

History A JWS 4

c/o Liebowitz
420 Riverside Drive
New York City

October 21st, 1938

Dear Tuck,

I wonder whether you see the European situation in the same way as I do. I believe that this last breach of faith which lead to the Munich agreement has definitely settled the fate of Europe for a long time to come. It will not be possible for either England or France to make international agreements and get anybody to believe that they will keep these agreements, if keeping them means risking war. It follows that international anarchy will rule in Europe and perhaps elsewhere. This may mean war or merely continuous unrest accompanied by "peaceful" changes.

I do not think that in the circumstances I would wish to live in England if I am limited to do work in physics in the Clarendon. But I would return to England if I find that it will be possible for me to cooperate with others in working towards changing this disastrous situation. I am taking steps to find out if anything of such nature is possible, in the meantime I shall ask Professor Lindemann for a further leave of absence without pay, or, if that should prove impossible, I shall resign from the Clarendon.

This for your personal information only. As to yourself, knowing that you are not very much interested in the fate of nations and believe that nothing can be done about it anyway, I think that you are very wise to think of America. Maybe you could try for a Common Wealth Fellowship for next year

Whether I can find something for you here right away I do not know, but I would try it at once and with great energy if, having received this letter, you advise me by cable that you want me to do so. I feel that it is doubtful whether you should accept any position which is not safe for at least two years and carries a salary of at least \$2500. I might try to find something for you on such terms on the basis of our cystein work, as it seems that I can make people in this country enthusiastic about it. Of course, I do not know in the least whether I can get them to go at it seriously. I believe though that another year or two spent with radioactive indicator work with cystein, carried out in collaboration with physiological chemists, would give you a very good start here. There is some danger though that, if I talk to people about you, mentioning your name - and of course I would have to mention it - rumours might arise which could reach Oxford and perhaps prejudice your position. You must let me know whether you want to take that risk. I shall then take all precautions I can. As to the merits of the question whether you should or should not leave England, I have little to say. Unless you take a Common Wealth Fellowship and come here as a temporary visitor, leaving England means a very grave decision. If you decide at all that you want to leave England, you ought to take that decision either on the ground that you believe that you would rather live here for the next ten years than in England, or on the ground of some broad principle. Moreover, it can easily be that, in spite of your decision, I am not able to find anything suitable for you. Of course, the possibility

to apply for a Common Wealth Fellowship would still remain. If you have something definite to say about ^{wanting me to do something} ~~this point~~, please send me a cable, because I have some free time now and intend anyway to discuss cystein with a number of people in the near future. In a few weeks time I might be away from the East and unable to do much about it.

As to the cystein publication I am not certain whether it is wise to say something about "moonshine", but if you prefer a joint publication I shall reconsider the matter. There is one condition, however, on which I have to insist, that is that I may make a footnote pointing out that you carried out all observations ^{yourself} during my absence from Oxford. This might be unconventional, but I feel that I cannot take credit for what I have not done. If you think we can make such a footnote, then I would be very glad to publish jointly but would have still to think a little bit about the advisability of a joint publication from the point of view that it might be premature to say those things which I wanted to say. I quite agree that you have to publish in any case very soon in order to satisfy the Salter people. By the way, what is your status now? Have they not extended the fellowship for another year? Or did Lindemann make some other arrangement? What do you think are your chances of a real career at Oxford?

As to the electron transformer, the instability of the electron path in a radially decreasing field of cylindrical symmetry which you think you have proved, does not exist. The mistake you make is the following: you consider the force acting on the electron instead of considering, as you ought to, the product of time

and force. If the electron spends less time in the stronger field than in the weaker field, the time factor may compensate for the force factor, and that is exactly what in fact happens. You can see this by considering a special case in which you can easily construct the path of the electron. I have done that on the enclosed sheet of paper for the following special case: the magnetic ^{uniform} field has a ~~constant~~ value, both inside the circle F and outside the circle F, but the value jumps at the circle and the value outside is smaller than the value inside. The path of the electron which I have drawn, starts at B; it is a ~~xxxx~~ segment of a circle, the center of which is at O and the radius of which is π . This segment ends at point C, from then on, between point C and D, the ^{in the weaker field} path is a segment of another circle, the center of which is at A and the radius of which is R. At D the electron again enters into the stronger field and the path is again a segment of a circle with a radius r, The center of this segment is however now O. It is obvious that O is at the same distance from the cross which marks the center of the circle F as O. Therefore, there is no displacement of the electron path, either towards the weaker field or towards the stronger field.

~~If~~ Though there is no instability of the electron path, there is an instability of the political situation, and if you also feel that we should not build an electron transformer at Oxford, please let me know quickly so that I can officially tell Lindemann that we are abandoning the project.

Please write.

Yours,

c/o Liebowitz
420 Riverside Drive
New York City

January 18th, 1939

Dear Tuck:

Please write me as soon as you get this letter 1) whether you got the long letter which I wrote you between October 20th and 30th, and in which among other things I explained to you why your argument about the instability of the electron path in the electron transformer was no good. 2) whether you answered my letter or whether you have written to me at all between October and today, as I have had no news from you during this period. 3) What did you decide about your cystein paper? 4) Whether you have a fellowship or what other arrangements you have got at Oxford.

Dr. Fransi Weiss, who is at present in England but who will sail for New York some time in February, will call on you, and you can give her all the things for me which she wants to take along with her.

Our image amplifier may get some importance in connection with the electron microscope which is now being worked out by the R.C.A. I may therefore send you some documents relating to an American patent application which you would have to sign at the American Consulate, Cavendish Square, London.

Hoping to hear from you soon,

yours,

George Eastman

1155 East 57th Street
Chicago 37, Illinois

July 15, 1948

Mr. James Tuck
Clarendon Laboratory
Oxford, England

Dear Tuck:

Do you remember that a provisional patent application was filed in your name and mine on a gadget called an image multiplier, and that you discussed it with Dr. Szegho after I left England? Dr. Szegho at that time was not interested. In the meantime he has moved to Chicago, and I talked to him about it again. I reconstructed the gadget from memory, and his associates are now studying it in order to determine whether they are interested. I shall be out of Chicago for two months now, but in September I shall contact him again to see whether they would be willing to build such a gadget, possibly on some government contract.

I remember that we had some sort of an agreement about this invention but it doesn't seem to be in the files which I have in Chicago. What I want to know from you is (1) do you remember what our agreement was? (2) if it should appear desirable to file a patent application in September, do you consider yourself to be a joint inventor in the terms of the American patent regulations (you probably know the distinction between joint ownership of an invention and joint inventorship in the American sense.) Since I may need information on both points in September, it would be best if you try to answer these questions upon receipt of this letter, lest you forget.

Mr. James Tuck

-2-

July 15, 1948

I have quite a well-equipped laboratory now for work on bacteria phage and bacteria mutations, and I find the field very interesting. The work on bacteria phage is in rapid progress in this country, but apparently no work in this field of any value goes on at present in England. Let me know what you are doing and if you plan to visit this country soon. I may visit Europe next summer, but hardly any earlier.

With kind regards to all,

Yours,

Leo Szilard

(4)
He is right) that the light
microscope is responsible for the
fusion, but I had the
impression that such a large
cross section was forbidden.

As for money affairs etc,
these are not so settled. Anyone
with sense would want to go
to America these days, but it is
difficult to see how this could
be arranged. I hope to get
some increase in income for next
year as my normal £300
does not seem to go very far.

I had no success with the
various patents etc and abandoned
them. I must say that I regret
your absence from here, most

TELEPHONE 3545.

Adom 10/39 [Tuck] ①

THE CLARENDON LABORATORY,
UNIVERSITY MUSEUM,
OXFORD.

Monday 19 June
1939

Dear Szilard,

Goldhaber is here
today to collect your
things and I am dashing
off this note for him to
take. I feel that my long
silence is inexcusable
particularly as I received ~~at~~
both your letter and your
wife. Please ascribe it to
hard work.

The work on the
electron accelerator is

beam. My large condensers ⁽³⁾ arrived a few days ago. The characteristics are very good, I think better than any other condensers in the world. It was made by Philips and has cap $0.2 \mu\text{F}$, 200 kV working potential, resistance $< 0.5 \Omega$ and inductance $< 0.5 \mu\text{H}$!

We are also all very interested in O^+ ions and I expect to write an article on the industrial (and other!) significance of it soon.

Goldhaber believes (I like

progressing tolerably well. ⁽²⁾ I have checked the orbits of electrons in the static case i.e. magnetic field constant (giving focusing in one plane only). It was very pleasant to observe the oscillations normal to the plane of the central orbit.

I expect to try to accelerate the electrons thus runned (in the absence of any device for extracting them, I shall for the present use a grid of suitable stopping power in the path of the

[Tolk, James]

46⁹ BANBURY ROAD,
OXFORD.
TEL. 48550.

1. Oct. 1948

My Dear Sjulard,

How nice to hear from you.

I do remember the image multiplier to which you refer, and in fact encountered the draft and calculations while clearing out old papers a few months ago. I seem to have burnt them.

In answer to your questions about our agreement, I can't remember the details but don't worry - I don't wish to retain any interest in the device and will furnish you with a legal disclaimer if you desire.

We have had an extraordinary number of visits from American friends lately including Alvarez, Oppie, Smith, Teller, etc. I was glad to hear that you are getting on so well with your biological studies - your other activities

46th BANBURY ROAD,
OXFORD.

TEL 48550

don't need reporting by letter!

Ed and I have been over here two years, and after a fearful struggle, have now decided, irrevocably to emigrate. After our life in America we don't seem to be able to settle somehow, although I have an excellent position and good income. Isn't it silly. A contributory factor is the behaviour of Collie, who is head of the nuclear physics side and as you predicted, impossible.

We haven't told anybody on this side yet as we haven't decided where to go. I'd love to go back to Los Alamos, but don't fancy anywhere in the East. Alternatively I may go to Australia.

Teller knows and is sounding the US authorities. Best wishes. yours ever
J.L.J.

1155 E. 57th Street
Chicago 37, Illinois
February 21, 1949

Professor T. L. Tuck
46D Banbury Road
Oxford, England

Dear Tuck:

This is in reply to your kind letter of October 1st. It was very nice to hear from you, though I have not done anything in the meantime about the image multiplier.

I am not surprised about the difficulty which you mentioned, in your letter, but I am rather staggered by the decision which you seem to have taken. I talked to Teller about it immediately upon receipt of your letter, and upon his return to Chicago from an extended trip, I will ask him again if he has made any progress.

Please let me know how your plans develop.

Sincerely yours,

Leo Szilard