

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

6th January, 1935.

I may need the indium in ten days or so and shall let

You know in advance.

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 ^{immediately} to "Nature" /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, ~~xxxxxxx~~ 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

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

6th January, 1935.
I shall do so or say no to it in midday and I am
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" 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 Press and
that I should not like to rush in with a letter to
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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, xxxxxxxxxxxx I wonder.

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

R-1
x
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 these 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 cuts 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 knew 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,

[Goldhaber, Maurice]

46, Belvoir Rd
Cambridge

18. 3. 36.

Dear S.,

Somebody sent a copy of your retraction
patents to the Lab. It was freely discussed
at tea and, of course, your intentions were
misunderstood to be financially or otherwise
unscientifically. I defended you by saying
that in your opinion it is better to direct
technical applications of science in a way which
will ~~be~~ be useful to science and not dama-
ging to the public. There is no reason for you
to worry about this.

In great hurry

Yours

M.G.

UNIVERSITY OF ILLINOIS
DEPARTMENT OF PHYSICS
URBANA, Ill.

Urbana
Tel: 7-3461

September 23, 1938

Dr. L. Szilard
King's Crown Hotel
420 West 116th Street
New York City

Dear Szilard,

A member of our department, Dr. D.W. Kerst, has worked out mathematically a scheme of accelerating electrons by means of a changing magnetic field. This scheme appears to be similar to the one you told me about last year. Unfortunately, I do not know sufficient details of these schemes to know whether they are quite identical.

Dr. Kerst would be very glad to discuss with you this method and also to know whether you have already made any experimental progress. Should you be in Chicago we would be very glad if you could come over here or, if you have no time to do so, perhaps we could arrange to meet some time in Chicago.

Kindest regards,

Yours
M. Goldhaber

M. Goldhaber

MG*DMR

My private address is
Apt 14, 905 South First Street
Champaign, Ill.
(a twin city of Urbana)
Tel: *Champaign 9345*

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publ. for
reproduction

MAGDALENE COLLEGE,
CAMBRIDGE.

June 12, 1939

Dear Fatherland,

We are sailing on Wed,
June 21 on the 'Normandie'
and shall arrive in New
York on Mo, June 26. We
shall stay in New York for
a few days.

I have written to Tuck
and hope I shall be able
to get in touch with him
before we leave. Bledsoe
has been ill, and I am
not certain whether I shall
still see him. I shall not
say much on Uranium.
Everybody seems to be doing
Uranium experiments here

but there is not anything
new to report. Can you
reconcile your and Helber-
holzer's observations on ^{very} fast
neutrons from U - fission?
It seems they have too few ^{counts}
~~numbers~~ to say anything cer-
tain; maybe in their arrange-
ment the fast photo - neutrons
contributed more appreciably
in the disintegration of U
than in yours.

The atmosphere here is dis-
quietingly peaceful. One
seems to be confident that
there will be war war, and
if there is one — it will
find England prepared.
After all, a continuous crisis
would be too much for one's
nerves!

Kind regards from Trude
and also to Trude W.
Yours M. Goldhaber

UNIVERSITY OF ILLINOIS
DEPARTMENT OF PHYSICS
URBANA

April 12, 1939

Dear Sautland,

Thank you for the paper. It reached me just before I was leaving for a trip to Madison, Wisconsin, together with Rabi.

I suppose you would have detected delayed neutrons with the He chamber if these neutrons had an energy of ~ 5 M.e.v. as Roberts et al. estimate. This makes either Roberts' estimate of the cross section for production of the 'delayed neutron' emitter much too large or your multiplication value too small. Roberts' estimate seems on the face of it unlikely high. - Have you any idea how fast your ^{multiplication} neutrons are?

Of course, it is a good idea to keep results secret until it is settled whether the U-bomb is possible, in reasonable dimensions, or not. Let us hope not. But if yes, it is important to be a step ahead of the dictator countries, though I fear it means only a step ahead. Have you thought of definite steps of buying off all U-^(etc.?) supplies? A map of their distribution in the world looks a

little discouraging, though it is only a qualitative one, which I looked up. — I fear you will not be too successful with the attempt to keep results secret. The first indications that people in Berlin are doing similar work appear in *Naturwiss.* ^(arrived to-day) in an article of Droste who with Reddenman is looking for fourth ventricles, when D + D ventricles are used on U. He mentions this at the end of a letter. As soon as the first papers from Germany appear, I am sure, many of those who have agreed to secrecy will see no further good in it, ^{and} ~~but~~ though they are right as far as the ultimate results are concerned, the immediate effect of secrecy is very important. Have you any results on Th?

Talking of 'secrets': Please keep this a secret for the time being. I have become engaged to Trude Scherff, and working on the hypothesis that there will be no war within the next few weeks (a very weak hypothesis, I fear) I have sent her an invitation (from Loomis) to come here on a visitor's visa. When she is here we can get married and ~~then~~ she can immigrate non-quota via Canada. This seems the only workable plan in a hurry.

I have written to Fowler.

Kind regards

Yours M. Goldhaber

S.6.3 7

UNIVERSITY OF ILLINOIS
DEPARTMENT OF PHYSICS
URBANA

May 7, 1939

Dear Hiland,

Forgive the delay in writing you about the $N + n_{slow}$ cross section. I was busy ~~for~~ preparing my trip to Europe. I had no answer to my cable yet, but probably means that Trude has not yet applied for a visa. I had a letter where she suggests waiting till June when she can get her job, and thus also her permit to stay in England, renewed. In this case it is quicker that I go over, and certainly this should simplify matters a lot. So I intend to sail Wednesday, May 24, 1 p.m. on the 'Queen Mary'. I shall arrive in New York ^{May 23} Tuesday afternoon. I shall inform you of any changes of plan immediately.

I hope you received my reprints. ~~Hallén~~, Frisch, Halban & Krod (Copenhagen Academy, or detailed title: Det Kgl. Danske Videnskabsnes Selskab. Matematisk-fysiske Meddelelser. XV, 10).

give for the cross section of $^{14}\text{N} + \alpha_s \rightarrow ^{12}\text{C} + \text{H}$
(1.3 ± 0.3) $\times 10^{-24}$ cm². This may not be suff-
ciently accurate for your comparison
with U. Our cross section = $\frac{1}{300}$ Boron cross
section $\sim 1.66 \times 10^{-24}$ (we used a lower
B cross section in our paper) is not better
than a factor 2. It may therefore be
advisable to compare the fission
cross section with that of B, either
by using low pressure BF₃ in the ion
chamber (mixed with dry O₂, or A) or
by having a chemist produce you
a thin U borate layer (or other U boron
compound) which, if really thin, would
give (a very good value ^{by direct comparison of the number of small & large hits} for σ) believe.

Did the Cd-wheel experiment go alright?
I see from the last 'Nature' that Halban
& Joliot have 'solved' everything. We have
not yet started our boron - fission chamber exp. on angular
distribution
enclosed the receipt.

Yours

Max Goldhaber

Bk 3 x
Hotel King's Crown
420 West 116th Street
New York City

April 18th, 1939

Dear Goldhaber:-

Congratulations!

I assume that you are going to the Washington meeting. If so, wouldn't you like to spend a few days in New York? I am sure you could help us with our experiments if you were here. So try to arrange matters so that you pass through New York either on your way to Washington or on your way back to Urbana.

Though I have written to Joliot early in February, as you will see from the enclosed letter, it was not possible to arrange a co-operation with Paris. Essentially for this reason Pegram has finally decided to let the papers appear, and you will see them in the next issue of Physical Review.

Let me know your plans and also if you succeeded in getting the visitors visa for T. S.

Yours,

(Leo Szilard)

P.S. Please could you send me as soon as possible 50 to 100 reprints of our joint indium paper?