One of the last books which I read before I left Berlin was "The World Set Free" by H. G. Wells. This book, which was published in 1933, predicted the liberation of atomic energy on an industrial scale and the development of the atomic bomb. It did not occur to me when I read it that I was reading a prophesy that might come true. But while strolling through the streets of London in the Fall of 1933, I was pondering upon the statement made by Lord Rutherford who said that "he who talks about the large scale liberation of atomic energy is talking moonshine"; it suddenly occurred to me how in certain circumstances it might become possible to set up a nuclear chain reaction, liberate energy on an industrial scale and construct atomic bombs. The thought that this might be, in fact, possible became a sort of obsession with me. It lead me to go into nuclear physics, a field in which I have not worked before, and the thought stayed with me even though my first hunches in this regard turned out to be wrong.

When the German troops moved into the Rhineland and England advised France against invoking the Locarno Pact, I knew that there would be war in Europe and I came to America at the end of 1937 under an arrangement which permitted me to divide my time between America and Europe.

5. England 1933 - 1938

Stenorette I
May 6, 1960 interview
(Corrected Dec 1967)



I reached the conclusion something will go wrong in Germany very early. I reached this conclusion in 1930, and the occasion was a meeting in Paris. It was a meeting of economists who were called together to decide whether Germany can pay reparations, and just how much reparations Germany can pay. One of the participants of that meeting was Dr. Hjalmar Schacht, who was at that time, I think, president of the German Reichsbank, and he, to the surprise of the world, including myself, took the position that Germany cannot pay any reparations unless she gets back her former colonies. This was such a frightening statement to make that it caught my attention, and I concluded that if Hjalmar Schacht believes that he can get away with this, things must look rather bad. I was so impressed by this that I wrote a letter to my bank and transferred every single penny I had out of Germany into Switzerland. I was not the only one, as I later learned, but within a few months after this speech of Schacht's, a very large sum of money, mainly by depositors from abroad, was drawn out of Germany. Apparently there are many people who are sensitive to this kind of signal.

I visited America in 1931 - I came here in December - Christmas Day, 1931, on the "Leviathan," as a matter of fact, and stayed here until - oh, I stayed here for about three months, and in the course of 1932 I returned to Berlin where I was privat-dozent at the University. Hitler came into office, I believe in January '33, and I had no doubt what will happen. I lived in the faculty club of the Kaiser Wilhelm Institute in Berlin Dahlem and I had my suitcases packed, and by this I mean that I had literally two suitcases standing in my room, which were packed; the key was in it and all I had to do was turn the key and leave when things got too bad. I was there when the Reichstag

Stayed in U.S. until May 4, 1932.

brand occurred, and I remember how difficult it was for people there to understand what was goin on. A friend of mine, Michael Polanyi, who was director of a division of the Kaiser Wilhelm Institute for Physical Chemistry, took like many other people a very optimistic view of the situation. They all thought that civilized Germans would not stand for anything really rough happening. The reason that I took the opposite position was based on observations of rather small and insignificant things. What I noticed was that the Germans always took a utilitarian point of view. I asked, well, suppose I would oppose this, thinking, what good would I do? I wouldn't do very much good, I would just lose my influence. Then why should I oppose it? You see, there was no - the moral point of view was completely absent, or very weak, and every consideration was simply consideration of what would be the predictable consequence of my action. And on that basis did I reach in 1931 the conclusion that Hitler will get into power, not because the forces of the Nazi revolution were so strong, but rather because I thought that there would be no resistance whatsoever.

Well, after the Reichtag fire, I went to see my friend Michael Polanyi who was in the Kaiser Wilhelm Institute for Physical Chemistry, and told him what had happened, and he looked at me and he said: "Do you really mean to say that you think that the Secretary of the Interior had anything to do with this?" and I said, "Yes, this is precisely what I mean," and he just looked at me with incredulous eyes. He had at that time an offer to go to England and to accept a professorship in Manchester. I very strongly urged him to take this, but he said that if he now went to Manchester, at least for another year he could not be productive, because it takes that much in order

Notes to p. 2.

Reichstag fire Feb. 27, 1933

Namier

to install a laboratory, and I said to him: "Well, how bng do you think you will remain productive if you <u>stay</u> in Berlin?" We couldn't get together on this so I finally told him that if he must refuse this offer he should refuse it on the ground that his wife is opposed to it, because his wife always can change her mind and so if he wanted to have the thing reconsidered, he would have an out. Later on when I was in England - this must have been - ja - the middle of '33, I was active in a committee, this was a Jewish committee, incidentally, this particular committee, where they were concerned about finding positions for refugees from Germany, and Professor Namier (?) came from Manchester and reported that Polanyi is now again interested in accepting a professorship in Manchester. He said that previously he had refused the offer extended to him on the ground that he was suffering from rheumatism, but it appears that Hitler cured his rheumatism.

I left Germany a few days after the Reichstag fire, but how quickly things move you can see from this - I took a train from Berlin to Vienna on a certain date - I don't know the precise date, but close to the first of April, 1933. The train was empty. The same train, on the next day, was overcrowded, was stopped at the frontier. The people had to get out and everybody was interrogated by the Nazis. This just goes to show that if you want to succeed in this world you don't have to be much cleverer than other people, you just have to be one day earlier than most people. This is all that it takes.

While I was in Vienna there were the first people dismissed from German universities, just two or three, and it was however quite clear

British Historians

what will happen. I met, walking in the street, by pure chance, a colleague of mine. He was an economist at Heidelberg, Dr. Jacob Marschak, who is now a professor at Yale; he also was rather sensitive, not being a German, but coming from Russia he had seen revolutions and upheavals and he went to Vienna where he had relatives because he wanted to see what was going to happen in Germany. I told him that I thought that since we are up there we may as well make up our minds what needs to be done to take up this lot of scholars and scientists who will have to leave Germany and the German universities. He said that he knew a rather wealthy economist in Vienna who might have some advice to give. His name was Schlesinger and he had a very beautiful apartment in the Liechtensteinpalais. We went to see him and he said, yes, it is quite possible that there will be wholesale dismissals from German universities, why don't we go and discuss this with Professor Jastrow? Professor Jastrow was an economist mainly interested in the history of prices, I believe, and we went to see the Professor - the three of us now - and Jastrow said: "Yes, yes, this is something one should seriously consider," and then he said, "You know, Sir William Beveridge is at present in Vienna. He came here to work with me on history of prices, and perhaps we ought to talk to him." So I said: "Where is he staying?" and he said: "He's staying at the Hotel Regina." It so happened that I was staying at the Hotel Regina so I volunteered to look up Sir William Beveridge and try to get him interested in this. I saw Beveridge and he immediately said that he had already heard about dismissals, at the London School of Economics, and he was already taking steps to take on one of them and that he was all in favor of doing something in England to receive those who have to leave German universities. So I phoned Schlesinger and suggested that he invite him to dinner. Schlesinger said no, he wouldn't invite him to dinner because Englishmen, if you invite them to dinner, get very conceited. However, he would invite him to tea. So we had tea and in this brief get-together, Schlesinger and Dr. Marschak and Beveridge, it was agreed that Beveridge will, when he gets back to England, and when he gets his most important things which he had on the dockets out of the way, he will try to form a committee which will set itself the task of finding places for those who have to leave German universities; and he suggested that I come to London and that I occasionally prod him on this, and that if I prod him long enough and frequently enough, he thought he would do it. Soon thereafter he left, and soon after he left, I left and went to London.

When I came to London I phoned Beveridge. Beveridge said that his schedule has changed and that he finds that he is free and that he can at once take up this job, and this is the history of the birth of the so-called Academic Assistance Council in England. The English adopted a policy of mainly helping the younger people, but did not demand that somebody should have an established name or position in order to find a position in England. Quite in contrast to American organizations. In addition to the Academic Assistance Council, there was a Jewish committee functioning. They raised funds privately and they found positions for people and provided them with fellowships for one or two years. The two committees worked very closely together, and in a comparatively short time practically everybody who came to England had a position, except I.

When I got to England, and after I had no longer to function in connection with placing the scholars and scientists who left the

German universities, when this was more or less organized and there was no need for me to do anything further about that, I was thinking about what I should do, and I was strongly tempted to go into biology. I went to see A.V. Hill and told him about this. Now A.V. Hill himself had been a physicist and became a very successful biologist, and he thought it was quite a good idea. He said, "Why don't we do it this way? I'll get you a position as a demonstrator in physiology, and then 24 hours before you demonstrate you read up these things, and then you should have no difficulty to demonstrate them, the next day. In this way, by teaching physiology, you would learn physiology and it's a good place to begin."

And now I must tell you why I did not make this switch at the time. In fact, I made the switch to biology in 1946. In 1932 while I was still in Berlin, I read a book by H.G. Wells. It was called "The World Set Free." This book was written in 1913, one year before the World War, and in it H.G. Wells describes the discovery of artificial radioactivity and puts it in the year of 1933, the year in which it actually occurred. He then proceeds to describe the liberation of atomic energy on a large scale for industrial purposes, the development of atomic bombs, and a world war which was fought by apparently allies of England, France, and perhaps including America, against Germany and Austria, the powers located in the central part of Europe. He places this war in the year 1956, and in this war the major cities of the world are all destroyed by atomic bombs. Up to this point the book is exceedingly vivid and realistic. From this point on the book gets to be a little, shall I say, utopian. With the world in

Notes to page 6

Wells, Herbert George. The World Set Free: a Story of Mankind. London, McMillan, 1914.

shambles, a conference is called in Brisgago in Italy, in which a world government is set up.

This book made a very great impression on me, but I didn't regard it as anything but fiction. It didn't start me thinking whether or not such things could in fact happen. I had not been working in nuclear physics up to that time. Now, this really doesn't belong here, but I will nevertheless tell you of a curious conversation which I had also in 1932 in Berlin. The conversation was with a very interesting man, named Otto Mandl, who was an Austrian and who became a wealthy timber merchant in England, and whose main claim to fame is that he had discovered H.G. Wells at the time when none of his works had been translated into German. He went to H.G. Wells and acquired the exclusive right to publish his works in German, and this is how H.G. Wells became known on the Continent. In 1932 something went wrong with his timber business in London, and in 1932 he found himself again in Berlin. I had met him in London and I met him again in Berlin and there ensued a memorable conversation. Otto Mandl said that not only he thinks, he knows what it would take to save mankind from a series of ever-recurring wars that could destroy it. He said that man has a heroic streak in himself. Man is not satisfied with a happy idyllic life. He has the need to fight and to encounter danger. And he concluded that what mankind must do to save itself is to launch an enterprise aimed at leaving the earth. On this start he thought the energies of mankind could be concentrated and the need for heroism could be satisfied. I remember very well my own reaction. I told him that this was somewhat new to me, and that I really don't know whether I would agree with him. The only thing I can say is this: that if I came to the conclusion that this is what mankind needs, and if I wanted to contribute something to save mankind, then I would probably go into nuclear physics, because only through the liberation of atomic energy can we obtain the means which would enable man not only to leave the earth but to leave the solar system.

3-20-68
Edited for M.I.T. + Boyle

Andersen starts here
3-14-68

I was no more thinking about this conversation or about H.G. Wells's book either, until I found myself in London about the time of the British Association, which must have been August or September or October 1933. I read in the newspapers a speech by Lord Rutherford. Lord Rutherford was quoted to say that he who talks about the liberation of atomic energy on an industrial scale is talking moonshine. This sort of set me pondering as I was walking the streets of London, and I remember that I stopped for a red light at the intersection of Southampton Road, as I was waiting for the light to change; and as the light changed to green and I crossed the street, it suddenly occurred to me that if we could find an element which is split by neutrons and which would emit <u>two</u> neutrons when it absorbed <u>one</u> neutron, such an element, if assembled in sufficiently large mass, could sustain a nuclear chain reaction. I didn't see at the moment just how one would go about finding such an element, or what experiments would be needed, but the idea never had left me, and soon thereafter, when the discovery of artificial radioactivity was announced, a discovery made by Joliot and Mme. Joliot, I suddenly saw that tools were at hand to explore the possibility of such a chain reaction. I talked to a number of people about this.

Notes to p. 8

Meeting of the British Association for the Advancement of Science, Leicester, September 11, 1933. A review of the speech by Rutherford, in Nature 132:432-33 (Sept. 16, 1933) contains the sentence:

> "One timely word of warning was issued to those who look for sources of power in atomic transmutations - such expectations are the merest moonshine."

A quote from the speech is given on page 374 of the book: Rutherford, being the Life & Letters of the Rt. Hon. Lord Rutherford, O.M., by A.S. Eve, Cambridge University Press, 1939.

> These transformations of the atom are of extraordinary interest to scientists but we cannot control atomic energy to an extent which would be of any value commercially, and I believe we are not likely ever to be able to do so. A lot of nonsense has been talked about transmutation. Our interest in the matter is purely scientific, and the experiments which are being carried out will help us to a better understanding of the structure of matter.

# RUTHERFORD

*Being the Life and Letters of the*
*Rt Hon. Lord Rutherford, O.M.*


By

A. S. EVE, C.B.E., D.Sc., LL.D., F.R.S.

*formerly Macdonald Professor of Physics*
*McGill University*


With a foreword by

EARL BALDWIN OF BEWDLEY, K.G.


CAMBRIDGE
AT THE UNIVERSITY PRESS
1939

here the last two years in connection with transmutation. I find the position very similar to that in the old days in Montreal when every week brought a new development. Professor Lewis of California sent me a sample of his water rich in $H^2$ isotope and we have used it in our work. We find we can easily detect the transmutation effect of 60,000 or 70,000 volts. We have confirmed some of the results found by Lewis in California in connection with lithium, but have examined the matter in more detail with some interesting results. We find that lithium under proton bombardment in addition to the main range of 8·4 cm. gives two short additional groups of ranges about 7 and 12 millimetres; we do not yet know the interpretation thereof. Walton and Dee have been taking Wilson photographs of the effects and have confirmed the results which we hope to publish in the September Proc. Roy. Soc.

Work on the positive electron goes on rapidly and it looks as if the positive and negative electrons can be born in the strong nuclear field probably under varied conditions, e.g. gamma-rays, neutrons or electrons of sufficient energy.

At the Meeting of the British Association at Leicester, Rutherford described the recent great developments in physics and chemistry. He recalled that at Leicester in 1907 Lord Kelvin had declared the atom to be an indestructible unit, eternal in its nature. Since that time very many transmutations had been effected by deliberate experiment, but he added:

These transformations of the atom are of extraordinary interest to scientists but we cannot control atomic energy to an extent which would be of any value commercially, and I believe we are not likely ever to be able to do so. A lot of nonsense has been talked about transmutation. Our interest in the matter is purely scientific, and the experiments which are being carried out will help us to a better understanding of the structure of matter.

Primordia quaerere rerum!

On Tuesday evening, 3 October, there was a great meeting of 10,000 people at the Albert Hall when Rutherford made the opening speech from the chair:

I remember that I mentioned this to G.P. Thompson and I mentioned it to Blackett, but I couldn't evoke any enthusiasm.

I had one candidate for an element which might be instable in the sense of splitting off neutrons when it disintegrates, and that was beryllium. The reason that I suspected beryllium of being a potential candidate for sustaining a chain reaction was the fact that the mass of beryllium was such that it could have disintegrated into two other particles and a neutron. So, it was conceivable, it was not clear why it didn't do this spontaneously - why it did not disintegrate spontaneously - since the mass was large enough to do that; but it was conceivable that it had to be tickled by a neutron which would shake the beryllium nucleus in order to trigger such a disintegration. I remember that I told Blackett that what we ought to do is really get a large mass of beryllium, large enough to be able to notice whether it can sustain a chain reaction. Beryllium was very expensive at the time, almost not obtainable, and I remember Blackett's reaction was, "Look, you will have no luck with such fantastic ideas in England. Yes, perhaps in Russia. If a Russian physicist went to the government and said, we must make a chain reaction, they would give him all the money and facilities which he would need. But you won't get it in England." As it turned out later, beryllium cannot sustain a chain reaction and it is, in fact, stable. What was wrong was that ~~a~~ the published mass of helium was wrong. This was later on discovered by Bethe, and it was a very important discovery for all of us, because we did not know where to begin to do nuclear physics if there could be an element which could disintegrate but doesn't.

Well, when I gave up the beryllium I did not give up the thought that there might be another element which can sustain a chain reaction.

And in the spring of 1934 I had applied for a patent which described the laws governing such a chain reaction. It was for the first time, I think, that the concept of critical mass was developed and that a chain reaction was seriously discussed. Knowing what this would mean - and I knew it because I had read H.G. Wells - I did not want this patent to become public. The only way to keep it from becoming public was to assign it to the government. So I assigned this patent to the British Admiralty.

1 quot

At some point I decided that the reasonable thing to do was to investigate systematically all the elements. There were 92 of them. But of course this is a rather boring task, so I thought that what I will do, I will get some money, have some apparatus built, and then hire somebody who would just sit down and go through one element after the other. The trouble was that none of the physicists had any enthusiasm for this idea of a chain reaction. I thought, there is after all something called "chain reaction" in chemistry. It doesn't resemble a nuclear chain reaction, but still it's a chain reaction. So I thought I will talk to a chemist, and I went to see Dr. Chaim Weizmann, Professor Chaim Weizmann, who was a renowned chemist and the Zionist leader, and I had met him on one occasion or another. And Weizmann listened and Weizmann understood what I told him, and he said "How much money do you need?" and I said that I thought that 2,000 pounds would be enough to do this, which would have been at that time about 10,000 dollars. So Weizmann thought that he will try to get this money. I didn't hear from him for several weeks, but then I ran into Michael Polanyi, who by that time had arrived in Manchester and was head of the chemistry department there. And Polanyi told me that Weizman had come and talked to him about my ideas for the possibility of a chain reaction, and he wanted to know Polanyi's advice, whether he should get me this money. And Polanyi thought, that he thinks, this experiment ought to be done. And then again he didn't hear anything. As a matter of fact, I haven't seen Weizmann then until the late fall of '45, after Hiroshima. I was at that time in Washington and I ran into him in the Wardman-Park Hotel. He seemed to be terribly happy to see me, and he said "Do you remember when you came to see me in London?" I said "Yes." He said "And do you remember what you wanted me to do?" I said "Yes," and he said, "Well, maybe you won't believe me, but I tried to get those 2,000 pounds and found that I couldn't."

Because of these thoughts about the possibility of the chain reaction, and because of the discovery of artificial radioactivity, physics became too exciting for me to leave physics. So I decided not to go into biology as yet, but play around a little bit with physics, and I spent several months in the spring - in the early spring - throughout the spring, at the Strand Palace Hotel, doing nothing but dreaming about experiments which one could do, utilizing this marvelous tool of artificial radioactivity which Joliot has discovered. I didn't do anything; I just thought about these things. I remember that I went into my bath - I didn't have a private bath, but there was a bath in the corridor in the Strand Palace Hotel - around nine o'clock in the morning . There is no place as good to think as the bathtub. I was just soaking there and thinking, and around twelve o'clock the maid would knock and say, "Are you all right, sir?" Then I usually got out and made a few notes, dictating a few memoranda, and I played around this way doing nothing and the summer came around. At that time, I thought that one ought to try to learn something about beryllium, and I thought that if beryllium is really so easy to split, the gamma rays of radium should split it and it should split off neutrons.

I had casually met the director of the physics department of St. Bartholomew's Hospital, so I dropped in for a visit and asked him whether in the summer time, when everybody is away, I couldn't at that time use the radium, which is not much in use in summer, for experiments of this sort. And he said, yes, I could do this, but since I'm not on the staff of the hospital, I should team up with somebody on his staff. There was a very nice young Englishman, Mr. Chalmers, who was game, and so we teamed up and the next two months we did experiments. It turned out that in fact beryllium splits off neutrons when exposed to the gamma rays of radium. This later on became really very important because these neutrons are slow neutrons, and therefore if they disintegrate elements like uranium - of course we didn't know that until after the discovery of Hahn - and if in that process neutrons come off, which are fast, you can distinguish them from neutrons of the source, which are slow.

We did essentially two experiments. We demonstrated that beryllium emits neutrons if exposed to the gamma rays of radium, and we demonstrated something else, which is called the Szilard-Chalmers effect, which I will not describe now. But anyway, these experiments established me as a nuclear physicist, not in the eyes of Cambridge, but in the eyes of Oxford.

There was an International Conference on Nuclear Physics in London in September, where these two discoveries were discussed by the participants and so I got very favorable notice; and this led within six months to an offer of a fellowship at Oxford. However, I didn't get this offer until I had left England and came to America, where I didn't have a position but had some sort of fellowship; and when I received this offer from Oxford, I had the choice of either keeping on this fellowship in America or returning to Oxford. I sat down and wrote a letter to Michael Polanyi in which I described my choice between these two alternatives, and this is what I wrote him: that I would accept the fellowship at Oxford and go to England and I would stay in England, I wrote, until one year before the war, at which time I would shift my residence to New York City. That was very funny, because how can anyone say what he will do one year before the war? So the letter was passed around and a few people commented on it when I finally turned up in England.

However, this is precisely what I did. In 1937 I decided that the time has come for me to change my full-time fellowship at Oxford to one which permitted me to spend six months out of the year in America. And on the basis of that arrangement - I had to take a cut of salary, of course I had to go on half pay, so my total income amounted to $1,000 a year - I came over to America, ~~and I~~

Anderson
3-14-68

Notes to p. 13

International Conference on Physics, London, 1934. Papers and discussions in two volumes. Cambridge, The Physical Society, 1935.

V. 1. Nuclear Physics. pp. 88-89.

Szilard - Chalmers: Phy 5, 6, 7, 8
British Association - Lord Rutherford

→ Tapes Master

# International Conference on Physics

London 1934

A JOINT CONFERENCE ORGANIZED
BY THE INTERNATIONAL UNION OF PURE
AND APPLIED PHYSICS
AND
THE PHYSICAL SOCIETY

# PAPERS & DISCUSSIONS

In two volumes

## VOL. I. NUCLEAR PHYSICS

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1935

A 15

A discussion of these experiments at the Conference is quoted on pages 88-9 of International Conference on Physics, London, 1934, Papers and Discussions in two Volumes . Cambridge, The Physical Society, 1935. Vol. I. Nuclear Physics, as follows:



considerable effect in nature. By exposing radioactive substances to the sun under [illegible] [illegible]nduced, and this gives a further p[illegible]of, though one difficult to accept, of th[illegible] effect.

Prof. J. C. McLennan, Mr L. G. Grimmett and Dr John Read. Szilard and Chalmers* recently reported that when some beryllium was given a surrounding of ethyl iodide and both were irradiated with $\gamma$-rays, the iodide became radioactive with a decay half-period of 30 min., but that when the ethyl iodide was irradiated without the beryllium being present it was unaffected. Szilard and Chalmers suggested that the $\gamma$-rays caused the ejection of neutrons from the beryllium atoms, and that the neutrons excited the radioactivity.

At the Radium Institute the experiment of Szilard and Chalmers was repeated by us, but iodine was used in place of ethyl iodide. It, too, became radioactive, and gave 314 Geiger-Müller kicks diminishing to 12 per min. When the iodine without the beryllium was irradiated, it again became radioactive, but to a much less degree, 45 kicks diminishing to 10 per min. being recorded. In both cases the half-period was 30 min. When the beryllium was surrounded by a silver cylinder and irradiated with $\gamma$-rays, the silver became radioactive, giving 50 kicks diminishing to 12 per min. with a half-period of two minutes. When the silver alone was irradiated it was unaffected. These half-periods, it will be recalled, were the same as those obtained by Fermi, who used radon plus beryllium as a neutron source. We consider that these results confirm Szilard and Chalmers's conclusion that beryllium ejects neutrons when irradiated with $\gamma$-rays.

Experiments are in progress to determine why the iodine becomes radioactive when no beryllium is present.

Dr L. Szilard. The Fermi effect can be used as an indicator for the detection of neutron radiations. It may prove to be of special value for the investigation of neutron radiations in the presence of a strong $\gamma$-radiation. One might expect that even slow neutrons will induce radioactivity in elements which, like iodine, transmute in the Fermi effect into their own radioactive isotope, but further experiments are necessary to settle this point. Meanwhile T. A. Chalmers, of St Bartholomew's Hospital, and I have worked out a method of isotopic separation which makes it possible to concentrate chemically the activity in the case of iodine and other elements which show a Fermi effect of this type. We used this method of isotopic separation to search for new neutron sources. By irradiating 25 gm. of beryllium with the penetrating radiation from 150 mgm. radium and exposing 100 c.c. ethyl iodide to the radiation excited in the beryllium we could induce radioactivity in iodine, and separate chemically the radio-iodine from the ethyl iodide in the form of a silver iodide precipitate. This precipitate showed a strong activity, decaying with a period of 30 min., the initial activity being more than 15 times stronger in the presence of beryllium than in its absence. About half of the residual activity in the control experiment may be due to neutrons coming directly from the radium source,

* *Nature*, **134**, 494 (1934).

A 16



the other half represents the natural background effect of the counter. Apparently the $\gamma$-rays of radium liberate neutrons from beryllium, which induce a strong Fermi effect in iodine. The 30 min. and the six hours half-periods of bromine can also be strongly excited by these neutrons, as we have shown in co-operation with E. Glückauf. I was very much interested to hear just now that Prof. McLennan has repeated some of our experiments and was able to confirm our results. If we determine which elements show a Fermi effect when exposed to neutrons from a $\gamma$-ray disintegration we get by means of very simple experiments some information both regarding the processes involved in the Fermi effect and in the $\gamma$-ray disintegration. By using the Fermi effect one could thus supplement in some respects the method of Chadwick and Goldhaber, who were the first to detect a $\gamma$-ray disintegration in their pioneer work on heavy hydrogen. I wish to take this opportunity to mention that this work has been carried out in the Physics Department of St Bartholomew's Hospital and was made possible by the very kind co-operation of Prof. Hopwood.

L. Szilard and T.A. Chalmers, "Detection of Neutrons liberated from Beryllium by Gamma Rays: a new Technique for Inducing Radioactivity" Nature 134:494-5, Sept. 29, 1934.

L. Szilard and T.A. Chalmers, "Chemical Separation of the Radioactive Element from its Bombarded Isotope in the Fermi Effect" Nature 134:462-3 Sept. 22, 1934.

5 England 1933 - 1938 (1) Stenorette I (12) R
5-6-60 c̄R. 1.
Corrected Transcription 1967 Dec

I reached the conclusion something would go wrong in Germany very early. I reached this conclusion in 1930, and the occasion was a meeting in Paris - a meeting of the economists who were called together to decide whether Germany ~~could~~ can pay reparations and just how much reparations Germany could pay. One of the participants of that meeting was Dr. Hjalmar Schacht, who was at that time, I think, president of the German Reichsbank, and he, to the surprise of the world, including myself, took the position that Germany cannot pay any reparations unless she gets back her former colonies. This was such a frightening statement to make that it caught my attention and I concluded that if Hjalmar Schacht believed that he could get away with this things must look rather bad. I was so impressed by this that I wrote a letter to my bank and ~~sent~~ transferred every single penny I had out of Germany into Switzerland. I was not the only one, as I later learned, but within a few months after this speech of Schacht's, a very large sum of money mainly by depositors from abroad, was drawn out of Germany. Apparently there are many people who are sensitive to this kind of signal.

I visited America in 1931 -- I came here in December -- Christmas Day, 1931, on the "Leviathan," as a matter of fact, and stayed here until ~~-- oh, I stayed here~~ about three months, and in the course of 1932, I returned to Berlin where I was privat-dozent at at the University. Hitler came into office, I believe in January '33, and I had no doubt what would happen. I lived in the faculty club of the Kaiser Wilhelm Institute in Berlin Dahlem and I had I had my suitcases packed, and by this I mean that I had literally two suitcases standing in my room, which were packed; the key was in it and all I had to do was turn the key and leave when things got too bad. I was there when the Reichstag ~~strand~~ brand (?) occurred, and I remember how difficult it was for people there for people to understand what was going on. A friend of mine, ~~Mika Polarni (?)~~ Michael Polanyi, who was director of a division of the Kaiser Wilhelm Institute of Physical Chemistry, took a very optimistic view of the situation. They all thought that civilized Germans would not stand for anything really rough happening. The reason that I took the opposite position was based on observations of rather small and insignificant things. What I noticed was that the Germans always took a utilitarian point of view. I asked, well, suppose I oppose this, thinking, what good would I do? I wouldn't do very much good, I would just lose my influence. And why should I oppose it? You see, there was no -- the moral point of view was completely absent, or very weak, and every consideration was simply consideration of what would be the predictable consequence of my action. And on that basis did I reach in 1931 the conclusion that Hitler would get into power, not because the forces of the Nazi revolution were so strong but rather because I thought that there would be no resistance whatsoever. Well, after the Reichstag fire, I went to see my friend ~~Mika Polarni (?)~~ Michael Polanyi who was in the Kaiser Wilhelm Institute for Phys~~i~~cal Chemistry and told him what had happened and he looked at me and he said: "Do you really mean to say that you think that the ~~Mxxxxxx~~ Secretary of the Interior had anything to do with it?" and I said, "Yes, this is precisely what mean," and he just looked at me with incredulous eyes. He had at that time an offer to go to England and to accept a professorship in Manchester. I very strongly urged him to take this, but he said that if he now went to Manchester he could not be productive for at least another year because it takes that much in order to install a laboratory ~~(?)~~ and I said to him, "Well, how long do you think you will remain productive if you stay in Berlin?" We couldn't get together on this so I finally told him that if he must refuse this offer he should refuse it on the ground that his wife is opposed to it, because his wife always can change her mind and so if wanted to have the thing reconsidered, he would have an out. Later on when I was in England -- this must have been - ~~yeah~~ ja - the middle of '33, I was active in a committee, this was a Jewish committee, incidentally, this particular committee, where they were concerned about ~~standing (?)~~ finding positions for refugees from Germany and Professor Namier (?) came from Manchester and reported that ~~Polarn (?)~~ Polanyi is ~~not~~ now again interested in accepting a professorship in Manchester. He said that previously he had refused the offer extended to him on the ground that he was suffering from rheumatism but it appears that Hitler cured his rheumatism.

5. a) I left Germany a few days after the Reichstag fire but how quickly things move you can see from this -- I took a train from Berlin to Vienna on a certain date - I don't know the precise date, but close to the first of April, 1933. The train was empty. The same train, on the next day, was overcrowded, was stopped at the frontier. The people had to get out and everybody ~~haxxa~~ was interrogated by the Nazis. This just goes to show that if you want to succeed in this world you don't have to be much cleverer than other people -- you just have to be one day earlier than most people. This is all that it takes.

While I was in Vienna there were the first people dismissed from German universities, just two or three, and it was, however, quite clear what will happen. I met, walking in the street, by pure chance, a colleague of mine. He was an economist at Heidelberg, Dr. Jacob ~~Marshak (?)~~ Marschak who is now a professor at Yale; he also was rather sensitive, not being a German but coming from Russia he had seen revolutions and upheavals and he went to Vienna where he had relatives because he wanted to see what was going to happen in ~~xxxxx~~/Germany. I told him that I thought that since we are up there we might as well make up our minds what needs ~~is~~ to be done to take up this lot ~~the~~ of scholars and scientists who will have to leave Germany and the German universities. He said that he knew a rather wealthy economist in Vienna who might have some advice to give. His name was Schlesinger ~~Schavinger?~~ and he had a very beautiful apartment in the Liechtensteinpalais. We went to see him and he said, yes, it is quite possible that there will be wholesale dismissals from ~~of~~ German universities why don't we go and discuss this with Professor JastroW~~(?)~~. Professor JastroW~~(?)~~ was an economist mainly interested in the history of prices? , I believe, and we went to see the Professor - the three of us now - and JastroW~~(?)~~ said, "Yes, yes, this is something one should seriously consider," and then he ~~I~~ said, "You know, Sir William Beveridge is at present in Vienna. He came here to work with me on the history of prices, and perhaps we ought to talk to him." So I said, "Where is he staying?" and he said, "He's staying at the Hotel Regina." It so happened that I was staying at the Hotel Regina so I volunteered to look up Sir William Beveridge and try to get him interested in this. I saw

Beveridge and he immediately said that had heard about dismissals, at the London School of Economics and he was already taking steps to take on one of them and that he was all in favor of doing something in England to receive those who have to leave German ~~residence~~ universities. So I phoned ~~Shaninger~~ Schessinger Lessinger (?) and suggested that he invite him to dinner. Sches~~s~~linger ~~(?)~~ said no, he wouldn't invite him to dinner because Englishmen, if you invite them to dinner ~~it's~~ get very conceited. However, he would invite him to tea. So we had tea and in this brief get-together, Sches~~s~~linger(?) and Marschak and Beveridge, it was agreed that Beveridge ~~would~~ will, when he ~~got~~ gets back to England, and when he ~~got~~ gets his most important things which he had on the docket out of the way, he would try to form a committee which would set itself the task of ~~xxxxx~~ finding places for those who have to leave German universities, and he suggested that I come to London and that I occasionally ~~xxxxxxxx~~ prod him on this and that if I prod him long enough and frequently enough, he thought he would do it. Soon ~~after~~ there he left, and soon after he left, I left and went to London.

5. 5. a) When I came to London I phoned Beveridge. ~~xxxxxxxxxxxxxxx~~ Beveridge said that his schedule had changed and he found that he was free and that he could at once take up this job, and this is the history of the birth of the so-called ~~Economic~~ Academic Assistance ~~Conference (?)~~ Council

in England. The English adopted a policy of mainly helping the younger people but did not demand that somebody should have an established name or position in order to find a position in England. Quite in contrast to American organizations. In addition to the Ac. As. Co, there was a Jewish committee functioning. They raised ~~the~~ funds privately and they found positions for people and provided them with fellowships for one or two years. The two committees worked very closely together and in a comparatively short time practically everybody who came to England had a position, except l ~~me~~.

B.H. When I got to England, and after I had no longer to function in connection with placing the scholars and scientists who left the German universities, when this was organized and there was no need for me to do anything further about it, I was thinking about what I should do and I was strongly tempted to go into biology. ~~Xkxx~~ I went to see A.V. ~~B.~~ Hill and told him about this. Now A.V. ~~B.~~ Hill himself had been a physicist and became a very successful biologist and he thought it was quite a good idea. He said, "Why ~~not~~ don't we do it this way?" "I'll get you a position as a demonstrator in physiology and then 24 hours before ~~xxxx~~ you demonstrate you read up on these things and you should have no difficulty in demonstrating them. In this way, by teaching physiology, you would learn physiology and it's a good place to begin."

And now I must tell you why I did not make this switch at the time. In fact, I made the switch to biology in 1946. In 1932, while I was still in Berlin, I read a book by H. G. Wells. It was called, "The World Set Free." This book was written in 1913 -- one year before the World War,-- and in it H. G. Wells describes the discovery of artificial radioactivity and puts it in the year of 1933, the year in which it actually occurred. He then proceeded to describe the liberation of atomic energy on a large scale for industrial purposes, the development of atomic bombs, and a world war which was fought by apparently allies of England and France and perhaps including America, against Germany and Austria -- the powers located in the central part of Europe. He places this was in the year of 1956 and in this war the major cities of the world are all destroyed by atomic bombs. Up to this point, the book is exceedingly vivid and realistic. From this point on, the book gets to be a little, shall I say, utopian. With the world in shambles, a conference is called in Brissago in Italy, in which a world government is set up.

This book made a very great impression upon me but I didn't regard it as anything but fiction. ~~xxxxxxxxxx~~ It didn't ~~find~~ start me thinking ~~that~~ whether or not such things could, in fact, happen. I had not been working in ~~xxxxxx~~ nuclear physics up to that time. ~~Bxxx~~ Now This really doesn't belong here.

nevertheless tell you of a curious conversation which I had also in 1932 in Berlin. The conversation was with a very interesting man, named Otto Mandl (?) who was an Austrian and who became a wealthy timber merchant in England, and whose main claim to fame ~~was~~ is that he had discovered H. G. Wells, at the time when none of his works had been translated into German. He went to H. G. Wells and acquired the exclusive right to publish his works in German, and this is how H. G. Wells became known on the Continent. In 1932, something went wrong with his timber business in London and in 1932 he found himself again in Berlin. I had met him in London and I met him again in Berlin and there ensued a memorable conversation. Otto Mandl (?) said that not only he ~~thought~~ thinks, he ~~knew~~ knows what it would take to save mankind from a series of ever-recurring wars that would destroy it. He said that man has a heroic streak in himself. Man is not satisfied with a happy idyllic life. He has the need to fight and to encounter danger. He concluded that what ~~mmkxx~~ mankind must do to save itself ~~was~~ is to launch an enterprise aimed at leaving the earth. On this start he thought the energies of mankind could be concentrated and the need for ~~hxxmxxxdm~~ heroism could be satisfied. I remember very well my own reaction. I told him that this was somewhat new to me and that I really didn't know whether I would agree with him. ~~The~~

The only thing I can say is this: that if I came to the conclusion that if this is what mankind needs, I wanted to contribute something to save mankind, then I would probably go into nuclear physics, because only through the liberation of atomic energy can we obtain the means which would enable man not only to leave the earth but to leave the solar system.

I was no more thinking about this conversation or about H. G. Wells's book either, until I found myself in London about the time of the British Association, which must have been either September or October of 1933. I read in the newspapers a speech by Lord Rutherford. Lord Rutherford was quoted to say that he who talks about the liberation of atomic energy on an industrial scale is talking moonshine. This sort of set me pondering as I was walking the streets of London and I remember that I stopped for a red light at the intersection of Southampton Road, waiting for the light to change and the as the light changed to green and I crossed the street, it suddenly occurred to me that if we could find an element which is ~~xxxxxxxxx~~ split by neutrons and which would ~~emerge~~ emit two neutrons when it absorbed one neutron, such an element, if assembled in sufficiently large mass, could sustain a ~~xxx~~ nuclear chain reaction. I didn't see at the moment just how one would go about finding such an element, or what experiments would be needed, but the idea never left me and soon thereafter, when the discovery of artificial radioactivity was announced, a discovery made by Joliot and Mmé. Joliot (?) I suddenly found the tools were ~~there~~ at hand to explore the possibility of such a chain reaction (?). I talked to a number of people about this. I remember that I mentioned this to J. P. Thompson and I mentioned it to Blackett(?) but I couldn't evoke ~~work up~~ any enthusiasm. ¶ I had one candidate for an element which might be instable in the sense of splitting of neutrons when it disintegrates, and that was beryllium ~~xxxxxxxx(?)xxxxxxxxx(?)xx~~ The reason I suspected beryllium ~~x(?)x~~ of being a carrier ~~potential~~ candidate for sustaning a chain reaction was the fact that the mass of beryllium ~~x(?)x~~ was such that it could have disintegrated into two other particles in a neutron. ~~XX~~ While it was conceivable, ~~xxxxxx~~ it was not clear why it didn't do this spontaneously - why it did not ~~xxx~~ disintegrate spontaneously - since the mass was large enough to do that, but it was conceivable that it had to be tickled by a ~~xxxxxxx~~ neutron which would shake the beryllium / nucleus in order to trigger such disintegration. ~~Xxxxxxxxxxxxxxxxxxxxxxxxx~~ I remember that I told Blackett(?) that what one ~~we~~ ought to do is really get a large mass of beryllium, large enough to notice whether it can sustain a chain reaction. Beryllium was very expensive at the time -- almost not obtainable, and I remember Blackett's reaction was, "Look, you will have no luck with such fantastic ideas in England. Yes, perhaps in Russia. If a Russian physicist went to the government and said, we must make a chain reaction, they would give him all the money ~~that~~ and facilities which he would need. But you won't get it in England." As it turned out later, ~~xxxxx~~ beryllium cannot sustain a chain reaction and it is, in fact, stable. What was wrong was that a published mass of helium was wrong. This was later on discovered by Bethe ~~(?)~~ and it was a very important discovery for all of us because we did not know where to begin to do nuclear physics if there could be an element which could disintegrate but doesn't.

Well, when I gave up the beryllium, I did not give up the thought that there might be another element which ~~could~~ can sustain a chain reaction. In the spring of 1934 I had applied for a patent which described the laws (March 12, 1934) governing such a chain reaction. It was for the first time, I think, that the critical mass, the concept of critical mass was developed and that a chain reaction was seriously discussed. ~~xxxxxxxxxxxxxxx~~ Knowing what this would mean -- and I knew it because I had read H.G. Wells -- I did not want this patent to become public. The only way to keep it from the public was to assign it to the government. So I ~~xxxx~~ assigned this patent to the British ~~Air Ministry~~ Admiralty. At some point I decided that the reasonable thing to do was to investigate systematically all the elements. There were 92 of them -- but of course this is a rather boring task, so I thought that what I ~~would~~ will do ~~would be~~ I will ~~to~~ get some money, have some apparatus built, and then hire somebody who would sit down and just go through one element after the other. The trouble was that none of the physicists had any enthusiasm for this idea of a chain reaction. I thought, there is after all, something called chain reaction in chemistry. It doesn't resemble a nuclear chain reaction but still it's a chain reaction. So I thought I ~~would~~ will talk to a chemist, and I went to see Dr. Chaim Weizmann (Professor Chaim Weizmann), who was a renowned chemist and the Zionist leader, and I had met him on one occasion or another. And Weizmann listened and Weizmann understood what I told him, and he said: "How much money do you need?" and I said that I thought that ~~$2x00~~ 2,000 pounds would be enough to do this, which would have been at that time about $10,000. So Weizmann thought that he would try to get this money. I didn't hear from him for several weeks, but then I ran into ~~Mika Polarni (?)~~ Michael Polanyi who by that time had arrived in Manchester and was head of the chemistry department there and he told me that Weizmann had come and talked to him about my ideas for the possibility of a chain reaction and he wanted to know ~~what~~ ~~Polarni's (?)~~ Polanyi's advice whether he should ~~to~~ get me this money. ~~Polani (?)~~ Polanyi said that he thought this experiment ought to be done. And then again I didn't hear anything. As a matter of fact, I didn't see Weizmann again until the late fall of '45, after Hiroshima. I was at that time in Washington and I ran into him in the Wardman-Park Hotel. He seemed to be terribly happy to see me and he said: "Do you remember when you came to see in London?" I said, "Yes." He said: "And do you remember what you wanted me to do?" I said, "Yes," and he said, 'Well, maybe you won't believe me, but I tried to get those 2,000 pounds and found that I couldn't."

Because of these thoughts about the possibility of the chain reaction and because of the discovery of artificial radioactivity, physics became too exciting for me to leave physics. So I decided not to go into biology as yet, but play around a little bit with physics, and I spent several months in the spring -- in the early spring -- throughout the spring -- at the Strand Palace Hotel, doing nothing but dreaming about experiments which one could do, utilizing this marvelous tool of artificial radioactivity which Joliot (?) had discovered. I didn't do anything; I just thought about these things. I remember that I went into my bath; -- I didn't have a private bath, but there was a bath in the corridor of the Strand Palace Hotel -- around nine o'clock in the morning (there is no place as good to think as the bathtub). I was just soaking there and thinking and around 12 o'clock the maid would knock and say, "Are you all right, sir?" Then I usually got out and made a few notes, dictatin g a few memoranda, and I played around this way doing nothing and the summer came around. At that time, I thought that ~~I~~ one ought to try to learn something about beryllium and I thought that ~~xx~~ if beryllium is really so easy to split the gamma rays of radium ~~should~~ should split it and it should split off neutrons.

I had casually met the director of the physics department of ~~some veteran's~~ St. Bartholomew's hospital so I dropped in for a visit and asked him whether in the summer time, when everybody is away, I couldn't ~~xx~~ at that time use the radium, which is not much in use in summer, in an experiment of this sort, and he said, yes, I could do it, but since I'm not on the staff of the hospital, I should team up with somebody on his staff. There was a very nice young Englishman, Mr. Chalmers, who was game, and so we teamed up and for the next two months we did experiments. It turned out that in fact beryllium spits off neutrons when exposed to the gamma rays of radium. This later on became really very important because these neutrons are slow neutrons and therefore if they disintegrate elements like uranium (of course we didn't know that until after the discovery of ~~[illegible]~~ Hahn) and if in that process neutrons come off, which are ~~fast~~ hard, you can distinguish them from neutrons of the source, which are slow. We did, ~~in effect, make~~ ~~in a sense~~ essentially two experiments -- we demonstrated that ~~xxxx~~ beryllium ~~xxxxxxxx~~ emits neutrons if exposed to the gamma rays of radium and we demonstrated something else, which is called the Szilard-Chalmers effect, which I will not describe now. ~~xxxxxxxxxx~~ But anyway, these experiments established me as a nuclear physicist, not in the eyes of Cambridge, but in the eyes of Oxford. Phy. 5-8, There was an international conference on nuclear physics in London in September ?1935 where these two discoveries were discussed by the participants and so I got very favorable notice and this led, within six months, to an offer of a fellowship at Oxford. However, I didn't get this offer until I had left England and come to America, ~~xx~~ where I didn't have a position but some sort of fellowship, and when I received this offer from Oxford, I had the choice of either keeping this fellowship in America or returning to Oxford. ~~XXXXXXXX~~ I sat down and wrote a letter to Michael ~~Rolarni(?)~~ Polanyi in which I described my choice between these two alternatives and this is what I wrote him: that I would accept the fellowship at Oxford and go to England and I would stay in England, I wrote, until one year before the war, at which time I would shift my residence to New York City. That letter was very funny, because how can anyone say what he will do one year before the war? So the letter was passed around and a few people commented on it when I finally turned up in England. However, this is precisely what I did. In 1937, I decided that the time had come for me to change my full-time ~~for a full-time~~ fellowship appointment at Oxford to one which permitted me to spend six months out of the year in America, and on the basis of that arrangement, (I had to take a cut of salary -- of course I had to go on half pay) so my total income amounted to $1,000 a year) I came over to America in [?] 1938, and I did nothing but loaf. I didn't look for a position, I just thought I would wait and see. Then came the Munich crisis, that the Munich crisis broke and I was at the time ~~at [illegible]~~ 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, who later was Lord Cherwell, who was director of the Clarendon Laboratory where I was employed. The letter said that I was now quite convinced ~~xxxxxxx~~ that there will be war, and that 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. ~~Before~~ And so I resigned at Oxford and stayed here.

I was still intrigued with the possibility of a chain reaction, ~~and for that~~

5. England 1933 – 1938 (1)

Stenorette 1963 tape
Summarized by L.S.
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In the Fall of 1933, I found myself in London. I kept myself busy trying to find positions for German colleagues who lost their university positions, with the advent of the Nazi regime. One morning I read in the newspapers about the annual meeting at the

British Association and Lord Rutherford was reported to have said that whoever talks about the liberation of atomic energy on an industrial scale was talking moonshine. Pronouncements of experts to the effect that something cannot be done have always irritated me. That day as I was walking down Southampton Row and ~~was~~ stopped ~~by~~ for a traffic light, I was pondering where Lord Rutherford might not prove to be wrong. "What could make him turn out to be wrong?" I asked myself. As the light turned green and I was crossing the street, it occured to me that if ~~I~~ one could find a n element that absorbs neutrons and is split in such a fashion that for each neutron it absorbs, it emits two neutrons, it would be possible to set up a chain reaction.

Later on, I asked myself which element might qualify in this regard and my attention focused, among several suspected elements, on beryllium. On the basis of the published mass ~~published for~~ of this element, beryllium should have been unstable. It seemed conceivable that if it were to abosrb a neutron, it would fall to pieces and release two neutrons in the process. By the Spring of 1934, I had developed the theory of such a chain reaction which included the concept of the critical mass and I filed a provisional patent application in England for the principles which were involved.

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I actually started experimenting with beryllium in the Summer of 1934 at St. Bartholomew's Hospital in London. I happened to know the head of the Physics Department there and I assumed that with the surgeons away on vacation, I might be given access to the radium stock of the hospital. I borrowed a Geiger-counter, and I teamed up with Chalmers, a young Englishman on the staff of the hospital; together we improvised a series of experiments. For an appreciable portion of my savings, I bought a quantity of ~~some~~ beryllium ~~and~~. We found that beryllium was indeed unstable, in the sense that it emitted neutrons when it was exposed to the gamma rays of radium. These so-called photo-neutrons of beryllium were destined later on to play a major role ~~in 1939~~ in the establishment of the fact that the fission of uranium can maintain a chain reaction.

standard no longer stimulates population but tends the opposite way; for the fourth, the external outlets are now largely self-producers. As regards the rapid introduction of new things—these mostly now demand increased leisure for their proper absorption and use, so that the two are co-related and mutually dependent.

It can be conceived that a socialistic organisation of society could obviate such of the maladjustments as depend upon gains and risks of absorption not being in the same hands, and a theoretical technique can be worked out for the most profitable rate of absorption of scientific invention, having regard to invested capital, and skill and local interests. It is sufficient to say that it needs a *tour de force* of assumptions to make it function without hopelessly impairing that central feature of economic progress, namely individual choice of the consumer in the direction of his demands, and an equally exalted view of the perfectibility of social organisation and political wisdom. But in the field of international relations and foreign trade, which alone can give full effect to scientific discovery, it demands qualities far beyond anything yet attainable.

Economic life must pay a heavy price, in this generation, for the ultimate gains of science, unless all classes become economically and socially minded, and there are large infusions of social direction and internationalism, carefully introduced. This does not mean government by scientific technique, technocracy, or any other *transferred* technique, appropriate as these may be to the physical task of production; for human wills in the aggregate are behind distribution and consumption, and they can never be regulated by the principles which are so potent in mathematics, chemistry, physics, or even biology. Scientific workers may contribute much by sharing the problems of social science along its own lines, by giving a greater proportion of brilliant minds to this field and by planning research.

## Atomic Transmutation

TWENTY-SIX years have passed since the British Association last met at Leicester in 1907, and the apparently stable world of a quarter of a century ago has altered almost out of recognition. These changes in political, moral and spiritual values are reflected in the world of physical science, which differs almost *toto cœlo* from the structure raised by the labours of the nineteenth century and its predecessors. But even then rumblings were apparent, and it is a remarkable fact that the discussion on atomic transmutation, opened in Section A (Mathematics and Physics) by Lord Rutherford on September 11, had its antitype in a sectional discussion on the constitution of the atom opened by Prof. Ernest Rutherford, as he then was, at the 1907 meeting, to which contributions were made by Lord Kelvin, Sir Oliver Lodge and Sir William Ramsay.

Lord Rutherford, whose contribution to the present discussion was a masterly review of a quarter of a century's work on atomic transmutation, remarked that, at the discussion which he opened in 1907, he indicated the importance of the transformations of radioactive bodies, and emphasised the difficulty of explaining the part played by positive electricity—we had then no inkling of a knowledge of the positive electron. He reminded the audience that Sir Oliver Lodge, who nevertheless proclaimed his belief in the electrical structure of the atom, had remarked that the opener was an adept in the art of skating on thin ice. Kelvin, who in 1904 was prepared to accept the notion of the transmutation of the radium atom, in 1907 did not find the evidence for transmutation satisfactory. It was about this period that Lodge, in a letter to the *Times*, suggested that if Kelvin would read the evidence he would change his opinion; Kelvin's reply was that he *had* read Rutherford's "Radioactivity" and remained unconvinced!

The work of the eighteenth and nineteenth century chemists had given to the world some eighty-odd elements, and it was quite clear that the atoms of the elements were very stable structures. But though the old ideas of transmutation were exploded, the problem still existed, and indeed had been clearly formulated by Faraday. The discovery of radioactivity showed that elements such as uranium and thorium were undergoing spontaneous transformation, and a large number of new elements were brought to light. Moreover, the property was shown to exist in a very slight degree in elements such as potassium and rubidium, the remainder of the normal elements being stable under ordinary conditions over periods to be reckoned in millions of years.

It was in 1911 that the nuclear structure of the atom was clearly evidenced, and a little later that Bohr's masterly interpretation of the movements of electrons gave an explanation of spectral regularities. It soon became evident that outer electrons played no major part in transmutations, that the changes produced by stripping off electrons were only temporary in character, and that the structure of the nucleus must be changed if we wished to institute any permanent atomic transmutation. Moreover, evidence had accumulated to show that the nucleus was a very small entity.

If an α-particle were fired at a nucleus, the enormous forces developed in a head-on collision might be expected to disturb the structure of the nucleus, and it was in 1919 that decisive experiments were made. When α-particles were fired in oxygen, no effect was produced, but when they were fired in nitrogen, a new type of particle appeared—the *proton*.

Replacement
pages A-15 + A-16

Notes to p. (4)

The Szilard-Chalmers reaction is used to separate chemically a radionuclide from isotopic target material following a nuclear reaction. Szilard and Chalmers in 1934 subjected liquid ethyl iodide to neutron bombardment and observed that a considerable fraction of the radioactive iodine produced was in a different chemical state and could be extracted in water solution.

L. Szilard and T.A. Chalmers, "Detection of Neutrons liberated from Beryllium by Gamma Rays: a new Technique for Inducing Radioactivity" _Nature_ 134:494-5, Sept. 29, 1934.

L. Szilard and T.A. Chalmers, "Chemical Separation of the Radioactive Element from its Bombarded Isotope in the Fermi Effect" _Nature_ 134:462-3 Sept. 22, 1934.

A discussion of these experiments at the Conference is quoted on pages 88-9 of _International Conference on Physics, London, 1934, Papers and Discussions in two Volumes_ Cambridge, The Physical Society, 1935. Vol. I. Nuclear Physics.

# Nuclear Rocket Runs at Full Power an Hour

BY MARVIN MILES
Times Aerospace Editor

A nuclear rocket reactor was operated at full power for 60 minutes Friday at Jackass Flats, Nev., in a test to prove the exotic system's endurance capability for future deep space missions.

The reactor produced 1,100 megawatts of power—about the same output as Hoover Dam—for double the time of any previous test at the Nuclear Rocket Development Station, 15 minutes longer than required for a voyage to Mars. The thrust level was 55,000 pounds.

Developed by the industrial team of Aerojet-General Corp. and the Westinghouse Astronuclear Laboratory, the test system is the sixth in a series of experimental reactors in the NERVA project, an acronym for Nuclear Engine for Rocket Vehicle Application.

NERVA is part of the Rover nuclear rocket program directed jointly by the Atomic Energy Commission and the National Aeronautics and Space Administration.

The test, which spewed a vapor plume 5,000 feet above the desert floor, was delayed for more than a week by technical difficulties and winter weather. It was conducted just before a heavy snowstorm closed roads in the area.

The reactor, connected to a huge storage facility that held 1 million gallons of chill liquid hydrogen fuel, will now be disassembled and examined to determine the effects of the test.

Primary purpose of the prolonged run was to obtain additional data on reactor and fuel element characteristics under extended operating time at design power.

On future deep space missions a nuclear rocket would be boosted into earth orbit by a giant Saturn 5 rocket as part of a cruise stage, then provide power for prolonged flight and course correction maneuvers.

For a roundtrip to Mars, five reactors would be coupled to the engine system of the stage, scientists said.

Under development for the past decade—at a cost of slightly more than $1 billion — nuclear rockets should be ready for manned space flight by the mid 1980s.

Replacement for pages A-15 + 16

Notes to p. (A)

The Szilard-Chalmers reaction is used to separate chemically a radionuclide from isotopic target material following a nuclear reaction. Szilard and Chalmers in 1934 subjected liquid ethyl iodide to neutron bombardment and observed that a considerable fraction of the radioactive iodine produced was in a different chemical state and could be extracted in water solution.

L. Szilard and T.A. Chalmers, "Detection of Neutrons liberated from Beryllium by Gamma Rays: a new Technique for Inducing Radioactivity" Nature 134:494-5, Sept. 29, 1934.

L. Szilard and T.A. Chalmers, "Chemical Separation of the Radioactive Element from its Bombarded Isotope in the Fermi Effect" Nature 134:462-3 Sept. 22, 1934.

A discussion of these experiments at the Conference is quoted on pages 88-9 of International Conference on Physics, London, 1934, Papers and Discussions in two Volumes Cambridge, The Physical Society, 1935. Vol. I. Nuclear Physics.

GWS

Additional material for page 98 (→ Bailyn folder)

re "everybody...had a position, except me."

| | | | |
|---|---|---|---|
| cc. Letter, | L.S. to ? | 11 August, 1933 | (R-1) |
| " Letter, | Einstein to Donnan. (Translation) | 16 August, 1933 | (R-1) |

A

To page (A-3)

page 101 Perspectives

Letter to Sir Hugo, 3-17-34

→ B

History A PWS

Columbia University
in the City of New York

DEPARTMENT OF PHYSICS

December 21, 1934

Dr. Leo Szilard
Strand Palace Hotel
Strand, London W.C.2.

Dear Szilard:

I was very glad to hear that you are coming to New York and we are looking forward to seeing you. I turned your letter over to Professor Pegram to answer the questions with regard to the radon supply since I have no direct connection with that work. From what I hear, the situation does not look very favorable for obtaining considerable supplies of radon or of radium.

We have been concentrating on measurements of nuclear spins and moments with molecular beams and I hope we shall have some interesting things to show you when you arrive.

I want to congratulate you on the very ingenious and important experiments which you have made on gamma ray disintegration.

Please let me know when you expect to come and if I can be of help in any way.

Best regards from Mrs. Rabi and myself,

Sincerely yours,

I. I. Rabi

I.I. Rabi

IIR:BM

March 4, 1935
c/o. B. Liebowitz
420 Riverside Drive
New York City, N.Y.

Dear Professor Lindemann:

A few days ago I saw Professor Einstein in Princeton. When he heard that we planned that I should work in the Clarendon and that the financial question is not yet settled, he suggested, - before I could mention our conversation on this subject - that the sum which was reserved for him by Christ Church might be used for this purpose. Subsequently I told Prof. Einstein of our conversation and asked him, if it were convenient to him that he should write to Christ Church, if you and Prof. Schrödinger come to the conclusion that this is the best course to take, and that in this case you should let him know in what way to write to Christ Church. I think, this is in perfect order and Prof. Einstein will write you a few lines direct.

I have informed the chairman of the Department at New York University of the position in Oxford and was relieved to see that he took a very friendly attitude in the matter. They seem to think that I ought to accept their offer and wait until I hear from you that something definite has been settled at Oxford. They emphasize that I could leave here at twenty-four hours' notice if required. For the moment I refrained from discussing this point beyond thanking for this offer.

I wonder if you could kindly let me know by cable (night-letter) the result of the I.C.I. meeting together with such comment of yours, as you think necessary.

Bethe has developped a simple theory which can explain the large cross-section of certain elements for the capture of slow neutrons and following up this line a number of simple experiments present themselves. I enclose a further page on this point. Could you perhaps also pass it on together with my kind regards to Collie and Griffiths ?

Yours sincerely

History A fo/s

PERSONAL.

74, Gower Street,
London, W.C.1.

3rd June, 1935.

Professor Lindemann,
Clarendon Laboratory,
Oxford.

Dear Professor Lindemann,

I hopevvery muchtto see you on Wednesday and talk to you about a matter which appears to me to be of great seriousness. For some time back I have suspected that the three radio-active periods which Chalmers and I found in the case of indium involved a new type of process in which a neutron

a) either knocks out another neutron from indium 113 in a non-capture process, or

b) liberates a neutron of the mass number 2 from indium 113 and gets captured in the process.

I have gradually come to the conviction that either a process of the type a) or, alternatively, a process of the type b) does occur and is possibly responsible for a number of other known radio-active periods, which I believe I can single out. I believe you will share this conviction after you have heard my arguments on Wednesday.

The question whether a neutron of the mass number 2 exists and can be liberated by fast neutrons cannot be answered offhand, but it is perhaps fair to say that since one of the two processes a) or b) certainly occurs, we have something like a fifty to fifty chance that such " double neutrons " are involved.

Professor Lindemann, Oxford. 3.6.35.

It seems to me that the question whether or not the liberation of nuclear energy and the production of radio-active material on a large scale can be achieved in the immediate future, hinges on the question whether or not " double neutrons " can be produced. If " double neutrons " can be produced, then it is certainly less bold to expect this achievement in the immediate future than to believe the opposite.

Even if I am grossly exaggerating the chances that these processes will work out as I envisage it at present, there is still enough left to be deeply concerned about what will happen if certain features of the matter become universally known. In the circumstances, I believe an attempt, whatever small chance of success it may have, ought to be made to control this development as long as possible.

There are two ways in which this can be attempted. The more important one is secrecy, if necessary, obtained by agreement among all those concerned that another form of publication should be used as far as the dangerous zone is concerned, which would make experimental results available to all those who work in the nuclear field in England, America and perhaps in one or two other countries, but otherwise keeping the result quiet, until those who are concerned ar satisfied that no " double neutron " is involved.

The other way, the less important one, is to take out patents. Early in March last year it seemed advisable to envisage the possibility that, contrary to current popular opinion, the release of large amounts of energy and the production of large amounts of radio-active material might be imminent. Realising to what extent this hinges on the " double neutron ", Professor Lindemann, Oxford. 3.6.35.

I have applied for a patent along these lines, including also the production of radio-active material by neutron bombardment. This was filed before Fermi started his fundamental experiments and was followed by a number of further patent applications along the same lines. Obviously it would be misplaced to consider patents in this field private property and pursue them with a view to commercial exploitation for private purposes. When the time is ripe some suitable body will have to be created to ensure their proper use. Also one has to avoid applying for patents wherever secrecy is endangered or in countries which are likely to misuse them; so far I have carefully observed this point.

Though I do not know for the present what will be the proper steps in this matter, I am very anxious to keep my full freedom of action in everything connected with it.

As far as experiments in this special field go, I should like to keep them, as far as possible, in my own hands and not merely act as a " catalyst ". As long as Collie, Griffithsaand I work alone in this field at Oxford, it is not quite easy for me to run these experiments in my own way and, without appearing pretentious, publish or not publish, according to what I think I should. I hesitate also to suggest that the whole Nuclear Department at Oxford should work in a field which may yield very little of purely scientific interest, if it turns out that we have to deal with a non-capture process after all.

If I knew that it would be convenient to you, I should make an attempt to get a budget of £1,000 for next year from private persons in order to be able to take on one or two helpers with whom I could work in this special field in the Clarendon Laboratory, while I would still, if it appears useful, work with Collie and Griffiths in the general field as envisaged hitherto.

~~Whether~~ Whether an attempt to get financial assistance will be successful or not, I cannot tell, but I feel justified in approaching ~~a~~ man of vision in this matter. I~~s~~should be very happy if you, too, thought that Oxford is in many ways well suited for this type of work and that, conversely, this type of work could greatly accelerate the building up of nuclear physics in Oxford.

There is another purely personal and therefore minor matter which I have to mention. I saw to-day in " Nature " a letter to the editor signed by Collie, Griffiths and myself. This is the first thing I knew of the experiments having been actually started, not to speak about the conclusions which Collie and Griffiths draw. I fully appreciate the good intentions which obviously actuated Collie and Griffiths and am anxious to avoid hurting their feelings. I am ataa loss what to say. I am sorry that I have to speak about this at a time when there are so many more important things to worry about.

Yours sincerely,

Backfolder No. 1.

UNIVERSITETETS INSTITUT
FOR
TEORETISK FYSIK

BLEGDAMSVEJ 15, KØBENHAVN Ø.

DEN February 4th 1936.

Dear Dr.Szilard,

I look forward to see you when I come to London, where my wife and I will be staying at Professor Donnan,23 Woburn Square, W.C.1, from Sundag evening Febmuary 9. till Wednesday morning February 12., when we are going up to Cambridge.

We have of course all here been very interested in your beautiful recent researches on the nuclear problems, with which we are also mueh occupied here. In the last months I have tried to develop some simple views about the constitution of the nucleus, which seem to account in a comprehensive way for the typical features of nuclear reactions including the disintegrations by $\gamma$-radiation observed by you. In my lecture in the university college, February 11. I will speak about these problems inatead of the more general one on space and time in atomic physics, originally proposed.

With kind regards,

yours sincerely,

N. Bohr

5.

[Hist J.] (R-46)

c/o Clarendon Laboratory,
Parks Road,
O x f o r d .

21st May, 1936.

Sir,

You might perhaps remember that I mentioned to you some two years ago patents for which I had applied in March 1934. One such patent has now been granted on the principle of production of artificial radio-active elements by neutrons and the question arises to what use such patents ought to be put.

I cannot consider patents relating to nuclear physics as my property in any sense whatever. It would seem that if such patents are important, they ought to be administered in a disinterested way by some disinterested persons. It is, however, hardly for me to take any decisions about the patents which I have taken out in the capacity of a sort of self-appointed trustee, and apart from yourself, one could perhaps think of Chadwick, Cockroft, Joliot and Fermi as being the proper persons to say whether these patents should be withdrawn or maintained and in what way and by whom they should be administered, if they are maintained.

About two years ago I attempted to point out to Oliphant in greater detail why I thought that the existence of such patents might be useful. I am referring to this in the enclosed copy of a letter addressed to Dr. Cockroft, which I am sending you in case you should care to have more detailed information.

If I were convinced that industrial applications of great importance were imminent, I should not hesitate to ask you to give your attention to the matter at this juncture. This not being so, I shall merely ask Cockroft to inform you about all this if he thinks it necessary or, alternatively, I shall of course be at your disposal at any time you should think the matter sufficiently important to give some of your attention to it. I hope to be able to get Cockrofts advice next week and to discuss with him and Dee some unpublished observations which may or may not have a direct bearing on these matters.

Yours very truly,

(Leo Szilard)

Draft of ~~the~~ letter sent 5-27-36

(Hist-J) (R-46)
67c

c/o Clarendon Laboratory,
Parks Road,
Oxford.

21st May, 1936.

Dear Dr. Cockroft,

Please forgive me for still being unable to talk about slow neutrons or any other similar subject in public (Kapitza Club). There are, however, some unpublished observations which I made in January and which I should very much like to discuss with you, Dee and Oliphant. They may or may not have a direct bearing on another matter on which I should like to have your advice. This concerns patents for the production of artificial radio-active elements for which I applied during a period of enforced leisure between March and September 1934.

I did not ever consider these patents as my property and the question now arises what to do with them. It is hardly for me to decide whether the patents should be withdrawn or maintained and in what form and by whom they should be administered. Though I should be glad to emphasise a definite point of view in this connection, it would seem that apart from Rutherford, it is for men like you, Chadwick, Joliot and Fermi to decide these questions, if the matter appears to be sufficiently important to deserve some attention.

Perhaps it is possible to envisage a proper form for a disinterested control of such patents. It would not seem right to me that physicists who take out such patents should derive financial or other privileges from them and if no disinterested form of control can be found, I personally would rather withdraw those patents which I have taken out. I am enclosing a booklet on the American Research Corporation which might interest you, though I do not believe that their example should be imitated.

Some two years ago I had a detailed conversation about these patents with Oliphant. I went to see him about them soon after Fermi's first discoveryand I attempted tompointoout to him why I thought the existence of such patents might be useful. It seemed at the time that the possibility of an important industrial development hinges on the possibility of setting up enormously efficient sources of neutrons and that this possibility in its turn hinges on the question of the existens of a heavy isotope of the neutron.

If such multiple neutrons exist, we may envisage the remote possibility of an industrial revolution in the not too distant future. In that case patents might be used by scientists in a disinterested attempt to exercise some measure of influence over a socially dangerous development.

On the other hand, if no heavy neutron isotope exists, it would seem that an industrial development based on the application of nuclear physics must necessarily be very limited and there is no real need for any physicist to concern himself with such patents. The only use to which such patents could then be put, is towards obtaining funds for research purposes from persons interested in the promotion of industrial development. I personally felt inclined to think that good use could be made of such funds if they were forthcoming, especially in universities which are less well off than Cambridge or Oxford.

In these circumstances it appeared useful to apply for patents along two lines, i.e. the production of radio-active elements by neutrons and the construction of abundant sources of neutrons which is based on the hypothetical existence of multiple neutrons. Since we do not know at present whether such multiple neutrons exist and can be used for setting up abundant sources of neutrons, references to multiple neutrons in patents are either misleading or dangerous and accordingly care has been taken that patents which contain such references should not be published.

When I applied for the first patents in March 1934, I had an unclear idea that all these patents might be assigned to the Cavendish or some similar institution. After Fermi's discoveries and my conversations with Oliphant, I realised that this would not be feasible and made an attempt to assign all the patents to a Government Department. In this I was not entirely successful; while one patent has been assigned in this way and remains unpublished, another patent, relating to the Fermi effect, could not be assigned and has been published.

Some observations which I have so far not published may have a bearing on the question of the multiple neutrons. These observations may allow of more than one interpretation. However, the only interpretation which satisfies my desire for simplicity involves a heavy isotope of the neutron of mass number 4. You will perhaps take an altogether different view of these experiments, but I feel that I have no choice but to find out the more direct methods of observation, whether ~~xxxx~~ or not such a particle is involved in my experiments. A number of more direct experiments suggest themselves and about these I should very much like to have your advice.

It is fairly obvious that if such a particle exists, its mass exceeds 4.014. This again has rather obvious implications. One of them is rather frightening; and I felt it might be better not to publish anything on such a dangerous subject. It is quite likely that I am taking a rather exaggerated view of these things.

I may be in Cambridge in the beginning of next week and I shall then try to get hold of you and Dee. If I should not turn up, could you possibly let me know if you will be about during the second half of the week ?

Would you be kind enough to pass on this letter to Dee ?

With best wishes,

Yours sincerely,

P.S. I am sending a copy of this letter to Oliphant. Another copy is being sent to Rutherford, to whom so far I made only a passing remark about having taken out certain patents, when I had a short interview with him two years ago.

from Darrow file "History" Eb.s. 2

Interview with L. Szilard on History of

February 12, 1945

Present: L. L. Szilard, K. K. Darrow, R. S. Mulliken

Szilard: I thought of the possibility of chain reaction in 1933-34. In 1933 Rutherford made a statement about releasing nuclear energy. At that time a mass effect of Be was measured by Bainbridge. He mass was already known. I thought, why does not Be disintegrate, and perhaps if one would "tickle" Be with a neutron it would disintegrate. I calculated the critical size of a chain reacting system; calculated the critical mass for Be.

In March 1944 I started to write these findings down. It turned out that the mass of He was wrong and the mass of Be was right. Actually it wasn't possible to make a chain reaction using Be. I thought of disintegration of Be with gamma rays. Found definite threshold, began to suspect that something was wrong. Later Bethe and Oliphant came to conclusion that Be mass was wrong. I did not give up the idea. Photo-disintegration was method of testing mass. It seemed that there should be a liberation with gamma rays. There was. I thought U, Th, or even In might be used. $3\frac{1}{2}$ hours in indium. We now know it is an excited state, did not know it then. Did not know that it was a stable atom. This was first case of a ~~long-lived~~ nuclear isomerism. Later on a similar situation was found in bromine.

Darrow restates Szilard's remarks:

In 1934, Dr. Szilard calculated from the available data on the mass of the Be nucleus that this mass was greater than the sum of the masses of two alpha particles and one neutron, and that therefore it should be possible to detach the neutron by tickling the nucleus. At this time he first conceived the idea of a chain reaction, and worked with the size of a sphere of Be sufficient to bring about such a chain reaction. The experiment was tried with $\gamma$ rays rather than neutrons, and it was found that there was a sharp threshold at 1.6, which indicated that the mass was after all smaller than the sum of the masses of the particles composing it as was later to be confirmed by better measurements of the mass of the $\alpha$ particle.

Szilard: Designed an experiment at Oxford which I never carried out there. I thought of detecting such a neutron emission by using an unknown element to emit neutrons which would be fast, then detecting by an ionization chamber.

Darrow restates:

It then occurred to Dr. Szilard to use Ra-$\gamma$-Be neutrons to bombard various elements from which he might expect to expel fast neutrons and detect these with a detector sensitive only to fast neutrons. Preparations were made for this experiment at Oxford, but were interrupted by departure of Dr. Szilard for America.

Szilard: In 1935 I sent a ~~paper~~ patent to the British Navy. It is first document pertaining to concepts of chain reaction. I thought this knowledge should be kept secret as I felt it was of military importance and so to do this in Britain one had to send such a paper to the government. (A corresponding American patent application was filed in March 1935, but British claimed the idea was Secret-Secret and parts relating to chain reaction had to be withdrawn.) By the winter of 1938 I had found that Be, Indium did not work. I worte a letter to the British Navy suggesting that the paper be dropped as by then I thought it useless. Before the letter reached them, I sent a telegram asking them to disregard the letter when it arrived because meantime fission had been discovered.

Szilard: I came to see Wigner at Princeton about two weeks or ten days before the meeting in Washington, D. C. (one of Gamov's meetings). I sent to Britain for a Be block to use as a gamma ray source. During the Washington meeting I was ill. Later I went to see Rabi and told him of the importance of the discovery of fission. Suggested to Rabi that he talk with Fermi and emphasize the necessary secrecy of any experiments to be carried out. After their discussion, I asked Rabi what was Fermi's reaction, and Rabi said that Fermi had said "Nuts!" But both Rabi and I then talked with Fermi and we finally got him to say that there was a remote possibility. I told Fermi about the experiment I had planned. Fermi discussed his own experiment.

Darrow restating:

Szilard then planned to perform the experiment which had been abandoned by his leaving Oxford. He tried to sell Fermi the idea of using photo neutrons. [(Fermi, however, preferred to use the available Ra-Be source and slow down the Be neutrons in water( --Szilard breaks in)]. However, Fermi's interest was extremely mild and accordingly Szilard and Zinn nevertheless went ahead with Szilard's method involving the use of photo-neutrons. Between March 1 and March 3, 1939, they got positive results. This was later written up and published in the Physical Review.

On the morning of March 4, 1939, Szilard visited Fermi who in the meantime had been working with his own method and had got indications of the positive results. Fermi's experiment was designed in hope of showing that U produces neutrons in excess of the number of neutrons absorbed. Szilard's experiment did not show absorption of neutrons. It was then realized that Fermi's experiment did not exclude the possibility of (n, ~~to~~ 2 n) reactions which are ~~in~~ not chain reacting. Fermi then conceded that Szilard was right, used ~~them~~ photo-neutrons himself and found fast neutrons appearing at distances greater than the photo-neutrons could reach.

Szilard: From March to April 1939, I was agitating for non-publication of these various experiments. On February 2, 1939, I wrote to Joliot to dissuade him from publishing any possible results which he might have found. I knew that Joliot was a clever man and that he was probably carrying on some experiments of his own after the news of fission became known. Fermi was willing not to publish if there was sufficient backing in this country. Szilard met Fermi and Teller in Washington. Later in New York, Fermi had told Wigner we should not publish anything. On March 20, 1939, I learned that Joliot had published. I still wanted to continue not publishing even after Joliot had published. I thought we could finally bring him around to our side before much harm was done. Wigner and Teller supported this view. (Mentions much correspondence between scientists of several countries about question of publishing). Joliot answered finally, saying "The question has been studied."

Darrow restates:

Many efforts were made to induce people to suspend publication.

Szilard: Joliot based his refusal to publish on the appearnce of new stories in America about fission. Neutron emission and possibility of delayed neutron emission had been discussed at Washington conference. We finally agreed to leave the whole decision up to Wigner; he decided in favor of publication. After that, the question was, how can we decide whether a chain reaction can be maintained in a water system? Fermi, Anderson and I made a joint experiment which was published in Physical Review. The experiment consisted of a lattice of Uranium oxide in water. Some manganese sulphate was dissolved in the water. We measured the number of neutrons in the water with and without uranium. We found more neutrons were produced than were absorbed by using uranium. Placzek visited us and asked what we were doing. Suggested we use helium and not water. His idea was that if we used a greater proportion of uranium we might find resonance-~~of~~ absorption was so great as to destroy the evidence of a chain reaction. The original experiment proved that more neutrons are produced than are absorbed by using uranium, not more neutrons are produced than are absorbed by using uraning plus water. At that time we were working against a deadline because Fermi had to leave in June for Ann Arbor. We modified our aims and agreed that Fermi should calculate what is absorbed by resonance in the experiment. We needed that value, which he calculated to be 1.5. We wanted to at least get the number of neutrons emitted by uranium to the ratio of neutrons absorbed. Fermi noticed that resonance absorption was smaller than in a homogeneous mixture. Thought he could change ~~this~~ the dimensions. By that time I lost all interest in water. I began to dream of the graphite system.

Darrow restates:

Up to and including part of June 1939, Fermi was working with lumps of uranium in water and was varying the sizes of the lumps in the hope of minimizing the resonance absorption. Meanwhile Szilard had become totally discouraged with the use of water and was beginning to contemplate the use of carbon. In the first ten days of July, Szilard became convinced that the chain reaction could be accomplished with graphite. This is attested by letters from Szilard to Fermi dated July 3 and July 8, 1939, in which at first a method of measuring the cross section of carbon was proposed to find out whether this cross section was so small as to be really promising. Then it was suggested that an attempt be made to produce a chain reaction in a mixture of carbon and uranium without waiting for the outcome of the foregoing experiment. (.3 is now the cross section of hydrogen.)

Fermi received Szilard's proposal with very tempered enthusiasm ~~on the basis that he~~ because he had made calculations on the homogeneous mixture -- which was the wrong thing to do.

Szilard: Does not want following remarks included in which he tells of necessity for haste and slowness of negotiations between committee which the

President appointed (Briggs chairman) and Dean Pegram at Columbia. Mentions that from June 1939 until March 1940 Fermi has been working on cosmic rays, and all that time nothing had been done on fission. By middle of February 1940 they were still not able to get a statement as to whether or not the graphite would be ordered for them. To push things along Szilard wrote up his results and opinions at the time and sent them to the Physical Review with instructions not to publish until notified to do so. This unpublished article is essentially the report A-55. Meanwhile Fermi calculated the lattice experiment, and confirmed Szilard's view that one gains a lot by using lattice in a homogeneous system.

Darrow restates:

In March 1940, graphite arrived and experiments were started in a cube of pure graphite such as is now called a $\Sigma$ pile. The experiment as was carried out was not an accurate one. Got .03 for cross section. The value found, although inaccurate, was low enough to give optimism. The theory at the time was still in an incipient state but in June 1940 Wigner proposed the method of computing thermal utilization, which perhaps with slight modifications is still employed. Wigner suggested boundary conditions but Fermi worked out the actual computation and he and Teller worked it out in the summer of 1940. The assumption about resonance absorption was that it takes place an surface of uranium. This assumption was made in Szilard's paper. Improvement was made over thermal absorption mentioned in Szilard's paper, no improvement was made over resonance absorption. The net result was worse than if it had not been attempted.

Interview adjourned

until Feb. 13, at 2:00 P.M.

from Darrow file
Lbs.



Continuation of Interview on Early History of Project

2/13/45

Szilard: Late in 1941 the next advance was made by Mr. Wigner. Mr. Wigner proved his point that the resonance absorption takes place not only in the thin layer of a surface of the sphere but takes place throughout the whole mass of the sphere. That is that the mass absorption term is important and in certain circumstances it is the dominating term.

Darrow: In other words this question of resonance bears on the evaluation of p?

Szilard: Yes.

This conclusion reached by Mr. Wigner was supported by measurements of Szilard and Marshall who found a large capture cross section for photo-neutrons in $U^{238}$. Marshall and Szilard had also shown in the fall of 1941 that fission is produced in $U^{238}$ by fission neutrons which indicated that fast neutron fission in $U^{238}$ may be an important correction in the neutron balance of the chain reaction.

Darrow: Was this the first observation of fast fission?

Szilard: Was first observation of fission in $U^{238}$ induced by fission neutrons. Slow neutrons cannot produce fission in $U^{238}$.

Thus by end of 1941 all factors which enter into calculating the neutron balance of the chain reaction in the uranium graphite system were known in principle although the accuracy of some of these factors was still not very great.

Darrow: By the factors do you mean specifically $\eta$, $\epsilon$, p, and f?

Szilard: Yes.

In the spring of 1941 enough uranium oxide and graphite was accumulated to make an empirical determination of the multiplication factor of a uranium oxide graphite lattice. This was done by Mr. Fermi who designed a method called the exponential experiment which applies to a uranium lattice the same principles as were used for determining the graphite absorption in the $\Sigma$ pile experiments.

Darrow: Asks if by empirical Mr. Szilard means empirical method signifies getting k by use of the formula involving the Laplacian and the migration area. Szilard agrees to this statement.

Szilard: Throughout 1941 my main concern was to make arrangements for obtaining materials in the required purity and in making provisions for the future supply of pure graphite and uranium metal. It did not seem probable that any important results could be based on the use of uranium oxide.

Another point of concern was the possibility that fast neutron chain reaction might be possible in uranium metal. That possibility, if it had been real, would have constituted a very important danger to the security in this country. In order to increase our knowledge in this respect, Mr. Zinn and I measured the inelastic scattering cross section of uranium. The value which we found was so high that it effectively reassured us that fast neutron chain reaction in all uranium metal appeared to be a rather unlikely possibility.

A very important step in clarifying the over-all picture was a manuscript which was sent to us by Mr. L. A. Turner in the spring of 1940. This drew attention to the importance of element 94 and contained particular significance after the work of Abelson and McMahon made it appear almost certain that 94 can be chemically produced and precipitated.

Darrow: This is very interesting. If Turner had not found these results, you feel your work might now have progressed as it did?

Szilard: Yes. Turner's activity encouraged me to hope that $U^{238}$ would be ultimately utilized in place of $U^{235}$. It also became clear that one could produce element 94 and use it for any purposes for which

$U^{235}$ might be used. In spite of this, to my knowledge, no one in this country had ever made any estimate up to the middle of 1941 which would show that amounts of $U^{235}$ or 94 which we could hope to manufacture would be sufficient for the construction of bombs that would be detonated on the basis of fast neutron chain reaction. In order to estimate the amount needed for such a bomb one had to know the fission cross section of $U^{235}$ for photo-neutrons. An experiment determining this cross section had in fact been performed by Mr. Zinn and myself as early as the summer of 1939. However, our measurements were never properly evaluated since up to the middle of 1941 we were not aware of the fact that there was a good prospect of separating the $U^{235}$ isotope. This was due to the compartmentalization of information which prevented Mr. Fermi and I from talking about this work with Mr. Urey. Thus the fact that atomic bombs are a practical possibility was not brought to the attention of this (U.S.) government by anyone in this country, but was communicated by the British government and, so far as I know, was due to the collaboration of Frisch and Peierls, one of whom carried out the neutron measurements and the other of whom was concerned with the practicability of the separation of $U^{235}$.

Darrow: Asks if British made any publications about this.

Szilard: No. This brings the information up to the beginning of the Chicago Project in 1941.

[illegible]

The thought that such a thing as a nuclear chain might be possible first occurred to me in 1933~~-1934~~. At that time the mass of beryllium as measured by Bainbridge indicated that beryllium is a metastable element which could potentially disintegrate with a release of energy into two alpha-particles and a neutron. It appeared conceivable that a slow neutron when interacting with a beryllium nucleus might induce such a disintegration and that in a sufficiently large structure of beryllium a nuclear chain reaction could be maintained. It appeared also conceivable that other elements such as thorium and uranium might be metastable in the same sense as beryllium~~, and~~. In 1934 I worked out the differential equation which controls the diffusion of neutrons and which determines the critical dimensions in such a chain reacting ~~mass~~ structure. These considerations were written down in the course of 1934 in the form of British patent applications and the resulting patent was assigned to the British Admiralty and was sealed secret.

It appeared likely that beryllium, if it was metastable, could be split by gamma rays of radium so that beryllium exposed to gamma rays of radium would emit neutrons which could be detected by virtue of the radioactivity which they would induce. Mr. Chalmers and I found in fact such a neutron emission from beryllium under the action of gamma rays of radium (at present called photo neutrons) ~~but~~ subsequent experiments Goldhaber and carried out in collaboration with Brasch and Lange showed that the photodisintegration of beryllium had an energy threshhold of about 1.6 million volts~~which was~~. This was difficult to reconcile with the condeption that beryllium was a metastable element which could sustain a chain reaction. ~~SEM~~ Subsequently Bethe and Oliphant showed that Astons mass of the alpha particle was in error and there was ~~farther~~ no longer any reason to suspect beryllium of being metastable.

I did not, ~~however~~, give up the idea that other elements might be metastable and might release fast neutrons when interacting with neutrons so that the chain reaction might be maintained. But as I had no conception of the possibility of fission, ~~however, and~~ was ~~therefore~~ unable to think of a mechanism which would permit such a neutron emission except by assuming that there might be neutrons of mass number 2 or 4 having a sufficient binding energy to make it possible for them to be emitted in an exothermic process from certain nuclei when they capture a neutron.

I had now the plan of making a systematic search for some such neutron emission by using the photoneutrons from beryllium as a tool. These photoneutrons have a sharp upper limit to their energy and if fast or secondary neutrons were emitted by any element which is exposed to these photoneutrons the secondary neutrons could be distinguished from the primary neutrons by virtue of their higher energies. In preparation for such experiments I had cast ~~xx~~ a cylindrical beryllium block of 6 cm diameter and 6 cm high with a cylindrical hole in the center so that one gram of radium could be inserted in the center giving a strong source of photoneutrons. I also had built a linear amplifier in order to detect such fast secondary neutron emission by amplifying the pulses induced by means of the hydrogen recoils in an ionization chamber. All this equipment was at Oxford but the amplifier was never actually made to work and the plan for such a systematic search was virtually abandoned before I came to the United States in 1938?.

2.) If first heard of the discovery of fission from Wigner at Princeton during the first half of January, 1939 about ten days before the Washington meeting. 5 ~~and~~ told ~~him~~ Wigner that I expected ~~this phenomenon~~ fission to be accompanied by a neutron emission which should be easily observable if the fission were induced by photoneutrons slowed down by paraffin. I cabled to Oxford requesting them to send the beryllium block which had been procured for the purpose and after a short illness contacted Rabi, Pegram, and Fermi at Columbia with proposals which were based on the assumption that a chain reaction might be set up in uranium. Fermi, who had independently thought of the possibility of a neutron emission accompanying fission and discussed this point ~~intended to see whether this phenomenon existed,~~ at the Washington meeting in January planned an experiment using radium-beryllium neutrons from which he ~~had~~ hoped he might be able to conclude whether there is an excess of neutron emission over neutron absorption from uranium by measuring the total number of neutrons emitted from a sphere of uranium oxide when a radium-beryllium neutron source is placed at the center.

Fermi had independently thought of the possibility of a neutron emission accompanying fission and discussed this possibility at the Washington meeting in January. Soon afterwards x x

→ U [illegible]

The beryllium block having arrived from Oxford, I obtained a gram of radium and with this photoneutron source Zinn and I irradiated uranium oxide and found that fast neutrons (about 2 per fission) were emitted from the uranium. This experiment was performed on March 3 and 4, 1939.

About the same time Fermi was engaged in an experiment that was also designed to obtain information concerning a possible neutron emission from fission. Fermi and Anderson used a fast neutron radium-beryllium source, slowed down the neutrons in a water tank, and measured the thermal neutron density in the tank as a function of the distance from the source. He then surrounded his neutron source with a sphere of uranium oxide and by measuring the thermal neutron and again measured the thermal density. These two measurements were supposed to show whether or not the uranium brought about an increase in the total number of neutrons slowed down and absorbed in the water. Such an increase would have meant that uranium emits neutrons in fission and also showed that more neutrons are emitted than absorbed in uranium under certain conditions. This would have been a favorable omen for the possibility of a chain reaction in a uranium-water system.

Fermi and Anderson indeed observed an increase in the number of thermal neutrons but they were unable to interpret the result because they could not exclude the possibility of an n, 2n reaction caused by the fast neutrons of their neutron source. The photoneutron source ~~ix~~ used in my experiments with Zinn was then turned over to Anderson and Fermi and by using this source they were able to demonstrate that for large distances from the source there was a very pronounced increase in the neutron density if they introduced uranium oxide into their system which they considered conclusive proof of the fact that neutrons are emitted in fission by uranium which are more energetic and therefore have a longer range than the photo-neutrons of the source.

3. In May and June 1939 Anderson and Fermi and I performed an experiment in which we observed the number of neutrons emitted from a cylindrical lattice of uranium oxide filled tubes. This lattice was immersed in water and was exposed to a photoneutron source in the center of the arrangement. We found that the number of neutrons which were slowed down and absorbed in water was about 20% greater in the presence of the uranium lattice than in its absence. While we were doing this experiment we had hoped that such a result might enable us to conclude that the chain reaction could be maintained in a water-uranium system since the result meant that more neutrons are emitted by uranium under the conditions of this experiment than are absorbed by uranium. Of course, a very large fraction of the neutrons were absorbed by the water, but we tacitly assumed that by increasing the amount of uranium and reducing the amount of water the fraction of neutrons absorbed by the water could be reduced as much as desired. While we were engaged in the measurement Placzek came to visit us and drew our attention to the fact that no such conclusion would be possible on the basis of our experiment. He pointed out that if we increase the amount of uranium and decrease the amount of water we would increase the fraction of neutrons absorbed by uranium at

resonance and thereby decrease the number of neutrons emitted by uranium per neutron absorbed by uranium. Placzek was skeptical about the possibility of having a chain reaction in a uranium-water system and advocated the use of helium for slowing down the neutrons in the place of hydrogen.

Realizing that we could not decide by means of our experiment whether or not a chain reaction could be maintained in uranium and water experiments, we decided to make use of the experiment for calculating the number of neutrons emitted from uranium per thermal neutron absorbed by uranium, a number which is a constant of uranium and which is independent of the water-uranium ratio. In order to do this we had to calculate the fraction of the neutrons which was absorbed at resonance by uranium under the conditions of our experiment and this calculation was actually carried out by Fermi. The calculation showed that the fraction of neutrons absorbed by uranium at resonance in our system was smaller than the value that would hold for the corresponding homogeneous mixture of uranium and water and this observation led Anderson and Fermi to hope that perhaps by skillfully choosing the characteristics of such a heterogeneous water system one could obtain conditions in which a chain reaction could be expected to go in a uranium water system. The advantage which they were able to obtain over a homogeneous water system was fairly small and not sufficient to enable them to conclude that a chain reaction could be maintained in any such system. While Anderson and Fermi were engaged in these calculations I became interested in using graphite as a slowing down agent in place of water. Between July 1 and July 15 I reached the conclusion that there was a very good chance of maintaining a chain reaction in graphite, that the use of a heterogeneous system, particularly a lattice of uranium metal spheres in graphite gave an enormous advantage in the multiplication factor in graphite and that graphite in the required purity was available at a moderate price. I devised an experiment for measuring the diffusion length in graphite (rather than measuring the absorption cross section of carbon. While I wanted to perform this experiment I pressed for making arrangements for a large scale experiment with a lattice of uranium and graphite without ~~xxxx~~ waiting for the ~~xx~~ outcome of the measurement of the graphite absorption. During this period I bombarded Fermi with letters on this subject and had conversations with Pegram, Wigner, Teller and Einstein. I was so optimistic about the possibility of using graphite that based upon this technique an approach was made to the U. S. Government which led to the appointment of the Uranium Committee under the chairmanship of Lyman J. Briggs, which on October 21, 1939 promised to supply four tons of graphite for the ~~xxxxxx~~ proposed absorption measurement.

My optimism with respect to graphite was based on the view that a water-uranium system, even though not chain reacting, may come fairly close to the multiplication factor of 1. From the slowing down and scattering properties of carbon which were known it was possible to deduce that without taking full advantage of the properties of a heterogeneous uranium-graphite system the multiplication factor of a carbon-uranium system would be about the same as of the best uranium-water system if the carbon absorption cross section were .001. This value, however, was the measured upper limit of the carbon absorption cross section and one could hope that the actual carbon absorption would be below this upper limit.

Fermi wrote in July that he had also considered carbon as one of the possibilities. It seems that Fermi considered a system containing very little uranium and very large amounts of carbon and therefore came to the conclusion that the system would be only chain reacting if the carbon absorption were very low. Fermi told me in September that he had actually calculated a homogeneous system which he of course knew to be less good than a heterogeneous system. He did not think at that time that the advantage of the heterogeneous system would be sufficient to give a material change in the overall picture.

ATOMS FOR PEACE AWARDS, INC.
A MEMORIAL TO
HENRY FORD AND EDSEL FORD

*77 Massachusetts Avenue, Cambridge 39, Massachusetts*
*Office of the Executive Secretary: University 4-9870*

FOR RELEASE AFTER DELIVERY
4:00 P.M., WEDNESDAY, MAY 18, 1960

Remarks by

DR. LEO SZILARD

in response to

THE ATOMS FOR PEACE AWARD CITATION

The National Academy of Sciences

Washington, D. C.

Wednesday, May 18, 1960

3:00 p.m.

(WITH BIOGRAPHICAL NOTE)

Mr. Chairman, Honored Trustees.

In 1913, one year before the first World War, H. G. Wells published a book entitled "The World Set Free" in which he predicts the discovery of artificial radioactivity and puts it into the year of 1933, the year in which it actually happened. In this book Wells describes how this discovery is followed by the release of atomic energy on an industrial scale, the development of the atomic bomb and a world war which is fought with such bombs. London, Paris, Chicago and many other cities are destroyed in this war, which Wells puts into the year of 1956. I read this book in 1932, before I myself had done any work in the field of nuclear physics.

In 1933, I went to live in London. In the fall of that year, the London papers reported a speech given by Lord Rutherford at a meeting of the British Association, in which he said that whoever talked of the release of atomic energy on an industrial scale was talking moonshine. I was pondering about this while strolling through the streets of London. On that occasion, it occured to me that Rutherford might be wrong, because there might exist an instable element that splits off neutrons when bombarded by neutrons and such an element could sustain a nuclear chain reaction. On the basis of the published masses of helium and beryllium, the beryllium nucleus should have been instable and it could have disintegrated into two alpha particles and one neutron, when hit by a neutron.

At this time, I was playing with the idea of shifting to biology. But the possibilities opened up by these thoughts were so intriguing that I moved into nuclear physics instead.

In the summer of 1934, T. A. Chalmers and I looked into the mystery of beryllium and we found that beryllium emits neutrons when exposed to the gamma rays of radium. Other experiments showed, however, that gamma rays of lower energy were incapable of splitting beryllium and this made it appear doubtful that beryllium could sustain a nuclear chain reaction.

Nevertheless, the thought that some element or other might be capable of sustaining such a chain reaction stayed with me and I pursued it from time to time without success until I finally gave up hope in the fall of 1938. In December of 1938, I so advised the British Admiralty, to whom I had previously assigned a secret British patent which described the general laws governing nuclear chain reactions. One month later, I visited Wigner, who was ill with jaundice in Princeton. On that occasion I learned from him that Hahn and Strassman had found that the uranium nucleus breaks into two heavy fragments when it absorbs a neutron. To me, it appeared at once very likely that these fragments would evaporate neutrons and this meant that uranium might sustain a chain reaction. "H. G. Wells, here we come!" - I said to myself. Neither Wigner nor I had much doubt at that time that we were on the threshold of a World War. Finding out whether neutrons are emitted in the fission of uranium appeared to us, therefore, as a matter of great urgency.

The rest is history.

In 1945, as the war drew to its end, one of the younger staff members came into my office at the Uranium Project at the University of Chicago and said that he felt it was a mistake that so much emphasis was placed on the bomb and that we were not paying sufficient attention to the peacetime applications of atomic energy. "What particular peacetime applications do you have in mind?" I asked him, and he said, "The driving of battleships."

I often told this story after the war as a joke. These days, when we are rapidly moving towards the so-called atomic stalemate, it seems to me that the story is even better, because it now represents two jokes rather than just one.

It is not always easy to say what is or is not a peacetime application of atomic energy, but if the large-scale liberation of atomic energy which we have achieved abolishes war, as it well may, the distinction will cease to be important. If war is to be abolished, the nations of the world must either enter into a formal agreement to get rid of the bomb or they must reach a meeting of the minds on how to live with the bomb. So far, we have not made much progress in either direction. It seems likely, however, that, as far as America is concerned, she will be forced to decide in favor of one or the other of these two alternatives during the term of office of the next President, and either decision might be better than no decision.

<u>BIOGRAPHICAL NOTE:</u> Leo Szilard was born in Budapest, Hungary on February 11, 1898. After studies at the Budapest Institute of Technology, he earned a Dr. Phil. degree at the University of Berlin (Germany) in 1922. He held various academic posts in Germany and England before coming to the United States in 1937. He was an active developer of radiation chemistry--the use of radioactive elements to influence the course of chemical reactions. He taught and conducted research in nuclear reactions at Columbia University, publishing papers in collaboration with the late Enrico Fermi and Walter Zinn among others. He was an early proponent of large scale research, sponsored by the government, into the possibility of nuclear chain reactions and, with Eugene Wigner, composed the famous letter to Franklin D. Roosevelt from Albert Einstein which was instrumental in establishing the "Uranium" project. His participation in the work of this project at Columbia and at the University of Chicago from 1938 to 1946 resulted in his being

granted (with Fermi) the first U.S. patent on nuclear chain reactions. Since 1946 he has been Professor of Bio-Physics at Chicago and has made important contributions to the control of the culture of micro-organisms (with Aaron Novick), which in turn have led to significant discoveries in the field of genetics. Professor Szilard is a Fellow of the American Physical Society. In 1951, he was married to Dr. Gertrud Weiss, who is a member of the faculty of the University of Denver.

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4:00 P.M., WEDNESDAY, MAY 18, 1960

# THE ATOMS FOR PEACE AWARD CITATION

for

LEO SZILARD

By your wide-ranging interest in science and in the implications of scientific advance for human welfare, you have made significant contributions to man's search for knowledge and for peace.

Your studies of the nature of nuclear reactions led to an anticipation of the possibility of producing useful power by using the forces within the atom. With colleagues, some of whom we also honor today, you carried these studies to the attainment of the controlled nuclear chain reaction-- the basis of the rapid development of atomic science in the past two decades.

By your zeal and devotion to your work, you attracted promising young scientists to the new field and imbued them with your vision of its potentiality.

You have been untiring in your efforts to arouse men of all nations to the social and political implications of atomic energy.

May this medal, symbolizing the Atoms for Peace Award, signify to all men the importance of your contributions "for the benefit of mankind".

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