). Elle montre qu'il est illusoire de tirer des conclusions quantitatives de la détermination de la densité neutronique *à une seule distance* : les conclusions qu'on tirerait ainsi changeraient avec la distance choisie. L'intérêt principal réside dans la valeur numérique de la surface des courbes obtenues. En effet, cette valeur étant l'intégrale de la densité neutronique étendue sur un volume, la surface de la courbe est proportionnelle au nombre des neutrons thermiques présents à chaque instant dans le système. Rappelons que ce nombre est égal au produit $Q\tau$, Q étant le nombre de neutrons de toute origine qui, par unité de temps, sont ralentis jusqu'à l'état thermique dans le milieu, et τ la vie moyenne d'un neutron thermique, c'est à-dire le temps moyen pendant lequel il diffuse dans le milieu, en équilibre thermique, avant d'être absorbé. Le produit $Q\tau$ n'est réellement proportionnel à l'intégrale $\int Ir^2 dr$ qu'à condition d'employer un détecteur qui absorbe les neutrons thermiques suivant la loi $1/v$; les données expérimentales dont on dispose actuellement semblent montrer que le dysprosium suit cette loi d'assez près.

(1) Les résultats donnés ici correspondent à des expériences faites avant le 1er septembre 1939.

Le Tableau suivant résume les résultats numériques de nos expériences :

Teneur en eau du milieu (atomes H par atome U).	0 H.	1 H.	2 H	3 H.	eau pure.
Surface totale de la courbe (unité arbitraire)......	0,78	0,98	1,04	1,17	1,00
Surface correspondant à l'intérieur de la sphère.	0,21	0,37	0,57	0,72	–
Surface correspondant à l'extérieur de la sphère.	0,57	0,61	0,47	0,45	–

Nous essayerons de dégager le sens physique de ces données et plus particulièrement de celles concernant le cas 3 H. On peut écrire, notamment :

$$Q_{\text{int.}}\tau_{\text{int.}} = 0,72 \qquad Q_{\text{ext.}}\tau_{\text{ext.}} = 0,45.$$

$Q_{\text{int.}}$ et $Q_{\text{ext.}}$: nombre de neutrons qui, par unité de temps, deviennent thermiques dans le milieu intérieur (extérieur), dans notre expérience avec 3 H; $\tau_{\text{int.}}$ et $\tau_{\text{ext.}}$: vie moyenne d'un neutron thermique dans le milieu intérieur (extérieur), dans la même expérience.

Les unités de Q et τ sont choisies de façon à avoir $Q = 1$ et $\tau = 1$ dans notre expérience avec l'eau pure; dans ces conditions les surfaces indiquées dans le tableau ci-dessus sont numériquement égales aux produits $Q\tau$ correspondants.

Il faut, cependant, discuter l'influence des phénomènes d'échange de neutrons à la surface de discontinuité entre les deux milieux. Des neutrons *lents* ($E < 100$ eV) traversent cette surface dans les deux sens; un neutron absorbé dans un milieu n'a donc pas nécessairement passé toute sa vie de neutron *lent* dans ce même milieu. Cet effet, qui intéresse deux couches sphériques de faible épaisseur de part et d'autre de la surface de discontinuité, entraîne deux corrections de sens contraire sur les vies moyennes $\tau_{\text{int.}}$ et $\tau_{\text{ext.}}$ des neutrons observés dans cette région. Un calcul approximatif montre que la résultante de ces deux corrections peut être négligée.

Nous pouvons évaluer $\tau_{\text{int.}}$ en tenant compte des concentrations et des sections efficaces connues. Avec 3 H pour 1 U, en posant pour les neutrons thermiques

$$\sigma_{\text{cap. H}} = 0{,}28 . 10^{-24}\ \text{cm}^2, \qquad \sigma_{\text{rup. U}} = 2{,}5 . 10^{-24}\ \text{cm}^2,$$
$$\sigma_{\text{cap. U}} = 1{,}3 . 10^{-24}\ \text{cm}^2,$$

et en négligeant la capture par l'oxygène, nous avons $\tau_{\text{int.}} = 0{,}43$, d'où nous tirons $Q_{\text{int.}} = 1{,}67$. En y ajoutant $Q_{\text{ext.}} = 0{,}45$ (puisque $\tau_{\text{ext.}} = 1$), nous obtenons un total de 2,12. Ainsi, malgré le fait que la présence de l'uranium empêche, par absorption par résonance une partie notable des neutrons primaires de devenir thermiques, la production des neutrons thermiques dans le système est *plus que doublée* du fait de l'introduction de l'uranium.

Essayons de distinguer, dans le total observé de 2,12, l'apport des neutrons primaires (issus de la source) de celui des neutrons non primaires (issus de l'uranium). Tout neutron primaire a la probabilité $1 - \varphi$ de devenir thermique dans le milieu $U + H$ en ne tenant pas compte de l'absorption par résonance. Dans ce milieu, au cours du ralentissement, le neutron a la probabilité p d'être absorbé par résonance. Ainsi, seule la fraction $(1 - \varphi)(1 - p)$ arrive à l'état thermique à l'intérieur du milieu et parmi ces neutrons thermiques une fraction w seulement produit des ruptures (le reste étant absorbé par simple capture dans l'uranium et l'hydrogène). Ces ruptures produisent tous les neutrons non primaires, soit

$$Q_{\text{ext.}} - \varphi \quad \text{à l'extérieur}$$

et

$$\frac{Q_{\text{int.}} - (1 - \varphi)(1 - p)}{1 - p} = \frac{Q_{\text{int.}}}{1 - p} - 1 + \varphi \quad \text{à l'intérieur}$$

(la division par $1 - p$ est nécessaire pour remonter des neutrons *thermiques* observés à l'intérieur aux neutrons *lents* produits et demeurés à l'intérieur). Le quotient

$$\frac{Q_{\text{ext.}} + \dfrac{Q_{\text{int.}}}{1 - p} - 1}{w(1 - \varphi)(1 - p)}$$

représente donc le nombre total des neutrons issus d'une rupture produite par un neutron primaire.

La valeur numérique de w résulte immédiatement des sections efficaces et des concentrations données plus haut; elle est égale à 0,54.

La valeur de p dépend dans une certaine mesure (déterminée par la forme de la raie de résonance) de la teneur en uranium du milieu. Nous l'avons estimée en utilisant une série de mesures pour des concentrations plus faibles en uranium faites d'après une méthode décrite par ailleurs [2].

140 H pour 1 U : $p = 0{,}11 \pm 0{,}02$,
65 H pour 1 U : $p = 0{,}14 \pm 0{,}02$
(contre $0{,}16 \pm 0{,}025$ publié antérieurement),
30 H pour 1 U : $p = 0{,}20 \pm 0{,}02$.

Pour des raisons théoriques on peut s'attendre à ce que, pour des teneurs plus élevées en U, p croisse beaucoup plus lentement et qu'il soit voisin de 0,50 pour la teneur 3 H/1 U. Pour le calcul ci-dessus nous adopterons par prudence $p = 0{,}40$. Ce chiffre comprendra également la perte des neutrons par absorption à l'état semi-rapide qui ne doit pas être négligeable (*cf.* [3]), ainsi que le confirme d'ailleurs notre expérience avec l'oxyde sec (*voir* le Tableau ci-dessus).

La valeur de φ peut être évaluée en assimilant les 25 cm d'épaisseur d'oxyde mouillé à un certain équivalent d'eau pure. Pour calculer cet équivalent nous adoptons, pour les sections efficaces de diffusion élastique des neutrons, la valeur $13 . 10^{-24}\ \text{cm}^2$ pour $\frac{1}{2}\ H^2O$ et $15 . 10^{-24}\ \text{cm}^2$ pour $\frac{1}{3}\ U^3O^8$. Le milieu renfermant 0,42 g d'eau par centimètre cube, 25 cm de mélange équivalent à $25 \times 0{,}42 = 10{,}5$ cm d'eau. Cependant, la diffusion contre l'uranium conduit à un nombre de chocs contre l'hydrogène qui est d'un facteur $\left(13 + \frac{15}{3}\right)/13$ plus grand que dans un milieu contenant 0,42 g d'eau par centimètre cube sans autre centre diffusant. L'équivalent s'établit ainsi à 15 cm d'eau et la distribution bien connue de la densité des photoneutrons dans l'eau (*voir*, par exemple, [4]) montre que la proportion des neutrons devenant thermiques à des distances = 15 cm de la source est égale à 85 %, d'où $\varphi = 0{,}15$.

En introduisant les valeurs numériques de $Q_{\text{int.}}$, $Q_{\text{ext.}}$, w, p, φ dans l'expression ci-dessus, nous constatons qu'une rupture primaire produit un total d'au moins 8 neutrons non primaires. Comme il paraît difficile d'admettre que tous ces neutrons proviennent d'un noyau unique d'uranium (*cf.* la valeur de $3{,}5 \pm 0{,}7$ neutrons par rupture que nous avons annoncée précédemment), nous en concluons que *des ruptures secondaires, tertiaires, etc., ont eu lieu et que nous sommes en présence d'une réaction en chaîne convergente.*

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Physical Evidence for the Division of Heavy Nuclei under Neutron Bombardment

FROM chemical evidence, Hahn and Strassmann[1] conclude that radioactive barium nuclei (atomic number $Z = 56$) are produced when uranium ($Z = 92$) is bombarded by neutrons. It has been pointed out[2] that this might be explained as a result of a 'fission' of the uranium nucleus, similar to the division of a droplet into two. The energy liberated in such processes was estimated to be about 200 Mev., both from mass defect considerations and from the repulsion of the two nuclei resulting from the 'fission' process.

If this picture is correct, one would expect fast-moving nuclei, of atomic number about 40–50 and atomic weight 100–150, and up to 100 Mev. energy, to emerge from a layer of uranium bombarded with neutrons. In spite of their high energy, these nuclei should have a range, in air, of a few millimetres only, on account of their high effective charge (estimated to be about 20), which implies very dense ionization. Each such particle should produce a total of about three million ion pairs.

By means of a uranium-lined ionization chamber, connected to a linear amplifier, I have succeeded in demonstrating the occurrence of such bursts of ionization. The amplifier was connected to a thyratron which was biased so as to count only pulses corresponding to at least 5×10^5 ion pairs. About fifteen particles a minute were recorded when 300 mgm. of radium, mixed with beryllium, was placed one centimetre from the uranium lining. No pulses at all were recorded during repeated check runs of several hours total duration when either the neutron source or the uranium lining was removed. With the neutron source at a distance of four centimetres from the uranium lining, surrounding the source with paraffin wax enhanced the effect by a factor of two.

It was checked that the number of pulses depended linearly on the strength of the neutron source ; this was done in order to exclude the possibility that the pulses are produced by accidental summation of smaller pulses. When the amplifier was connected to an oscillograph, the large pulses could be seen very distinctly on the background of much smaller pulses due to the alpha particles of the uranium.

By varying the bias of the thyratron, the maximum size of pulses was found to correspond to at least two million ion pairs, or an energy loss of 70 Mev. of the particle within the chamber. Since the longest path of a particle in the chamber was three centimetres and the chamber was filled with hydrogen at atmospheric pressure, the particles must ionize so heavily, in spite of their energy of at least 70 Mev., that they can make two million ion pairs on a path equivalent to 0·8 cm. of air or less. From this it can be estimated that the ionizing particles must have an atomic weight of at least about seventy, assuming a reasonable connexion between atomic weight and effective charge. This seems to be conclusive physical evidence for the breaking up of uranium nuclei into parts of comparable size, as indicated by the experiments of Hahn and Strassmann.

Experiments with thorium instead of uranium gave quite similar results, except that surrounding the neutron source with paraffin did not enhance, but slightly diminished, the effect. This gives evidence in favour of the suggestion[2] that also in the case of thorium, some, if not all, of the activities produced by neutron bombardment[3] should be ascribed to light elements. It should be remembered that no enhancement by paraffin has been found for the activities produced in thorium[3] (except for one which is isotopic with thorium and is almost certainly produced by simple capture of the neutron).

Prof. Meitner has suggested another interesting experiment. If a metal plate is placed close to a uranium layer bombarded with neutrons, one would expect an active deposit of the light atoms emitted in the 'fission' of the uranium to form on the plate. We hope to carry out such experiments, using the powerful source of neutrons which our high-tension apparatus will soon be able to provide.

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Lz Lanz History A JWS

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Disintegration of Uranium by Neutrons: a New Type of Nuclear Reaction

On bombarding uranium with neutrons, Fermi and collaborators[1] found that at least four radioactive substances were produced, to two of which atomic numbers larger than 92 were ascribed. Further investigations[2] demonstrated the existence of at least nine radioactive periods, six of which were assigned to elements beyond uranium, and nuclear isomerism had to be assumed in order to account for their chemical behaviour together with their genetic relations.

In making chemical assignments, it was always assumed that these radioactive bodies had atomic numbers near that of the element bombarded, since only particles with one or two charges were known to be emitted from nuclei. A body, for example, with similar properties to those of osmium was assumed to be eka-osmium ($Z = 94$) rather than osmium ($Z = 76$) or ruthenium ($Z = 44$).

Following up an observation of Curie and Savitch[3], Hahn and Strassmann[4] found that a group of at least three radioactive bodies, formed from uranium under neutron bombardment, were chemically similar to barium and, therefore, presumably isotopic with radium. Further investigation[5], however, showed that it was impossible to separate these bodies from barium (although mesothorium, an isotope of radium, was readily separated in the same experiment), so that Hahn and Strassmann were forced to conclude that *isotopes of barium* ($Z = 56$) *are formed as a consequence of the bombardment of uranium* ($Z = 92$) *with neutrons*.

At first sight, this result seems very hard to understand. The formation of elements much below uranium has been considered before, but was always rejected for physical reasons, so long as the chemical evidence was not entirely clear cut. The emission, within a short time, of a large number of charged particles may be regarded as excluded by the small penetrability of the 'Coulomb barrier', indicated by Gamov's theory of alpha decay.

On the basis, however, of present ideas about the behaviour of heavy nuclei[6], an entirely different and essentially classical picture of these new disintegration processes suggests itself. On account of their close packing and strong energy exchange, the particles in a heavy nucleus would be expected to move in a collective way which has some resemblance to the movement of a liquid drop. If the movement is made

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sufficiently violent by adding energy, such a drop may divide itself into two smaller drops.

In the discussion of the energies involved in the deformation of nuclei, the concept of surface tension of nuclear matter has been used[7] and its value has been estimated from simple considerations regarding nuclear forces. It must be remembered, however, that the surface tension of a charged droplet is diminished by its charge, and a rough estimate shows that the surface tension of nuclei, decreasing with increasing nuclear charge, may become zero for atomic numbers of the order of 100.

It seems therefore possible that the uranium nucleus has only small stability of form, and may, after neutron capture, divide itself into two nuclei of roughly equal size (the precise ratio of sizes depending on finer structural features and perhaps partly on chance). These two nuclei will repel each other and should gain a total kinetic energy of *c*. 200 Mev., as calculated from nuclear radius and charge. This amount of energy may actually be expected to be available from the difference in packing fraction between uranium and the elements in the middle of the periodic system. The whole 'fission' process can thus be described in an essentially classical way, without having to consider quantum-mechanical 'tunnel effects', which would actually be extremely small, on account of the large masses involved.

After division, the high neutron/proton ratio of uranium will tend to readjust itself by beta decay to the lower value suitable for lighter elements. Probably each part will thus give rise to a chain of disintegrations. If one of the parts is an isotope of barium[5], the other will be krypton ($Z = 92 - 56$), which might decay through rubidium, strontium and yttrium to zirconium. Perhaps one or two of the supposed barium-lanthanum-cerium chains are then actually strontium-yttrium-zirconium chains.

It is possible[5], and seems to us rather probable, that the periods which have been ascribed to elements beyond uranium are also due to light elements. From the chemical evidence, the two short periods (10 sec. and 40 sec.) so far ascribed to ^{239}U might be masurium isotopes ($Z = 43$) decaying through ruthenium, rhodium, palladium and silver into cadmium.

In all these cases it might not be necessary to assume nuclear isomerism; but the different radioactive periods belonging to the same chemical element may then be attributed to different isotopes of this element, since varying proportions of neutrons may be given to the two parts of the uranium nucleus.

By bombarding thorium with neutrons, activities are obtained which have been ascribed to radium and actinium isotopes[8]. Some of these periods are approximately equal to periods of barium and lanthanum isotopes[5] resulting from the bombardment of uranium. We should therefore like to suggest that these periods are due to a 'fission' of thorium which is like that of uranium and results partly in the same products. Of course, it would be especially interesting if one could obtain one of these products from a light element, for example, by means of neutron capture.

It might be mentioned that the body with half-life 24 min.[2] which was chemically identified with uranium is probably really ^{239}U, and goes over into an eka-rhenium which appears inactive but may decay slowly, probably with emission of alpha particles. (From inspection of the natural radioactive elements, ^{239}U cannot be expected to give more than one or two beta decays; the long chain of observed decays has always puzzled us.) The formation of this body is a typical resonance process[9]; the compound state must have a life-time a million times longer than the time it would take the nucleus to divide itself. Perhaps this state corresponds to some highly symmetrical type of motion of nuclear matter which does not favour 'fission' of the nucleus.

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Joliot-Curie

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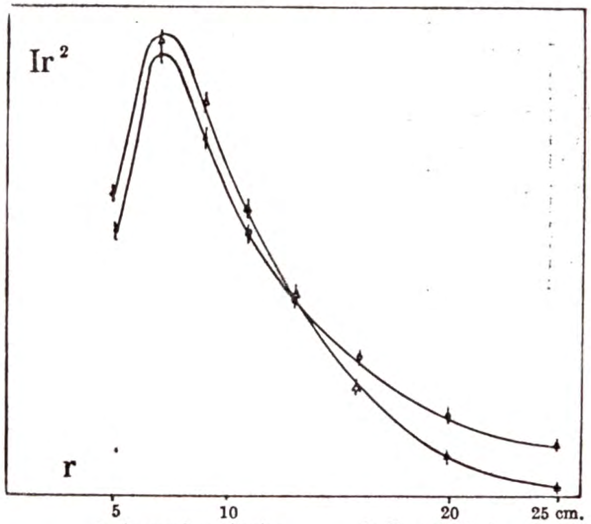
Liberation of Neutrons in the Nuclear Explosion of Uranium

RECENT experiments[1,2] have revealed the existence of a new kind of nuclear reaction: neutron bombardment of uranium and thorium leads to an explosion of the nucleus, which splits up into particles of inferior charge and weight, a considerable amount of energy being liberated in this process. Assuming a partition into two particles only, so that the nuclear mass and charge of uranium have to be distributed between two lighter nuclei, the latter contain considerably more neutrons than the heaviest stable isotopes with the same nuclear charges. (A splitting into, for example, ^{98}Rb and ^{141}Cs means an excess of 11 neutrons in the first, and of 8 neutrons in the second of these two nuclei.) There seem to be two possibilities of getting rid of this neutron excess. By the emission of a β-ray, a neutron is transformed into a proton, thus reducing the neutron excess by two units; in the example given above, five and four successive β-activities respectively would be needed to restore the neutron-proton stability ratio. In fact, the explosion products have been observed to be β-active and several periods have been recorded, so that a part, at least, of the neutron excess is certainly disposed of in this way. Another possible process is the direct liberation of neutrons, taking place either as a part of the explosion itself, or as an 'evaporation' from the resulting nuclei which would be formed in an excited state.

In order to find some evidence of this second phenomenon, we studied the density distribution of the thermal neutrons produced by the slowing-down of photo-neutrons from a Ra γ - Be source in a 1·6 molar solution of uranyl nitrate and in a 1·6 molar solution of ammonium nitrate (the hydrogen contents of these two solutions differ by only 2 per cent). Plotting Ir^2 as a function of r (where r is the distance between the source and a given point, and I is the local density of thermal neutrons at the same point, measured by the activity induced in a dysprosium detector), a curve is obtained the area of which is proportional to $Q.\tau$, Q being the number of neutrons per second emitted by the source or formed in the solution and τ the mean time a neutron spends in the solution before being captured[3,4]. Any additional nuclei, which do not produce neutrons, brought into the solution, will increase the chances of capture and therefore decrease τ and the area. If, however, these dissolved nuclei are neutron-producing, Q will be greater and the area of the curve will tend to increase. Evidence of neutron production, as indicated by an actual increase of the area, will only be obtained if the gain through Q (neutron production) is greater than the loss through τ (neutron capture). This loss can anyway be studied separately, since it has been shown[5] that the introduction of nuclei which act merely by capture or by increasing the hydrogen content of the solution can affect the shape of the density curve only in a characteristic way: the modified curve can always be brought to coincide with the primitive curve by multiplying all abscissæ by a suitable factor and all ordinates by another factor.



Δ, Ammonium nitrate O, Uranyl nitrate

The accompanying graph shows the two curves obtained. At small distances from the source the neutron density is greater in the ammonium solution than in the uranyl solution; at distances greater than 13 cm., the reverse is true. In other words, the decrease of the neutron density with the distance is appreciably slower in the uranyl solution.

The observed difference must be ascribed to the presence of uranium. Since the two curves cannot be brought to coincide by the transformation mentioned above, the uranium nuclei do not act by capture only; an *elastic* diffusion by uranium nuclei would have an opposite effect: it would 'contract' the abscissæ, instead of stretching them. The density excess, shown by the uranyl curve beyond 13 cm., must therefore be considered as a proof of neutron production due to an interaction between the primary neutrons and the uranium nuclei. A reaction of the well-known $(n,2n)$ type is excluded because our primary neutrons are too slow for such a reaction (90 per cent of Ra + Be photo-neutrons have energies smaller than 0·5 Mev. and the remaining 10 per cent are slower than 1 Mev.).

The degree of precision of the experiment does not permit us to attribute any significance to the small increase of the area in the uranyl curve (as compared to the ammonium curve), which we obtain by extrapolating the curves towards greater distances. In any event, an inferior limit for the cross-section for the production of a neutron can be obtained by assuming that the density excess due to this production is equal throughout the whole curve to the excess observed at $r = 25$ cm.; this limit, certainly inferior to the actual value, is 6×10^{-25} cm.[2].

Our measurements yield no information on the energy of the neutrons produced. If, among these neutrons, some possess an energy superior to 2 Mev., one might hope to detect them by a (n,p) process, for example, by the $^{32}S(n,p)^{32}P$ reaction. An experiment of this kind, Ra γ - Be still being used as the primary neutron source, is under way.

The interest of the phenomenon observed as a step towards the production of exo-energetic transmutation chains is evident. However, in order to establish such a chain, more than one neutron must be produced for each neutron absorbed. This seems to be the case, since the cross-section for the liberation of a neutron seems to be greater than the cross-section for the production of an explosion. Experiments with solutions of varying concentration will give information on this question.

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Number of Neutrons Liberated in the Nuclear Fission of Uranium

RECENT experiments have shown that neutrons are liberated in the nuclear fission of uranium induced by slow neutron bombardment: secondary neutrons have been observed which show spatial[1], energetic[2] or temporal[3] properties different from those which primary neutrons possess or may acquire. Such observations give no information on the mean number of neutrons produced per nucleus split; this number ν may be very small (less than 1) and the result of the experiment will still be positive.

We are now able to give information on the value of ν. Let us consider the curve representing the density distribution of neutrons slowed down in an aqueous solution surrounding a primary neutron source[1]; the area S of this curve is proportional to $Q.\tau$, Q being the number of neutrons per second emitted by the source or formed in the solution, and τ the mean time a neutron spends in the solution before being captured. Assuming that the solution contains only nuclei which absorb neutrons according to the $1/v$ law (the only exception to this rule will presently be dealt with), τ is proportional to $1/\Sigma c_i\sigma_i$, where c_i is the concentration (atom grams per litre) of an absorbing nucleus, σ_i its cross-section for the capture of neutrons of velocity 1 and the index i is extended to all kinds of neutron-absorbing reactions attributable to nuclei present in the solution. Substituting the symbol A_i for $c_i\sigma_i$ and A_{tot} for ΣA_i, we have identically:

$$\frac{\triangle S}{S} = \frac{\triangle Q}{Q} - \frac{\triangle A_{tot}}{A_{tot}}, \qquad (1)$$

neglecting all terms of higher orders, such as those containing $(\triangle Q)^2$, $\triangle Q.\triangle A_{tot}$, etc.

Let the symbol $\triangle$ stand for the differences observed between the two solutions (uranyl and ammonium) used in our previous experiment[1]. Neglecting $\triangle A_{tot}$ before A_{tot} introduces an ambiguity in the definition of A_{tot} (uranyl *vs.* ammonium value) which is numerically unimportant and can be reduced by adopting the arithmetical mean $(A_{tot}(\text{amm.}) + \triangle A_{tot})/2$.

In the quantity $\triangle A_{tot}$ the uranium nuclei are represented by several separate terms standing for the different modes of neutron capture (see below); let A_f be the term for the capture leading to fission. Every neutron has the probability A_f/A_{tot} of causing a fission and, since one individual fission process liberates ν neutrons on the average, the total number ΔQ of neutrons thus created is $Q.\frac{A_f}{A_{tot}}.\nu$, and the equation (1) can be rewritten as follows:

$$\nu = \frac{\Delta S}{S} \cdot \frac{A_{tot}}{A_f} + \frac{\Delta A_{tot}}{A_f}. \qquad (2)$$

Let us estimate the values of all quantities necessary to calculate ν according to this formula. The area variation $\Delta S/S$ can be read from the graph given in our previous letter with an error of less than 20 per cent (due to the uncertainties of inter- and extrapolation; in order to facilitate the latter, we added to the curves a further experimental point for $r = 29$ cm.). The value of A_{tot} for the ammonium solution can be easily calculated from the known concentrations and capture cross-sections (hydrogen, nitrogen and oxygen). A_f is equal to c_U (1·6 in our experiment), multiplied by the value of σ_f given in a recent paper by Anderson *et al.*[4]. ΔA_{tot} contains a term expressing the small difference of the hydrogen content between the two solutions; and three terms relative to uranium, namely, the fission term A_f, already dealt with, the thermal capture term A_{ct} which can be calculated by using a recently found value for σ_{ct} [5] and finally the resonance capture term A_r which requires some explanation.

Our reasoning assumed that all neutrons introduced into the solution spend practically all their life, and are absorbed, in the thermal state. This is true in so far as the $1/v$ law is valid for absorption of neutrons in all nuclei concerned; and, therefore, not wholly true for uranium, which shows a pronounced resonance capture of neutrons of about 25 volts[6]. A certain proportion of neutrons entering the solution is bound to come within this resonance band and to be absorbed by resonance; therefore, it will never reach the thermal state. This proportion depends on the width of the resonance band and on the concentration c_U; its value in our system of symbols is equal to A_r/A_{tot} and was numerically determined by an experiment reported elsewhere[5].

Putting all numerical values in the formula (2) (with 10^{-24} cm.[2] as the unit of cross-section), that is: $\Delta S/S = 0{\cdot}05 \pm 0{\cdot}01$; $A_{tot} = 36 \pm 3$; $A_f = 1{\cdot}6 \times 2 = 3{\cdot}2$; $\Delta A_{tot} = 8{\cdot}7 \pm 1{\cdot}4$ decomposable into $\Delta A_H = 1{\cdot}2 \pm 0{\cdot}1$, $A_{ct} = 1{\cdot}6 \times (1{\cdot}3 \pm 0{\cdot}45) = 2{\cdot}1 \pm 0{\cdot}7$, $A_r = 6{\cdot}4 \pm 1{\cdot}1$ and $A_f = 3{\cdot}2$, we find:

$$\nu = 3{\cdot}5 \pm 0{\cdot}7.$$

We were not able to allow for an error in A_f, since the value of σ_f given by Anderson *et al.* contains no indication of probable error. Any error in σ_f will affect $\nu - 1$ in an inversely proportional way; in any case ν will remain greater than 1.

The interest of the phenomenon discussed here as a means of producing a chain of nuclear reactions was already mentioned in our previous letter. Some further conclusions can now be drawn from the results reported here. Let us imagine a medium containing only uranium and nuclei the total neutron absorption of which, as compared to that of uranium, may be neglected (containing, for example, only some hydrogen for slowing down purposes). In such a medium, if $\frac{A_f}{A_{tot}} \cdot \nu > 1$ (A_{tot} includes now only uranium terms), the fission chain will perpetuate itself and break up only after reaching the walls limiting the medium. Our experimental results show that this condition will most probably be satisfied (the quantity $\frac{A_f}{A_{tot}} \cdot \nu - 1$, though positive, will be, however, small), especially if one keeps in view that the term A_r, because of the self-reversal of the resonance absorption line, increases much more slowly than the other uranium terms when the uranium content of the medium is increased.

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Control of the Chain Reaction involved in Fission of the Uranium Nucleus

IT has recently been shown that the number[1] of neutrons liberated[2] in the nuclear fission of a uranium nucleus is sufficiently high to make the realization of a self-perpetuating reaction chain seem possible. The danger that a system containing uranium in high concentration might explode, once the chain is started, is considerable. It is therefore useful to point out a mechanism which gives the possibility of controlling the development of such a chain.

We form an expression which is characteristic for the behaviour of the chain :

$$\nu'' = \frac{A_f}{A}\nu(1 - \alpha), \quad . \quad . \quad (1)$$

A_f being the product of the cross-section for nuclear fission for a thermal neutron of the uranium nucleus with the concentration of the uranium ; A_i the product of the absorption cross-section for thermal neutrons of the nucleus of kind i multiplied with its concentration ; A the sum of all A_i's, which is to be taken over all kinds of nuclei present in the solution ; ν is the average number of neutrons liberated in one fission, α the average probability for a neutron to diffuse out of the system before being absorbed.

The energy liberated by the chain will be

$$E = NF, \quad . \quad . \quad (2)$$

F being the energy liberated in one fission and N the number of fissions produced by the chain. We have

$$N = \nu'' + \nu''^2 + \nu''^3 + \ . \ . \ . \ . \ . \ .(3)$$

The chain gives thus a quantity of energy, which is increasing rapidly with time, if ν'' is greater than 1. Let us consider the case of a chain which is due to fission produced by thermal neutrons; that is, a chain propagating itself in a system containing sufficient hydrogen for the slowing down of the neutrons.

If the cross-sections for capture or fission of all nuclei present follow the $1/v$ law, ν'' will not depend on the velocity of the neutrons and therefore not on the temperature of the system (since α will in practice be small and since it depends in the first place on the distance necessary for slowing down the neutron ; the temperature has, of course, an effect, although it will be very small).

Let us, however, introduce an absorbent, such as cadmium, the cross-section of which does not depend on the neutron energy in the thermal region. We will have, instead of (1),

$$\nu'' \doteq \nu \frac{A_f}{A' + A_c}(1 - \alpha), \quad . \quad . \quad . \quad (4)$$

where A' is the sum of all A_i's following the $1/v$ law and A_c is a constant, the term due to the newly added absorbent. ν'' will now decrease with increasing temperature. At a temperature, which will be characteristic for the composition and the geometrical constants of the system, ν'' will become smaller than unity and the system will stabilize itself somewhere near this temperature; the equilibrium being determined by the fact that the amount of energy given out per unit of time by the system (in the form of heat and nuclear radiation) is equal to the energy produced by the system. Similar questions have been discussed by F. Perrin[3].

Added in proof: In the case of a chain propagating itself by thermal neutrons, the time necessary for the slowing down and for the absorption of a neutron, that is, its mean life, is of the order of 10^{-4} sec. If one makes ν'' as small as 1007, it needs 100 times the mean life of a neutron or about 10^{-2} sec. to double the number of neutrons, and with that the energy liberated per unit of time. It is therefore possible to control the development of the chain by a periodical interaction of absorbers which break up the chains by entering the system.

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