

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:*

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Discussion

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 γ -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 γ -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 γ -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 γ -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 γ -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).

Artificial radioactivity

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the other half represents the natural background effect of the counter. Apparently the γ -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 γ -ray disintegration we get by means of very simple experiments some information both regarding the processes involved in the Fermi effect and in the γ -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 γ -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.

c/o Clarendon Laboratory,
Parks Road,
Oxford.

5th December, 1935.

Dear Goldhaber,

Enclosed you find my manuscript to "Nature", please read it carefully and make such corrections as you think necessary and send it back to me to Oxford. I am sending you a second copy to be kept by you or by any of your friends in the Cavendish Laboratory.

The absorption experiments in boron to which the last paragraph refers, have in the meantime been carried out. If we assume that the Fermi, Bethe, Perrin and Elsasser theory holds for boron and lithium, though it does not hold for elements which show a strong radiative capture, we can conclude that indium has a selective absorbing region at a neutron energy of the order of magnitude of three volts.

With kind regards to all,

Yours sincerely,

H. St. J.

c/o Clarendon Laboratory
Park Road,
Oxford.

16th June, 1935.

Dear Professor Singer,

When I last saw you I told you about a plan to start research in a direction which may or may not be the starting point of a new industrial revolution. I thought that it might be possible to get financial support for such work, but abandoned any attempt in that direction after a few tentative discussions with you and a few others. I plunged into experimental work instead in which I was very lucky, and you see the results from the enclosed reprints. In the course of this work, I observed certain anomalies, the implications of which I did not realise until about four weeks ago.

There is no doubt left that these observations settle one of the two crucial points which determine whether or not the development will lead to a sort of industrial revolution in the near future, and settle it in a affirmative sense. The second crucial point is still unsettled but leaves the way open for only two alternatives. So ~~that~~ we may fairly say of having something like a 50/50 chance for getting an answer in an affirmative sense. This, I believe, has to be considered a very high chance in view of the issue which is involved, and even if this chance were less we would have every reason to take action on it.

Let me tell you which steps I have taken until now. In March last year, it appeared advisable to envisage the possibility that

the recent discoveries in the field of nuclear physics might enable us to liberate energy on a large scale and to store energy through the whole-sale production of radio-active bodies. At that time, I did not see my way for a direct experimental attack but, seeing the outlines and the importance of the two crucial points which I mentioned above, I applied for a number of patents dealing with methods and apparatus involved in the prospective industrial application. These patents were very lucky in foreshadowing the subsequent development. The first of these patents, for instance, protects the production of radio-active bodies through neutron bombardement and can be considered as a basic patent. A month or so, after this patent had been filed Fermi discovered that radio-active bodies can be produced through neutron bombardement and a rapid development started when these radio-active bodies were used as a tool for further research. I myself got in August last year permission to use the radium in St. Bartholomew's Hospital during the holidays, and jumped ^{into} to experimental work. This work went so well that I extended it beyond the holidays until the end of the year and went then, after a six weeks visit to Oxford, to New York. Not until I was in America did I realise the full significance of my London observations. About the same time, I received a definite offer of an appointment for three years from the Clarendon Laboratory and so I came to London a fortnight ago.

Here I informed Prof. Lindemann, who is in charge of the Clarendon Laboratory of the situation which has arisen and raised the question of fitting in my plans with the other work. I told him that I wish to keep the experiments in my own hands and to have all the freedom of movement which is necessary at this juncture. Prof. Lindemann promised me his support and all the facilities which were previously granted for my work in Oxford, the biggest item of which is the use of £ 15 000 worth radium.

In addition of the facilities which are available right now in Oxford? I should require for the work a budget between £ 600 and £ 1 200 a year and should attempt to raise in the first instance £ 600 per year for three years, before actually starting the work. I thought it better not to ask Lindemann to take the initiative in the matter and so, once more, I am looking for a Maecenas.

While I am fully aware of the difficulties to raise such a sum, I am inclined to think that ultimately it will be possible to find somebody who has sufficient imagination to grasp the situation and who is, at the same time, in a financial position to help in this matter.

Should you be able to think of somebody whom you know personally and of whom you think that he could be approached? Please, do let me know. I am very anxious though to keep this matter quiet and this letter is only intended for your information.

I may add that it is not intended to approach industrial companies and that, though every effort will be made, to get patent protection and that, though these patents may be exploited, on a profit basis it would be misplaced to apply in this matter primarily commercial considerations. The patents might be handed over to a body which would administer them along commercial lines, but would use the profit for constructive purposes. A precedent for this, though on a small scale, is the Research Corporation in New York, which was created in 1911 for the administration of the patent rights handed over to them by F.G. Cottrell and which has since grown into an active business organisation. While it is premature to discuss these things at present it is ^{necessary} ~~better~~ to mention them in order to explain that it is not possible to offer the patent right to private persons or to draw up any commercial agreements about them, though an adequate for invested funds need not be excluded.

I have also to add, in order to give you a complete picture, that the disaster to which this development can lead may be more imminent than the industrial revolution which it may bring about, and that, from this point of view, an attempt will have to be made to keep the patents secret and gradually to bring about something like a conspiracy of those scientists who work in this field.

Yours sincerely,

Hirst-J (R-46)

c/o Clarendon Laboratory,
Park Road,
Oxford.

9th June 1955.

Dear Professor Singer,

I should be very pleased indeed to see you again, and perhaps you and your niece would care to have lunch with me some time in the second half of June. Please let me know after you have settled in London which day would suit you best. I have just arrived from America and may settle in Oxford where I have been offered a three years' appointment in the Clarendon Laboratory. I can tell you more about these personal things which have worked out fairly well when I see you, but I had better write to you now about a matter of great earnestness in which you may or may not be able to help.

When I last saw you I told you about a plan to start re-
search in a direction which ~~might possibly~~ be the starting point of a new industrial revolution. I thought that it might be possible to get financial support for such work, but abandoned any attempt in that direction after a few tentative discussions with you and a few others. I plunged into experimental work instead in which I was very lucky, and you see the results from the enclosed reprints. In the course of this work I observed certain anomalies, the implications of which I did not realise until about four weeks ago. ^{may or may not} To-day the position is this: I can demonstrate the crucial points on which I based my expectation that an industrial revolution may be brought about in the immediate future. Other points remain to be settled before we can say anything with certainty, but it may be fair to say that there is a fifty-to-fifty chance that these other points will work out alright. Even if these chances are smaller than I anticipate, they are certainly large enough to get excited at this juncture.

The disaster to which all this may lead is more imminent than the pleasant changes it may bring about, since applications for purposes of war are closer at hand than anything else and go beyond anything one is likely to conceive. An attempt to control this development will have to be made, however small the chances of success may be, and the most essential question in this respect is whether it will be possible to get the physicists in America and England to take precautions about publishing observations which fall into this dangerous zone, at a time

Must one of the two

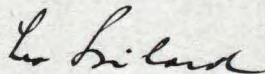
when such precautions necessarily seem to be premature. Unfortunately it will appear to many people premature to take some action until it will be too late to take any action. It will take some time until one can see what practical steps have a chance of being put across.

In the meantime I shall make an attempt to make further experiments in order to settle as quickly as possible those questions which are still open. I informed yesterday Professor Lindemann who is in charge of the Clarendon Laboratory in Oxford of all this, and suggested that I should devote my time and the facilities which I have been previously promised in Oxford, to this task, that the limitations which would interfere with my freedom of action in this matter, should be removed, and told him that I should make an attempt to get further facilities from outside-sources. I know from my friends that the budget of the Clarendon Laboratory has been strained, and it would not be wise to embark on this venture without having the necessary equipment. I should rather not ask professor Lindemann to approach government institutions or private individuals, and I should rather see what I can do in this respect myself. As far as Oxford is concerned the path is clear. I should think that facilities of about £1000.-- per year will be required for one or two years. This money would be used in two ways. For salaries of two men of the order of £200.- to £ 300.- a year each, who would be required to assist this work, and the rest would be used for buying equipment. There is already some equipment for this work in Oxford, the biggest item being £15,000.- worth of radium.

I am fully aware of the difficulty of getting hold of a private person who has some vision and who is able to provide part or the whole of the required sum. On the other hand I cannot believe that it should be impossible to find somebody in Great Britain. Should you be able to think of anybody whom you think one could approach and whom you happen to know personally, I should appreciate very much if you could let me know.

If it is not possible to get the facilities here, I shall have to make an attempt in America, but I can hardly do so before september, since everybody is leaving New York at this time of the year.

Yours sincerely



Leo Szilard

Clarendon Laboratory
Parks Road
Oxford
Telephon:3545

16th June, 1935.

Dear Professor Singer,

I believe I have not seen you since I had lunch with you and your niece sometimes in June last year. Many things have happened since then.

I arrived from America a fortnight ago and I may settle in Oxford where I have been offered a three years appointment in the Clarendon Laboratory.

I should be very pleased if you and your niece would care to have lunch with me if you happen to be in town. Please let me know which day would suit you best. I am in London once or twice a week and if so, I am staying at the Harewood Hotel, 74/8 Gower Street, Telephone:Museum 3941.

To-day I should like to write to you about another matter and I do this by a separate letter. I wish it would be as easy as that to keep private and public life in watertight compartments.

With kindest regards,

Yours sincerely,

365187

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12th February, 1935.

031898

By Hand.

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

Dear Dr. Szilard,

I have to acknowledge receipt of your letter of the 10th inst., enclosing our letter to you of the 10th January 1935, embodying an agreement between us in regard to your invention for the reproduction of books, duly countersigned by you.

In reply to the points raised by you in your letter of the 10th February, I have to reply as follows:-

1. The final paragraph, ^{of our letter of the 10th January 1935} beginning with the words "alternatively, this licence agreement" is not intended to affect the minimum royalty referred to in paragraph (b) of Clause 2, in our letter of the 10th January 1935. *A. Benjamin*

2. It was certainly not our intention when drawing up paragraph 2 (f), to ask that you give us the first offer of refusal only on those of your future patents which fall under the claims of the patents mentioned in paragraph (1) of our letter of the 10th January. The intention of this paragraph was that you should give us the first offer of all your future inventions having any relation or bearing on the broad subject of the "Microbook". You are interpreting the words "relating to the invention, the subject of this agreement" in too narrow a sense.

Dr. Leo Szilard.

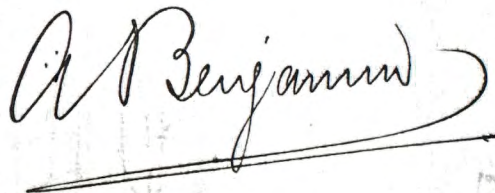
There cannot be any hardship to you whatever in making us the first offer, in accordance with paragraph (f), nor will there be any difficulty in your remembering this condition in future, seeing that this agreement about the "Microbook" is the only one that can come into question between us.

3. We will, without any liability for inadvertence or oversight, take care of the British patent application forthwith, and bear all costs in connection with its prosecution until grant, irrespective of whether this is during the optional period or the period of the licence.

As regards corresponding applications in other countries, we will bear the cost of filing those cases which have been submitted to us by you beforehand, and in regard to which we have decided to file such application or applications, and (again without liability for inadvertence or oversight) will take care of the prosecution of such cases.

I will have the counterpart of the letter of agreement signed on behalf of the Company as soon as possible, and shall be glad if you will let me know the address to which it should be sent (as well as the cheque for £30 to yourself), in case, as seems probable, it cannot reach you before you leave England.

Yours very truly,



AB/VT.

Strand Palace Hotel,
Strand, London W.C.2.

14th February, 1935.

Your Ref. 031998.

A. Benjamin, Esq.,
Electric & Musical Industries Ltd.,
Blyth Road,
Hayes, Middlesex.

Dear Mr. Benjamin,

I had your letter of the 12th instant.
I regret not to be able to accept your interpretation
of paragraph 2 (f), since the first paragraph of
the agreement clearly defines the scope of the agreement
as limited to one patent application, and those further
applications which can be derived in following up this
application.

However, while I should like to interpret
paragraph 2 (f) so as to keep it within the scope,
I am quite willing to meet your wishes insofar as I
herewith agree to the following:

If I file any patents, the primary object of
which is the 'Microbook' in England in 1935 or 1936
I shall let you have the first offer in the same way as
if these/^{patent}applications ~~would have~~ came under paragraph 2 (f)

A. Benjamin, Esq.

-2-

14th February, 1935.

of our agreement. In consideration of this you may,
if for legal reasons you think it advisable, pay me 1/-.

Yours very truly,

Strand Palace Hotel,
Strand, London, W.C.2.

6th January, 1935.

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.eomvms ni word boy.

Dear Goldhaber,

I came back to-day from Oxford for the weekend and heard from Professor Hopwood over the telephone that he wishes that a letter should be sent to "Nature" ^{immediately} /about our heavy water experiments. I told him that as far as I know, you did some work about this which you intend to publish in the Cambridge Proc. and that I should not like to rush in with a letter to "Nature". He seemed to insist on the matter - all over the telephone - (a letter which he wrote to me on the subject did not reach me in Oxford) and it seemed advisable for me to go as far as I can go, considering that I am a guest of his department, in saying that I am not prepared to sign such a "letter" and I should appreciate it if my name were not mentioned in the "letter", if they decide to send one. I do not know what they will decide in the circumstances, in any case, I will not be a party to it. I shall see Hopwood tomorrow morning; perhaps he will see my point of view by then, ~~xxxxixdxmxxxxixx~~ I wonder.

I got the advice note for 1 gramme of indium and you will have the stuff at the latest on Wednesday. I hope you are able to look at it at once

British Patent Office
London, W.C.2.

I may need the indium in ten days or so and shall let
you know in advance.

Dear Goldhaber,

I am sorry to hear from Oxford for the
weekend and heard from Professor Hopwood over the tele-
phone that he wishes that a letter should be sent to
"Nature" ^{immediately} about our heavy water experiments. I told
him that as far as I know, you did some work about this
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what they will decide in the circumstances, in any case,
I will not be a party to it. I shall see Hopwood
tomorrow morning; perhaps he will see my point of view
by then, xxxxxxxxxxxxxxxx I wonder.

I got the advice note for I examine of
indium and you will have the result at the latest on
Wednesday. I hope you are able to look at it at once

Strand Palace Hotel,
Strand, London W.C.2.

15th January, 1935.

Dear Goldhaber,

I understand that Professor Hopwood, Mr. Banks and Mr. Chalmers sent a letter to 'Nature' about the disintegration of heavy water by gamma rays of radium which will appear in this week's issue. I wish to inform you of the following:

Chalmers and I observed in October or November very weak effects with sources which were not free from neutrons. Since I did not think it urgent to follow up the matter I proposed to let it stand over and take it up again as soon as we have neutron free sources. When I heard through a visitor that you and Dr. Chadwick did similar work on heavy water there seemed to be not much point in going on with those experiments from the point of view of increasing our knowledge. However, I decided in the circumstances to carry on just the same thinking that if we have definite results in the normal course of our work at a time when you have not yet published, we can arrange that neither of us puts in in front of the other. It so happened that no neutron free sources were available until the time when I moved

to Oxford in January. This as well as some other work which I have started being unfinished, it required some sort of arrangement as to which of us should carry it on if we did not choose to carry it on jointly.

As far as this work was concerned Professor Hopwood quite rightly assumed that I would not care to go on with it myself whereas I know that he had taken great interest in it from the beginning and co-operated with it all the time. Nevertheless I was surprised to hear that the experiments have been repeated with the old sources and that a letter was about to be sent to 'Nature' about them. I was asked whether I wanted to sign it or to be mentioned in it in some other way. I answered that since you have completed your work much earlier and have chosen a slower way of publishing we must not cut in in front of you by a letter to 'Nature' and that for this reason I could not participate in any way in the matter and that if a letter is sent to 'Nature' it is sent without my consent. I told Professor Hopwood, Mr. Banks and Mr. Chalmers that in spite of the minor material importance of the issue I wished to take the strongest possible stand on it by writing you this letter of which I am letting them have copies.

Yours sincerely,

B or C

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To.....M

THE INFORMATION OVERLEAF WILL INTEREST YOU.

Kind regards
S. J. Lard