

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

den 31. Oktober 1939

Lieber Teller:

Ich habe Briggs ein Memorandum geschickt und werde auch Ihnen, sobald ich eine verfügbare Kopie habe, diese zugehen lassen.

Inzwischen habe ich ueber den Versuch zur Messung von magnetischen Kernmomenten mit zwei guten Ingenieuren gesprochen, und der Effekt ist etwa 20 bis 100 mal so gross wie die messtechnisch leicht erreichbare Grenze. Unter diesen Umstaenden muss man also jetzt genauer die Einstellzeit des thermo-dynamischen Gleichgewichts betrachten. Meine Abschaetzung bezieht sich im uebrigen auf 250 cc fluessiges Paraffin, und es waere gut, wenn man Fluessigkeiten oder feste Koerper verwenden koennte.

Wigner sagt, dass seines Wissens die Teller-Heitlerschen Zeiten von etwa einer Sekunde experimentell widerlegt sind und dass er glaubt, dass die Einstellzeiten mehr in der Groessenordnung von $\frac{1}{1000}$ Sekunde liegen. Er konnte nicht genau angeben, von wem die Arbeiten stammen und meinte, dass Sie diese wahrscheinlich kennen. Auf meine Frage, wen man ausser Ihnen fragen koennte, - fuer den Fall, dass Sie diese Arbeiten nicht finden - nannte er Van Vleck. Waere es Ihnen moeglich, herauszufinden, wie es mit dieser Angelegenheit steht? Es ist insofern eilig, als ich auch schon einen Mann habe, der anscheinend bereit waere, den Versuch zu machen.

Es gibt noch eine Frage, die in diesem Zusammenhang von Interesse ist, und zwar die folgende: Durch die Koppelung mit der Rotation ist ein Stoerfeld von der Groessenordnung von 30 gauss vorhanden. Da die

Praezessionszeit etwa 10^{-7} Sekunden ist und das Stoerfeld rasch wechselt - man hat vielleicht 10^{11} Zusammenstoesse per Molekuel und Sekunde - besteht die Frage, ob dieses Stoerfeld eine 30 gauss entsprechende Verbreiterung der Resonanz hervorrufft oder ob die Verbreiterung wesentlich kleiner ist infolge des unregelmaessigen, aber raschen Wechsels.

Wichtig und dringend ist natuerlich nur die Frage der Einstellungsgeschwindigkeit des thermischen Gleichgewichts. Ueber alles andere koennen wir gelegentlich muendlich sprechen. Natuerlich koennen wir, wenn es sein muss, eine paramagnetische Substanz hinzumischen, doch waere es schoener, wenn man das vermeiden koennte.

Ihr

(Leo Szilard)

Prozessionszeit etwa 10-15 Sekunden ist und das Brevier noch wech-
 seln - man hat vielleicht 10-15 Minuten pro Material und Be-
 kunde - besteht die Frage, ob diese 30 Jahre entpre-
 chende Verbreitung der Resonanz hervorruft oder ob die Verweise-
 rung wesentlich kleiner ist infolge des unregelmässigen, aber raschen
 Wechsels.

Handwritten signature: Meyer

Wichtig und dringend ist natürlich nur die Frage der Hinsteif-
 geschwindigkeit des thermischen Gleichgewichts. Über alles andere
 können wir gelegentlich mündlich sprechen. Natürlich können wir,
 wenn es sein muss, eine physikalische Substanz hinzumischen, doch
 wäre es schöner, wenn man das vermeiden könnte.

My

(100 2/10)

June 11, 1941

Professor Edward Teller
4533 North 32nd Road
Arlington, Virginia

Dear Teller:

Many thanks for your letter of June 10. Fermi also received your letter today, and asked me to tell you that he expects to answer it tomorrow after he has seen Professor Pegram. He also asked me to mention to you, if I write to you today, that the question of appointing you for a whole year has been discussed at the last department meeting, and that a recommendation is being made by the department for your being appointed for a period of a year. At this department meeting Fermi has mentioned your telegram and Pegram has mentioned the letter which he had received from Marvin. It is hoped that the budget can be arranged in such a way as to settle this matter now for a whole year. Undoubtedly you will hear more about this point from Fermi within the next few days.

I have the feeling that it might be useful for Fermi to know how much your present salary at George Washington University is. Could you possibly send me a telegram giving me the figure upon the receipt of this letter?

I shall write you a separate letter about some other questions which are raised by your letter which you wrote me.

In haste,

Yours,

LS:H

(Leo Szilard)

Reissner

1155 East 57th Street
Chicago 37, Illinois
October 18, 1944

Dear Teller:

I had told you I would let you know when I shall be in a position to release B. T. Feld from his present work so that you may put in a claim for him. I am now writing to relate the following sequel of events: Feld's name was put some time ago by our project on the list of people who might be made available for your project. Neither he nor I knew of this until he received from your project a sheet with a request to fill in certain personal data. Subsequently Feld received a formal offer of a job from your project and was informed today of the specific nature of the job for which he was wanted. I understand that he will decline this offer tomorrow on the ground that he is much more suited for work of another type. In the circumstances I feel that I ought to let you know of all this because I want you to feel free to put in a claim for your own group for Feld.

Feld and I have been working rather hard lately on problems which may be of no practical importance for this war and we both feel that it would be preferable to work on problems which have a better chance of being applicable in the present war. As to my own problem, I ~~may~~ write you about it in another letter in a few days if the situation becomes clearer. In the meantime I wanted to let you know about Feld so that you should be able to take action concerning him without delay, on the return of Shane to your place.

Sincerely yours,

Leo Szilard

Mr. E. Teller
P. O. Box 1663
Santa Fe, New Mexico

THIS DOCUMENT HAS BEEN
TAKEN FROM A FILE OF THE
ARGONNE NATIONAL LABORATORY
AND WAS TURNED OVER TO
DR. LEO SZILARD ON

December 22, 1944

~~SECRET~~

MUC-28-48

This document consists of 1
pages and 0 figures
No. 3 of 3 copies, Series A

Handwritten signature
277

Mr. E. Teller
P. O. Box 1663
Santa Fe, New Mexico

Dear Teller:

I have asked that an application which has been prepared at Y be submitted to you before I sign it. This application is identified as Case 5-668X and contains claims drafted at Y and a few claims added in Chicago. In view of the fact that we had collaborated in this field in the early days of this work it seems advisable to have your opinion on record before proceeding with this case. I would like you to read all the claims, remove any claim which does not appear to be reasonable or of which you think I should not consider myself to be the inventor.

Handwritten initials
MRD

Please write me a letter identifying the claims by number which you suggest that I remove for one reason or another. It is my intention to execute the case, leaving in only such claims which you think are justified and reasonable.

Yours sincerely,

Leo Szilard

ls:s

~~SECRET~~

This document contains information affecting the national defense of the United States within the meaning of the Espionage Act, U.S.C. 50, 51 and 52. Its transmission or the revelation of its contents in any manner to an unauthorized person is prohibited by law.

~~SECRET~~

P. O. BOX 1663
SANTA FE, NEW MEXICO

This document consists of 2 page(s)
No. 1 of 3 copies, Serial A

Teller

July 2, 1945

Conclusion

Dr. Leo Szilard
P. O. Box 5207
Chicago 80, Illinois

CLASSIFICATION CANCELLED
Date 10/30/57
For The Atomic Energy Commission
C. R. Marshall / W. C. Reuther
Director, Division of Classification

Dear Szilard:

Since our discussion I have spent some time thinking about your objections to an immediate military use of the weapon we may produce. I decided to do nothing. I should like to tell you my reasons.

First of all let me say that I have no hope of clearing my conscience. The things we are working on are so terrible that no amount of protesting or fiddling with politics will save our souls.

This much is true: I have not worked on the project for a very selfish reason and I have gotten much more trouble than pleasure out of it. I worked because the problems interested me and I should have felt it a great restraint not to go ahead. I can not claim that I simply worked to do my duty. A sense of duty could keep me out of such work. It could not get me into the present kind of activity against my inclinations. If you should succeed in convincing me that your moral objections are valid, I should quit working. I hardly think that I should start protesting.

But I am not really convinced of your objections. I do not feel that there is any chance to outlaw any one weapon. If we have a slim chance of survival, it lies in the possibility to get rid of wars. The more decisive a weapon is the more surely it will be used in any real conflict and no agreements will help.

Our only hope is in getting the facts of our results before the people. This might help to convince everybody that the next war would be fatal. For this purpose actual combat-use might even be the best thing.

And this brings me to the main point. The accident that we worked out this dreadful thing should not give us the responsibility of having a voice in how it is to be used. This responsibility must in the end be shifted to the people as a whole and that can be done only by making the facts known. This is the only cause for which I feel entitled in doing something: the necessity of lifting the secrecy at least as far as the broad issues of our work are concerned. My understanding is that this will be done as soon as the military situation permits it.

All this may seem to you quite wrong. I should be glad if you showed this letter to Eugene and to Franck who seem to agree with you rather than with me. I should like to have the advice of all of you whether you think it is a crime to continue to work. But I feel that



H3210

~~SECRET~~ 4058

I should do the wrong thing if I tried to say how to tie the little toe of the ghost to the bottle from which we just helped it to escape.

With best regards.

Yours,

E. Teller

E. Teller

1155 East 57th Street
Chicago 37, Illinois
June 29, 1949

Professor Edward Teller
P. O. Box 1663
Santa Fe, New Mexico

Dear Teller,

I spent two days in Washington where I saw a number of people both in and out of Congress. In Congress I limited myself to talking to those whom I consider to be our political friends. Throughout these conversations, I was absolutely frank and got what I believe was a very frank response. As a result of these conversations I came to the following conclusion: Our only hope for a vigorous program of development work taking place in the future in contrast to the past depends on three factors; (1) to bring about the clear recognition of the shortcomings of the past; (2) to bring about a helpful attitude in Congress toward the Atomic Energy Commission; and, (3) to have the scientific community exert a steady pressure on the Atomic Energy Commission in the right direction.

I am quite optimistic about the first of these points since I found that some of the responsible men in key positions (notably Hafstad) have the correct appreciation of the problem. The other two points are more difficult to achieve, and I would like to discuss them with you when the opportunity arises.

Because the attacks against the Commission follow a political line-up, everything that we might publicly say ^{up this line} will be made out to be either black or

Professor Edward Teller


-2-

June 29, 1949

white. Presenting a balanced picture of black and white spots would in these circumstances serve no useful purpose. To put it in a nutshell (1) I would not volunteer to testify, (2) if asked to testify, I would get out of it by giving a polite excuse.

I thought I should give you my reactions for whatever they are worth.

Sincerely yours,



m

UNIVERSITY OF CALIFORNIA
LOS ALAMOS SCIENTIFIC LABORATORY
(CONTRACT W-7405-ENG-36)
P. O. Box 1663
Los Alamos, New Mexico

July 5, 1949

IN REPLY
REFER TO:

Professor Leo Szilard
1155 East 57th Street
Chicago 37, Illinois

Dear Szilard:

Thanks. I shall do so.

Best regards,



Edward Teller

ET:fa

(Probably Feb. 1, 1956
K.W.)

(36.s. 5)

D

1155 E. 57th St.,
Chicago 37, Illinois.

Professor Edward Teller,
Department of Physics,
The University of California,
Berkeley, California.

Dear Teller:

Recently I went through my files relating to my work in the early development of atomic energy and picked out a number of documents. Since you and I were in close touch during most of the period that these documents cover I thought that you might perhaps like to have a set of them. Or else you might just glance at them and return them to me if you don't want them to take up space in your office.

The attached material consists of the following:

- 1) A letter to the Editor of the Physical Review (Szilard and Zinn, Phys. Rev. 55, p. 799, 1939) that describes the discovery of the neutron emission in the fission of uranium and states that about two neutrons are emitted per fission. (1) The same discovery was made about the same time, but by a different method, independently by Malban, Joliot, and Kowarski, as well as by Anderson and Fermi.
- 2) An article in the Physical Review (Anderson, Fermi, and Szilard, Phys. Rev. 56, p. 284, 1939) in which the possibility of maintaining a chain-reaction in a uranium-water system is explored. (2)
- 3) Three letters written by me to Fermi in July, 1939, dated July 3rd, 5th and 8th, proposing the use of the uranium-carbon system. (3)
- 4) Fermi's answer, dated July 9, which crossed my July 8th letter to him and also my answer to Fermi's letter. (4)
- 5) During the second week of July, I saw that by using a lattice of uranium spheres embedded in graphite, one would have a great advantage over using alternate layers of uranium and carbon. I also saw

that if we have a lattice of uranium spheres in graphite a further advantage can be gained by using uranium metal instead of using uranium-oxide. It was these facts which led me to believe from then on that there is a good chance of maintaining a chain-reaction in a uranium-graphite system.

Early in July, I told Wigner of my pre-occupation with the uranium-graphite system. The two of us paid a call on Einstein and Wigner left then for the West Coast. You came to New York to spend the summer there, and I kept you informed throughout July on my thoughts concerning the uranium sphere - lattice system. My appraisal for the chances of a chain-reaction in such a system was communicated to Einstein when you and I called on him later in July.

Einstein's letter to President Roosevelt and my memorandum which was attached to it are enclosed. (5)

When Fermi returned to New York in the fall of 1939, I showed him my computations on the uranium sphere lattice in graphite and he showed me his of a homogeneous mixture of uranium and carbon. In accordance with this the memorandum which I submitted to Dr. Briggs in October, 1939 contains the following passage:

"The properties of a system composed of uranium and graphite have been calculated independently, for a homogeneous mixture, by Fermi, and, for a lattice of spheres of uranium oxide, or uranium metal, embedded in graphite, by myself. The results of these two independent calculations are in reasonable agreement and show that the two arrangements have different properties. For instance, in the case of using a lattice of spheres a great advantage could be obtained by using uranium metal instead of uranium oxide, whereas in the case of the homogeneous mixture the use of uranium metal would be of no great advantage."

So that you do not gain the wrong impression from this passage, I should stress that Fermi knew that it is better to keep the uranium

and carbon in separate layers rather than to use a homogenous mixture. This is shown by the passage in his letter of July 9, 1939, which reads as follows:

"Since however the amount of uranium that can be used, especially in a homogeneous mixture is exceedingly small, even a very small absorption by carbon either at thermal energy or even before might be sufficient for preventing the chain-reaction; perhaps the use of thick layers of carbon separated by layers of uranium might allow to use a somewhat larger percentage of uranium."

In the case of the uranium-water system, Fermi had computed as early as June, 1939 that some advantage can be gained by keeping the uranium and water in separate layers. I have no knowledge, however, that Fermi has recognized the advantage of having a lattice of uranium spheres or rods of small diameter embedded in graphite, or the advantage of using the metal in place of the oxide in such a lattice - until I presented my results to him.

The full text of the memorandum which I submitted to Dr. Briggs in October, 1939 is enclosed. (5)

6) Report A-55 of the Uranium Committee which is a copy of a paper that I submitted to the Physical Review in February, 1940. (6) This paper was accepted for publication, but publication was deferred at my request because of the nature of the paper. On the basis of measurements which Joliot and his co-workers have performed on a uranium-water system, it is concluded in this paper that one should be able to maintain a chain-reaction in a uranium-graphite system if a lattice of uranium metal spheres is embedded in graphite. The paper gives a rough theory of such a system. It explains that the chain-reaction can be controlled by shifting an absorber between positions differing in neutron density and it states that the delayed neutron

emission of uranium permits it to move such absorbers quite slowly.

7) A page from Chemical Engineering News showing that the "first nuclear reactor patent (No. 2,708,656) was issued to Leo Szilard and the late Enrico Fermi on May 17, 1955 by the United States Patent Office."

Yours,

Leo Szilard

LEO SZILARD

Edward Teller
Physics Department
University of California
Berkeley, California

1155 E. 57th Street
Chicago 37, Illinois

February 1, 1956

Dear Teller:

Enclosed you will find some material relating to my part in early history. Since you lived through most of it, you might be interested in seeing it.

Ed McMillan has shown in the past some interest in my ancient contributions to "early history". You might ask him if he might care to thumb through this material. And if he does, please pass it on to him.

With kindest regards,

Yours,

(Leo Szilard)

Ingl

The Quadrangle Club
The University of Chicago
1155 East 57th Street
Chicago 37, Illinois

May 7, 1956

Dr. Edward Teller
Department of Physics
University of California
Berkeley, California

Dear Teller:

Sorry to write you so many letters. The present one is about the following situation: I shall be transferred in Chicago on the 1st of July to the Institute of Nuclear Studies, and have the title of Professor of Biophysics. The Institute can give me an office but no lab space, and even if I could get lab space outside of the Institute—which I assume I could—I could not build up a good group in biology for the men who would come there would have no chance for promotion.

The Biology Division of Cal Tech (Beadle), the Department of Biophysics of the Medical School of the University of Colorado (Puck), and the Department of Pharmacology of the Medical School of NYU, have been discussing with each other and with me the possibility of asking the National Science Foundation to set up a Roving Research Professorship which would leave me free to pursue my scientific interests anywhere I desired and it would not be limited to these three institutions.

Apparently the officers of the National Science Foundation are interested in creating such a novel institution. The National Science Foundation would however not set up such a "fellowship" for more than five years.

The situation is complicated by the following calamity. If I stay at the University of Chicago and retire at the age of 65, about seven years from now, I would have a retirement income from Teachers Annuity of \$113 per month. It is this low because my regular academic employment started in 1946.

Dr. Bernard D. Davis, head of the Department of Pharmacology at the NYU Medical School, and some of my other friends in New York, believe that it might be possible to raise from foundations and private individuals funds to supplement what the National Science Foundation might be able to do, and create some sort of a "fellowship for life." They wish to enlist your help in this matter and you will receive a letter from Dr. Davis in the near future. He discussed with me what he proposes to do, and it sounds all right.

Sorry to bother you with this.

Yours,

Leo Szilard

University of California
Radiation Laboratory
Berkeley 4, California
June 26, 1956

Professor Leo Szilard
The University of Chicago
Division of the Social Sciences
Chicago 37, Illinois

Dear Szilard:

Would you send me a new set of the papers. I should like
to have them -- I might be able to use them again.

With best regards,



Edward Teller

ET/eo

The Quadrangle Club
The University of Chicggo
Chicago 37, Illinois
July 16, 1956

Dr. Edward Teller
University of California
Radiation Laboratory
Berkeley 4, California

Dear Teller,

Your letter of June 26th chased me around half the world before it reached me. My present correct address is: The Quadrangle Club, The University of Chicago, Chicago 37, Illinois.

Since I wrote you I read in the newspapers that a nominating committee has been appointed by the trustees set up by the Ford Motor Company, and that this nominating group includes Rabi and Bacher. I had lunch some time ago with Rabi -- this was, I believe, before his appointment to the Nominating Committee -- who told me at the time that he thought eligibility for the Ford prize would be limited to current advances. This information may or may not have been correct.

Enclosed you will find the collection of papers which you requested.

Many thanks!

Yours,

Leo Szilard

m
Encl.

University of California
Radiation Laboratory
Berkeley 4, California
June 27, 1956

Professor Leo Szilard
The University of Chicago
Division of the Social Sciences
Chicago 27, Illinois

Dear Professor Szilard:

Among the papers which you sent to Dr. Teller to be forwarded to Professor Compton is a paper identified as A-55. We are sure this paper has been declassified, but our Information Division has no record of any change in its classification since it was marked Secret.

If you will cite for me the authority by which the paper was declassified, Dr. Wakerling will downgrade the paper and I will send it on to Professor Compton at a later time.

Sincerely yours,



Eileen Osmond
Secretary to Edward Teller

The Quadrangle Club
The University of Chicago
Chicago 37, Illinois
July 17, 1956

Miss Aileen Osmond
Secretary to Dr. Edward Teller
University of California
Radiation Laboratory
Berkeley 4, California

Dear Miss Osmond:

Many thanks for your letter of June 27th. Report A-55 was the report of the Uranium Committee and was classified "Confidential". My copy has a stamp on it which reads as follows: "Classification changed to not classified, 11/21/46. Authority of D.E.M.E.D." I hope this answers your question concerning this report.

Please note that all correspondence to me should be addressed to me at The Quadrangle Club, The University of Chicago, Chicago 37, Illinois.

With many thanks,

Sincerely yours,

Leo Szilard

Teller
The Quadrangle Club
The University of Chicago
Chicago 37, Illinois
July 17, 1956

Dr. Edward Teller
Radiation Laboratory
University of California
Berkeley 4, California

Dear Teller,

My memory is somewhat hazy but if I am correct in thinking you had been Privatdocent in Germany and left Germany after January 1, 1933, then you are eligible for a quite considerable pension under a new law which was passed in Germany in December of last year. I will write you the details after I have heard from you, telling me what your exact status in Germany was. I am told that this pension might amount to something like \$450. a month less 30% German tax and probably also less U.S. income tax. Still you would get it as long as you live and, if you should be survived by Mizi, she would get a certain fraction of it as long as she lives. The deadline for applying is December 31st. Please drop me a line and I shall write you in greater detail if it turns out that you are eligible.

With kind regards,

Sincerely,

Leo Szilard

UNIVERSITY OF CALIFORNIA

RADIATION LABORATORY
BERKELEY 4, CALIFORNIA

February 21, 1957

Mr. Cass Canfield
c/o Harper & Brothers
49 East 33rd Street
New York 16, New York

Dear Mr. Canfield:

I have studied with greatest interest the proposals by Szilard and Doering concerning a scientific project of unusually wide scope. I am not surprised to find that the proposals are of great interest. I believe that there exists a need of research bringing together various branches of thought and escaping the overspecialization which characterizes too much of today's science.

Unfortunately I find myself so loaded down with specific duties that I feel that it would be wrong for me to intend to participate. Furthermore, I am rather ignorant of biology which seems to be the focus of interest in the present proposals.

Regretting that I shall not be able to be of specific help I hope that you will find it possible to give support to the proposals in hand.

Sincerely yours,



Edward Teller

ET:gg

COPIED FROM ORIGINAL
IN THIS COLLECTION

UNIVERSITY OF CALIFORNIA

RADIATION LABORATORY
BERKELEY 4, CALIFORNIA

September 25, 1957

Dr. Leo Szilard
The University of Chicago
The Enrico Fermi Institute
for Nuclear Studies
Chicago 37, Illinois

Dear Szilard:

Thank you for your letters of August 15th and September 20th. I have been away all summer (on a real vacation with nothing being sent after me) so I received both of your letters at the present time. It is very nice to hear from you and I am not surprised that you are at it again. In fact, I should be rather surprised if that were not the case. There are, however, two strong reasons why I feel that I have to stay away from anything of the kind that you are discussing.

One reason is this: I am not confident that scientists are in any better position than other people to make reasonable recommendations about questions like the stability of the world. The facts are really available to all people and to the extent they are not I feel that we should make efforts to make them more available.

Among the people with whom you are in touch there are many for whom I have a great deal of respect. About others I have more doubts. This would be so with any group, whether scientific or not scientific.

There is also another reason for my begging off. In the last months it has been discovered that I don't seem to be terribly well off as far as my health is concerned, and therefore I have to restrict my activities quite sharply. I have already dropped out of most of the things I am doing and I am quite systematically saying "no" to any additional suggestions. I am told that if I behave myself I might be all right again in a year or two.

With best regards,



Edward Teller

ET:gg

November 29, 1957

Dr. Edward Teller
Physics Department
The University of California
Berkeley, Cal.

Dear Teller,

Enclosed you will find a letter which I wrote to Warren Johnson which is self-explanatory. I saw a lot recently of Weizsäcker (and so did, incidentally, Weisskopf) and was very much impressed by him.

I don't know whether the meeting about which I wrote you will come off or whether there will be similar meetings called by some organisation other than the University of Chicago. But I wish to urge you to attend if you are invited, for the following reasons:

I believe you owe it to the scientific community to defend your views before your peers. Moreover I think, it would be a healthy thing for you to do so. The views which you have presented in the past in a few speeces are rather scetchy and there is a general feeling around that they would not stand up to a critical analysis of the kind scientists are accustomed to apply - at least within their own field.

Looking at things from this side - admittedly the wrong side - of the Atlantic, it would seem that the American policy is at present directed at goals which are even more unattainable than those persued previously. However, I might be mistaken, for lack of adequate information. With kindest regards,
Sincerely,
Leo Szilard.

3/50

Quelque

UNIVERSITY OF CALIFORNIA

RADIATION LABORATORY
BERKELEY 4, CALIFORNIA

Decembred 15, 1957

Dr. Leo Szilard
Hotel Kempinski
Berlin W 15 - Kurfurstendamm 27
Germany

Dear Szilard:

It was nice to get your letter of November 29th. I wonder whether you will decide to stay in Germany. For a moment I thought that I would regret it since I might have less of a chance to see you, but in your case I expect that your having a job on the other side of the Atlantic will not diminish our chances of coincidence.

Do let me know how your plans for the conference are coming. I still believe that I will not be able to participate. I continue to doubt that a conference of this kind would be useful. The questions to be discussed are so terribly far from scientific questions. They are in the field where a discussion between two people is usually much more fruitful than a conference and even such exchange of opinions between friends have hardly any immediate effect. The best that can be gained by such exchange is to get a new point of view and think some more about problems later.

On the other hand, a conference even when handled in the most discreet way has some elements of a public act in it and this makes everything much more difficult.

In any case, you may want to let me know of your plans and I'll think it over.

With best regards (also to Weizsacker, if you see him),

Teller

Edward Teller

ET:gg

1957 Cedar Street
Berkeley 9, Calif.
January 29, 1960.

Dear Leo,

You hit the nail on the head.
Tudóval: "gewusst wohin". Pedig
napjainkban a diplomácia még
a modern fizikánál is nehezebb
tudomány...

Nem tudom, hogy tudja-e,
hogy én is a fiam után jöttem
ide, aki közben (1935 nyarán
találkoztunk Londonban, amint
arra bizonyára nem emlékszik)
32 éves ifjú lett és a forradalom
után jött ide. Különben tesztelt:
ő is fizikusnak készült. Így tehát
duplán egy esítette esatádunkat
Hálával, Köszönettel és
nagy szeretettel
Teller Kira Emmi

THE UNIVERSITY OF CHICAGO

CHICAGO 37 • ILLINOIS

THE ENRICO FERMI INSTITUTE
FOR NUCLEAR STUDIES

August 2, 1960.

Professor Edward Teller,
The Radiation Laboratory,
University of California,
Berkeley, California.

Dear Teller,

Enclosed you will find a copy of my latest "masterpiece". A revised and edited version of this will be printed in book form, together with some of the other stories which I have written. The attached preface gives you some further information.

I should appreciate any comments which you might care to make.

With kind regards,

Sincerely,



Leo Szilard

Enclosure

MSO Mrs. Teller
has copy

C O P Y

September 12, 1961

Dr. Alvin Weinberg, Director
Oak Ridge National Laboratory
Oak Ridge, Tennessee

Dear Alvin:

It was good to talk to you on the phone and it is wonderful that you are planning to come to Livermore on the 23rd of October. In the meantime I would like to make some additional remarks about both topics which we discussed. I shall start with the one in which you are most interested.

Your suggestion about Berlin is certainly the best and most reasonable I have heard. Nevertheless I am worried about it. I am certainly in favor of cooperation with everyone in science, including the Russians, but I am worried lest the use of Berlin in this connection gives the impression of a big brother act. In this way importance of other nations might be underplayed. This would be a grave mistake. If your suggestion on Berlin is accepted, as it well might be, I would be particularly careful to guard against this point.

My second worry is that in this scientific center we would have placed an object of great monetary and greater spiritual value as a pawn into the hands of the Soviet Union. The Communists are not gentle in treating pawns and they seem to have passed the point where they care about world opinion.

My third question is whether it is at all reasonable for us to propose solutions. I happen to agree with de Gaulle. The Soviets are making illegal demands. We should simply ignore them. If they become violent we will have to accept a local defeat. We should make arrangements to save the inhabitants of free Berlin by evacuating them in case of extreme necessity. We also should make arrangements to care for them and this could be done in cooperation with our NATO Allies and possibly in cooperation with other free countries. I have no doubt that the East Berliners would come out in big numbers. This situation if forced upon us would be a testimony to the peaceful and determined attitude of the West. At the same time it would be a disaster that would help to wake us up. And, as you know, I feel that our greatest danger lies in the fact that we are asleep.

Dr. Weinberg

-2-

September 12, 1961

The second point, in which I am more interested, is the statement that the Soviet Union has been testing all along. I am firmly convinced that in the present state of affairs small tests will lead to much more rapid advances than big ones. I know this to be true on our side and I suspect that it is also true for the Russians. If the Russians had not tested and felt the need of testing they could have performed small tests secretly. They would not have incurred an unfavorable reaction in the neutral countries and they would not have incited us to test. Even if big tests should be really required, their value would be greatly increased by earlier small model tests. On technical grounds it seems likely that the present sizeable Soviet tests have been preceded by small-scale tests which make their present tests more significant.

If you do assume that the Soviets have been testing all along, their present action appears more understandable. In three years of tests, considering their past rate of progress, they would have accumulated a decisive advantage. This advantage would make them feel that they will stay ahead of us even if we resume testing. Thus they will feel less inhibited to test openly. At the same time progress in the field of small explosions may have raised interesting possibilities concerning big explosions.

Of the various advantages they would derive from testing in the atmosphere I am particularly worried about their tests of the anti-ICBM devices. These have to be tested in the atmosphere. Between equally matched opponents I consider the anti-ICBM an almost forelorn hope. But if the Russians are stronger in nuclear explosives as well as in rocketry, and if they plan on a first strike which reduces our strike capability, then a well developed anti-ICBM defense may indeed become effective. For this reason I would urge that our capability of delivering decoys should be given urgent attention.

There are other plausible reasons for Soviet test resumption, but I do not want to enlarge upon them. My main worry is that the Soviets are now ahead in their conventional manpower, in submarines, in rocketry, and now probably also in nuclear explosives. If this is true, it may be correctly stated that they are ahead in every really relevant method of military preparedness. It seems to me most urgent that this dangerous situation be recognized and the public be made aware of it. The very great sacrifices which will be required if we are to survive will not be made if we continue in our state of euphoria.

This letter certainly does not need an answer. We can talk further during your visit.

Sincerely yours

Edward Teller

ET:gg
Dictated, but not signed
by Dr. Teller

VIA RA

52.21.68

1963 AUG 9 16 42



SGZ2473 BH MU094

HVA003 CHICAGOILL 38 9 1003A CDT

TELEGRAMME

5491 9 16:51

DOCTOR LEO SZILARD CARE DOCTOR MARTIN KAPLAN
Martin Kaplan
WORLD HEALTH ORGANIZATION PALIS DES NATIONS GENEVA
des Nations GENEVA

YOUR LETTER OF AUGUST 6 JUST ARRIVED STOP I WROTE YOU
YESTERDAY PROPOSING YOU VISIT UNIVERSITY IN OCTOBER STOP
HOPE THIS IS SATISFACTORY

EDWARD

OSUISSE

AMME

VIA RADIOSUISSE

TELEGG

1963 AUG 9 10 45

10:51

1963 AUG 9 16 44

YOUR LETTER OF AUGUST 6 JUST ARRIVED STOP I WROTE YOU
1963 AUG 9 16 44
YESTERDAY PROPOSING YOU VISIT UNIVERSITY IN OCTOBER STOP
HOPE THIS IS SATISFACTORY

EDWARD

Dr. Edward Teller
Department of Physics
University of California
Berkeley, Calif.

Dear Teller:

Sorry to write you so many letters. The present one is about the following situation. I shall be transferred ^{in Chicago} on the 1st of July to the Institute of Nuclear Studies, ~~so that I shall be called~~ ^{and will} ~~and remain the title of~~ Professor of Biophysics in the Institute. ~~But~~ I do not intend to try to build ^{up} a biology department, since the ~~man~~ ^{group there} whom I would bring there would have no chance of promotion, (the Institute of Radiobiology and Biophysics ^{is} ~~being~~ no longer in existence) .

The Biology Division of Cal Tech (Beadle), the Department of Biophysics of the Medical School of the University of Colorado (Puck), and the Department of Pharmacology of the Medical School of NYU, have been discussing with each other ^{and with me} the possibility of asking the National Science Foundation to set up a ~~Roving Research Professorship~~ ^{Research Professorship} which would leave me free to pursue my scientific interests anywhere I pleased, ^{desired.} ~~including these three institutions,~~ ^{and} apparently the officers of the ^{Nat.} Foundation are interested in creating such a novel institution.)

The National Science Foundation would however not set up such a "fellowship" for more than five years, ^{and} my situation is complicated by the following calamity. If I stay at the University of Chicago and retire at the age of 65, about seven years from now, I would have a retirement income from Teachers Annuity of \$113 per month. ~~Dr. Bernard D. Davis, head of the Department of Pharmacology at the NYU Medical School, and some of my other friends in New York, believe that it would~~ ^{might} be possible to raise from foundations and private individuals funds to

~~because I had not been regular academic employment began in 1946~~
^{It is this way}
~~because I had not been regular academic employment began in 1946~~
in 1946

Personal letter
Place Dept
Pappasheim
(Mace)
Walter
Science

3
D R A F T

Dr. Edward Teller
Department of Physics
University of California
Berkeley, California

Dear Teller:

Sorry to write you so many letters. The present one is about the following situation: I shall be transferred in Chicago on the 1st of July to the Institute of Nuclear Studies, ^{and here} ~~and retain~~ the title of Professor of Biophysics. ~~I do not intend to try to build up a biology group there since the men whom I would bring there would have no chance of promotion. (The Institute of Radiobiology and Biophysics is no longer in existence.)~~

Shunt
way → The Biology Division of Cal Tech (Beadle), the Department of Biophysics of the Medical School of the University of Colorado (Puck), and the Department of Pharmacology of the Medical School of NYU, have been discussing with each other and with me the possibility of asking the National Science Foundation to set up a Roving Research Professorship which would leave me free to pursue my scientific interests anywhere I desired and ^y would not be limited to these three institutions.

Apparently the officers of the National Science Foundation are interested in creating such a novel institution. The National Science Foundation would however not set up such a "fellowship" for more than five years.

~~My~~ ^{the} situation is complicated by the following calamity. If I stay at the University of Chicago and retire at the age of 65, about seven years from now, I would have a retirement income from Teachers Annuity of \$113 per month. It is this low because my regular academic employment started in 1946.

Dr. Bernard D. Davis, head of the Department of Pharmacology at the NYU Medical School, and some of my other friends in New York, believe that it might be possible to raise from foundations and private individuals funds to

Teller - 2

supplement what the National Science Foundation might be able to do, and create some sort of ^a fellowship for life." They wish to enlist your help in this matter and you will receive a letter from Dr. Davis in the near future.

He discussed with me what he proposes to do, and it sounds all right.

Sorry for bother
you with this.

~~Sincerely yours,~~

James

Leo Szilard

Dear Sillard,

^{Terrestrial}

In the Dept. of ~~Thermal or~~ ~~Theoretical~~ magnetic ~~the~~ bombardment

with ^{uranium} quanta } Li and D neutrons is being carried on.
10 seconds after the cessation of the bombardment
neutrons and γ -rays are coming out from the
[emanated]

uranium
quanta}. No more is known so far. A letter is on its
way to the Physical Review (it's not my fault) *

I am very interested now in the uranium story and
would be glad to speak with you. Will be in D.C.
Tuesday night.

An avoird (so long)

Teller

* This, moreover, is not yet dangerous, and Washington hasn't
thought of any ^{the} serious consequences
[realized]

EDWARD TELLER
2610 GARFIELD STREET
WASHINGTON, D. C.

magyarul

publ
mon

Kedves Li Lövel!

Ma kinn voltam Tuve- ihusál. Ezen
levél kísért a Phys. Rev.-nek a
"love kérés" kárta lammal. Az Urán-
bombázás után 10- perces periódus-
os főlépés neutronek néma kónit-
belül fél- akkora mint a fission-
processusok néma. Ez is más
kísérletek mutatják hogy ezen
neutronok nem y- sugárzás isle-
zi elő. A 10- perces ^{az argon relatív károsítás - vizsgálata} neutronek ^{transzmutáció}
bombázásánál is föllépnek. Ehat
história veletlén hogy a neutronek
egy "splitter"-ből erednek.

Mi helyt csak ebbe a kitem irodék-
lődni Urán iránt mindjárt heves
diskusszió indult meg a gyakorlati
jelentőségéről. Tuve Hapstad is Robert
keljesen kísérté van vannak hogy minál
van ró. A Fermi kísérleteiről is
szóltak. Ezen perre nem mondtam
semmit. A fönt említt levél bajt

nem ahogy hat. További hiszintekhez
nagyobb mennyiségű urán
hiánya miatt nem kevesebb. Külö-
nösen érdekes látni U₂₃₈ és U₂₃₅
elválasztása iránt mivel Bahr
szerint csak az utóbbi ad fissziót.

Nem tudom a vére kés keveseket
de nagy látni, hogy mindig a felső szint
széles. Naqyan az ember rájöhet, hogy
nagy minél van az. A Washington
ak mevetnek Carnegie-institút
széles beosztású hogy a dolgot gyáran.
Lati jöhetnek a segére való beosztás
főbb pénz adjanak U-research-
re. Ez talán nem rossz idea ment
a Carnegie-ét jóabban is óra-
tásak (talán hiszik túl óratásak)
De ez egyelőre nem hisz nekem
akutis, ha az a vere közeget Fermi
kötés a nagy más esemény kap-
csán nem kap nagyobb kedvet mint
eddig (Az hírem re kívánatos volna)

Ismerem hogy Washingtonban
Chain-reaction hangulat van. Csak az
kellene mondani, hogy "Urán" és azpán
két órák halgathatom, hogy őt mit
gondolnak. Övülnek továbbá arról hallani
és ha lehet segíteni.

Teller

Kedves Izidárol,

Cimem September 10 vagy 15-ig:
c/o Mrs. Furness, West Tisbury, Martha's
Vineyard Island, Mass.

Észembe juttatt egy ismer-
tőshöz fordulni lehetne: Tucke-
man, Bureau of Standards, magas állás,
széleskörűen repülőgépek-
re alkalmazható. Érdekes, nagyon élénk
szű. Szívesen ~~szívesen~~ eszem egyeztetés
közé fordulhat. De ha valamilyen
még Washingtonban vagyok szíve-
sen megteszem mindent amit
hasznosnak tart. Híre háibben
még beszélni, remélhetőleg.

Rádját hallgatunk és remél-
jük, hogy háibben lesz. Az még a
jobb eset. A gyomromban egy híre

1
bizonytalan érzés van. De mi a
világ háború egy kamaly egya-
mouvantás hoz képest.

Teller.

EDWARD TELLER
2610 GARFIELD STREET
WASHINGTON, D. C.

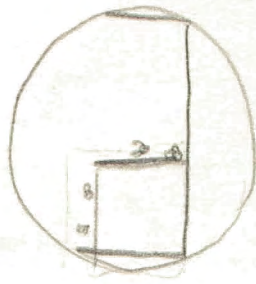
Kedves Li level!

A Dept. of Term. Magnesium-be-
kvánt Li + D - neutronokkal ban-
váltak. 10 - másodperccel a ban-
váltás megkezdte után ~~was~~ az
kvántból neutronok és γ - sugá-
rak jönnek ki. Több még nem
ismeretes. A Level a Phys. Rev.
nek írt ban van (én nem lehet
vóla.) * Az kvánt história most
nagyán érdekel és kíváncsi
ha beszélhetünk. Kérek a
New-York ban levelet.

Visszant lákásig

Teller

szülőnként
* Ez még nem biztos
is Washington ban semmi
kamaty következménye
nem gondolható.



Amber
months

Columbia 6955
2610 Garfield Street N. W.
Washington, D. C.

Kedves Ki leveled,

Vagyom ömilit. hogy a doloz
Roosvellet convergat. Lambat
veggel Washington ban levek is
vendelkerisve fogat. allui ha
ellenfides utasitast nem kapok.

Yici valoszinuleg Washingtonban
marad is in csak Pimbenen ke-
szel New Yorkban. Yi volna, ha
folthivna mi hely Washington-
ba interesit. Valoszinuleg legjabb ke-

He náhunk lahik.

Tueheman-nal beszéltém,
de nem reagált. ~~Ugyan~~ Ugy-
látásik nekem az évvel előző
szós fölhelyeni valami iránt a
nem a saját ítélet.

Tav-nal nem beszéltém a Uro-
stól minemvél nyáról. Ugy látásik
a laboratóriumban az évvel előző
he nem folyt. Vizont a Novemberi
Carnegie-exhibícióra (melynek cél-
ja a Carnegie-életnek reklámot csi-
nálni) lesz egy fixán-exhibíció.

Prémium láfam a Wechsler. He
nem Briggs vil úgy náhunk kátam.

So long

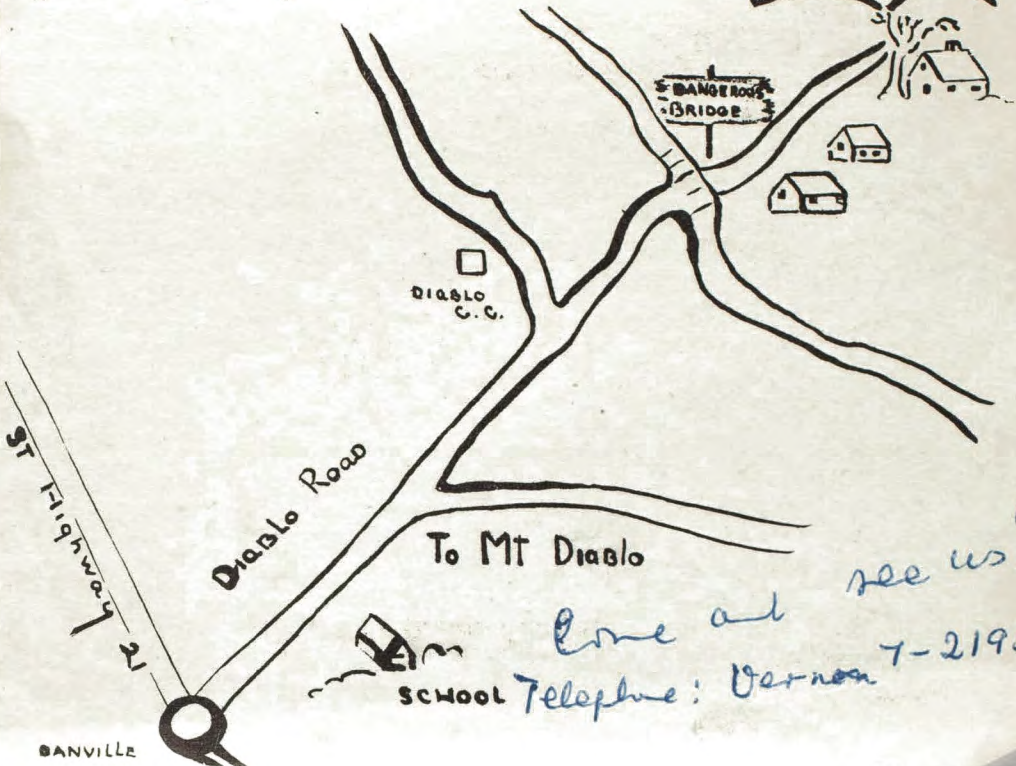
Teller

Merry Christmas and a
 Happy New Year
 For they are both very near
 and when they actually
 get here
 Let Christmas Carols
 ring in your ear.
 We wish you would follow
 this map
 and on our door you would
 tap



By Post

Tellers
 Edward, Lisa, Paul, Denny



A.H. Compton
Ryerson Physical Laboratory
University of Chicago
Chicago Ill.

Temporarily attached to

Having worked for the last six months with the Columbia group I feel that your presence and leadership at Columbia would have most desirable effects whereas transplanting the Columbia group to another place would necessitate readjustments and may thereby be harmful for the work.

Edward Teller

x