IMPERIAL HOTEL,
Russel Square,
LONDON, W.C.1.

8th May, 1933.

Dear Dr. D,

As far as the main details of our work is concerned it would be best for the Vice-Chancellor to get in touch with Sir William Beveridge to whom I shall write simultaneously. I wired to-day:-

> " Suggest Vice-Chancellor communicate with Sir William Beveridge London School of Economics. Letter following."

As far as individual cases are concerned I would suggest that if funds are available and if laboratory places are available (Tyndall to dispose of both for a period of <u>only one year</u>) if the best way would be to use these funds in giving fellowships of £100 a year, and applying for an additional £100 to a committee which already deals with individual cases.

You may write to me either c/o Mrs. Hicklin, 65 ~~Richmond~~ Ridgemount Gardens, W.C.1., or to Sir Philip Hartog, Woburn House, Fourth Floor, Upper Woburn Place, W.C.1.

I hope that within one year there will be a more general solution of the problem with which we have to deal.

With kindest regards,

Yours Sincerely,

From : Dr. LEO SZILARD,

IMPERIAL HOTEL,
LONDON, W.C.1.

May 7, 1933.

Dear Dr. D.,

Enclosed you will find an outline of the work before us.

I would like also to inform you of my part of the work. I got in touch in Vienna with Sir William Beveridge, the Director of the London School of Economics, who happened to be there, and I discussed with him and ~~other~~ some friends the situation.

Sir William Beveridge promised to try and enlist the sympathies of one or two of the universities, and since his return to London he has been very active in this respect.

Although I cannot as yet say definitely what may or may not be the final result of the interviews which have taken place between Sir W. Beveridge and the Vice-Chancellors of London, Cambridge and Oxford universities, I feel certain that within a month or so we shall have an English group under the leadership of some outstanding personality who will undertake to raise funds, and I feel equally certain that such funds will be applied to good purpose.

I do not wish to interfere in any way with the formation of the English group which is entirely in the hands of English university people; nor can I represent such a group in any way. What I am concerned with at the present is to co-ordinate the foreign groups which are already in existence, and to stimulate the formation of groups in countries where there are no suitable groups as yet.

Of the different groups already in existence, I would like to mention the Committee of the Jewish Board of Deputies and Anglo-Jewish Association, appointed for the purpose of awarding fellowships to exiled Jewish scientists. Sir Philip Hartog is the Chairman of this Committee, of which I have attended the first meeting.

Sir Philip Hartog will, I am convinced, see that nothing should interfere with the formation of a broader English group. I also had a long and satisfactory interview with Dr. Weizmann, to that effect.

All going well, in England, I am free to leave for Belgium where I have an appointment with the Director of the Liege University, Mr. Duisberg, on Saturday next, May 13th. in Brussels (I shall be at the Fondation Universitaire). I hope he will take up the matter with the

P.T.O.

other Belgian universities.

You probably know that Dr. Liebowitz is in touch with the Anthropologist Franz Boas of Columbia University and that he has had an interview with Niels Bohr in Copenhagen. He has arranged an interview between Bohr and Boas and hopes to hear soon from Boas about the steps which have been taken in the U.S.A. as a result of that interview.

I had conversations here with Niels Bohr, Harald Bohr, Sir John Russell (Agricultural Chemistry), A. V. Hill (Physiology), Professor Hardy (Mathematics, Trinity College, Cambridge), and Donnan (Physical Chemistry). They all agree regarding the spirit in which constructive work should be carried on and would be glad to co-operate in one way or another if funds were available.

It seems now to be important that an international Board of some twenty scientists should be created, and I hope to have conversations with some personalities on the subject whom we would wish to be chairman of such a Board. I hope I can do something along these lines before I leave for Belgium.

This is all the information I can give you today, and also I am not able to make any suggestions as to the details of how to co-operate with Bristol University. If you have an opportunity to discuss this matter with your friends there, please do so. I hope Dr. Liebowitz will be able to send you within the next 48 hours information of a confidential character. He will act instead of me during my absence from London in the next few days. He would also be glad if required to come to Bristol and discuss the matter with you personally, if the Vice-Chancellor should care to have more detailed information.

Yours very truly,

other Belgian universities.

You probably know that Dr. Liebowitz is in touch with the Anthropologist Franz Boas of Columbia University and that he has had an interview with Niels Bohr in Copenhagen. He has arranged an interview between Bohr and Boas and hopes to hear soon from Boas about the steps which have been taken in U.S.A., as a result of that interview.

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Yours very truly

1155 East 57th Street
Chicago 37, Illinois

June 21, 1948

Professor M. Delbruck
Department of Biology
California Institute of Technology
Pasadena 4, California

Dear Delbruck:

When Luria was here a few weeks ago he told us that he finds a phenomenon which you described in your Cold Spring Harbor paper very much more markedly with T_2 which has been exposed to ultraviolet irradiation. Novick and I thought we would look a little into your phenomenon (without using ultraviolet) and the enclosed memo, which I wrote for our own files, gives you a summary of ouf experiments.

If this note reaches you in time, please try to telephone me on the 23rd of June in Chicago at MIDway 0800, Extension 1785, or Extension 717. If that fails, try BUTterfield 4800, Extension 61

Sincerely yours,

Leo Szilard

LS:am
Enc.

June 21st/48

HASTY SUMMARY OF EXPERIMENTS WITH PHAGE FROM MIXEDLY INFECTED B

(Memo written up by Szilard)

Novick and I find that if we infect mixedly with T_2 and T_4 the B strain of coli (10 T_2 and 10 T_4 per bacterium) the filtrate shows the following properties.

If we add B/2 to a certain quantity of the filtrate, allow five minutes for adsorption, dilute with broth, and plate (before lysis) on B/4, we obtain a certain apparent T_2 titer (roughly about the same T_2 titer is obtained if we plate on B/4 without first adding B/2). If however we add to such a sample of the filtrate B/2, allow five minutes for adsorption, dilute with broth, then incubate for 1¼ hours to permit lysis, and plate (after lysis) on B/4, we find an apparent T_2 titer, which is about ten times higher. This is accompanied by an increase in the T_4 titer (as defined by plating on B/2) by a factor of about 100.

We tried to repeat this process, ~~that is~~, in the sense of adding B/2 to the lysate, and incubating to permit lysis, but found no appreciable increase in the apparent T_2 titer (as defined by platings on B/4), at least not under conditions under which there still was further increase in the T_4 titer by a factor of about 10.

Control experiments carried out with ~~[illegible]~~ mixtures of T_2 and T_4 showed no increase in the T_2 titer when allowed to be adsorbed on B/2, diluted with broth, and incubated for 1¼ hours. A decrease rather in T_2 titer appeared to occur in these experiments.

We concluded that under the conditions of our experiment, B/2 upon lysis yielded phage which behaves like T_2, inasmuch as it can plate on B/4, and does not appear to be able to grow further in B/2.

This phenomenon ~~can be repeated~~ we could reproduce also under conditions in which multiple infection of B/2 can be neglected. We conclude therefore that the phenomenon ~~mentioned~~ is exhibited by a single phage particle, or else by a number of phage particles sticking together.

We convinced ourselves however that we don't have to deal here with phage particles composed of T_2 and T_4 particles, which might stick together and might enter jointly B/2. This possibility was ruled out by platings on a mixed indicator (B/2 plus B/4).

When we added a sample of the filtrate to a certain quantity of B/2, in such proportions that multiple infections of B/2 could be neglected, allowed five minutes for adsorption, and plated before lysis on the mixed indicator, we obtained no clear plaques. Phage particles composed of T_2 and T_4 entering jointly B/2 and giving a mixed yield of T_2 and T_4 would cause the formation of clear plaques.

In order to obtain more information about the phenomenon involved, we made a single burst experiment, as follows.

A growing culture ~~made under the following conditions~~ of B (at about 5×10^7 bacteria per cc) was mixedly infected with T_2 and T_4, and after allowin five minutes for adsorption, the bacteria were freed from virus by centrifugation, were resuspended in broth and, before lysis could occur, a drop of the suspension, containing on the average two infected bacteria, was placed in each of 100 test tubes. Each test tube contained 0.25 cc of broth, and after allowing 1½ hours for lysis, o.1 cc from each test tube was plated on B/4 (100 plates in the first series). Subsequently, B/2 was added to the remainde in each test tube, and after about two hours of incubation, the contents of each test tube were plated on B/4. (100 plates of the second series.) T_4 wa added in the platings of the second series to the agar in order to remove the

B/2 and thereby to avoid turbid plaques.

On the basis of the 10-fold rise in the apparent T_2 titer (corresponding to our results described above) we should expect the total number of plaques in the second series of platings to be about 15 times as great as the total number of plaques obtained in the first series of platings. The actual ratio found was, however, 9. The difference might perhaps be explained by some loss of titer due to the too long incubation of T_2 particles with B/2.

Fourteen of the 100 plates show no plaques (or one plaque at the most) in the second series of platings, and these 14 plates do not show a significant number of plaques in the first series of platings either (zero plaques, one plaque, two plaques at the most).

In contrast to this, there are 47 pairs of plates which, in the first series of platings, show no significant number of plaques (three plaques at the most, and an average of 1.5 plaques perplate) but which in the second series of platings, show an average of 80 plaques. 16 out of these 47 pairs of plates show no plaques in the first series of platings.

We conclude that in the case of a mixed infection of B with T_2 and T_4, under the conditions of our single burst experiment, an appreciable fraction of the bacteria which yield none or only a small number of T_2 particles, that plate on B/4, will yield particles which by growing in B/2, can produce on the average 80 particles that will plate on B/4.

Our experiments so far are consistent with the assumption that about 10% of the T_2 particles yielded by ~~B~~ a B culture in a mixed infection, under the conditions of our experiment, carry along with them some material of T_4 character, which enables these particles to enter into B/2 and to grow there.

Bodaluff

1155 East 57th Street
Chicago 37, Illinois
July 22, 1949

Professor Max Delbruck
Department of Biology
California Institute of Technology
Pasadena, California

Dear Delbruck,

Many thanks for the copy of your semi-annual report which I read with great interest. Novick and I early this last winter have also performed the serum experiments which you describe and got the same result. Perhaps we will be able to talk about it a little this summer. I plan to spend ten days in Pasadena from about August 20th to September 1st. Do you think you can put me up in the Atheneum for that period of time?

Enclosed is also a manuscript which we at last completed and which might interest you. Please pass it on also to Dubecco and Luria and to whomever else might be interested out there.

Yours,

Leo Szilard

m
Encl.

From our semi-annual report to the Natl. Fdtn. Inf. Paralysis for period Jan 1 - Jun. 30, 1949

Hope to see you here some time this summer.

R. D.

July 1, 1949

...) (Cold Spring Harbor Symp., Quant. Bioc. 11, 33-37). Our previous finding may be stated as follows: bacteria mixedly infected with T2 and T4 liberate mixed yields of T2 and T4 particles. Of the T2 particles a considerable proportion fails to form plaques on the indicator strain for T2, but will form plaques on a mixture of the indicator strains for T2 and T4, respectively.

In 1948 Drs. Szilard ~~et~~ and Novick at Chicago looked a little closer into this matter and concluded from their experiments that the T2 particles liberated from the mixedly infected bacteria consist of two types. One of the types behaves in every respect lik normal T2 particles. The other type behaves as if it carried some material of T4 character which enabled the particles to enter the indicator strain for T4, to multiply there, and to emerge as normal T2 particles (personal communication from L. Szilard, dated June 21, 1948, not published).

My experimental results are in full agreement with the experimental results of Szilard and Novick. They definitely support the view of two types of T2 particles. The new type may be referred to as T2(4) particles, the "T2" referring to its genetic character, while the "(4)" refers to its transient phenotype which is similar to T4, and which is lost after passage through one bacterial host organism. My experiments have added one detail to the characterization of these particles. If tested with antisera specific against T2 and T4, respectively, these particles are found to be as sensitive as the corresponding homologous antigens to both these antisera. In other words the T2(4) particles combine the serum sensitivity of T2 and T4 particles. I am uncertain as to the correct interpretation of this unexpected finding. It has previously been assumed, on the basis of detailed investigations by A.D. Hershey, that antibodies inactivate bacterial viruses by combining with a specific site on the virus surface. The normal function of this site is not definitely known. Our finding would seem to indicate that the T2(4) particles possess the sensitive sites of T2 and of T4, and that a block at either of these sites inactivates the virus. Further studies must show whether this notion can be corroborated or whether a deeper revision of our views on inactivation of viruses by antibodies is necessary.

On April 7, 8, and 9 a meeting on bacterial viruses took place at Bloomington, Indiana, which was attended by about twenty interested research workers including all four members of our group.

July 29, 1949

Dr. Max Delbrück
Biological Laboratory
California Institute of Technology
Pasadena 4, California

Dear Dr. Delbrück:

I have forwarded your note of the 27th, together with your letter from J. Monod of the Institut Pasteur and the paper on "Biochimie Bacterienne", to Professor Szilard in Colorado. Your postcard dated July 26th has also been forwarded to him there.

Sincerely yours,

Norene Mann (Mrs.)
Secretary to
Professor Szilard

m

February 6, 1951

Dr. Max Delbrück
Kerckhoff Laboratory of Biology
California Institute of Technology
Pasadena 4, California

Dear Delbrück:

I have now read with pleasure your manuscript on T2 (4), which is not going to be published.

In the meantime, our paper has appeared in SCIENCE, and I had an exchange of letters about it with Frank Horsfall. I would like to show him your manuscript, and, unless I hear from you to the contrary, I shall assume that you have no objection.

Had your manuscript been published, I would have been somewhat embarrassed by your attributing to me the interpretation of the phenomenon which we now believe to be the correct one, and to Novick and me the experimental verification of this interpretation. What the facts of the matter are, I no longer recall, but, in any case, it would seem better not to go behind the façade of joint authorship, unless a real idea of considerable originality is involved, which is certainly not so in this case. Ideas of a minor sort frequently crop up in the course of conversations, in which case, authorship of the idea is incapable of definition.

My only reason for bringing up this point here is the assumption that whoever will continue your experiments will also inherit your presentation of the history, and I would appreciate it if, when the time of publication comes, the reference to my authorship of the interpretation, as separate from the experimental verification of the interpretation, were omitted.

I expect to go West some time toward the end of February - if all goes according to plan - and would then hope to see you at Cal Tech.

Sincerely,

Leo Szilard

LS:hw

CALIFORNIA INSTITUTE OF TECHNOLOGY
PASADENA, CALIFORNIA

DIVISION OF BIOLOGY

Sz 67121

March 5, 1956

Dr. L. Szilard
University of Chicago
Chicago 37, Illinois

Dear Szilard:

I wonder whether you would like to give a Biophysics Seminar when you come here later this month. This seminar meets on Monday 4 p.m., and I suppose that March 19 would be the only likely date for you. If you prefer another day of the week, let me know and we will se whether we can arrange to put on a Special Seminar. In any event, if you are willing to give a seminar let me know a title in advance, so I can put out an announcement for the interested parties.

With kind regards.

Sincerely yours,

Max

M. Delbrück

5712
Vienna

MD/am

Sz 8480

Strauss Lewis L.
AD 40700

Friday

~~With~~

June 14, 1957

Dr. Max Delbrück
Division of Biology
California Institute of Technology
Pasadena, California

Dear Delbrück,

Enclosed is a manuscript, together with a copy of a letter which I wrote to Alex Rich that will furnish you such additional information as is needed.

If your time permits, I would be very grateful if you could look through this paper. As you know, this is not exactly my field, and so I would appreciate it if you would let me know if you see that I have overlooked some basic consideration. I understand that for another ten days or so I can withdraw this paper without causing anyone any appreciable inconvenience.

With kind regards,

Sincerely,

Leo Szilard

m
Encl.

June 24, 1957

Dr. Max Delbrück
Division of Biology
California Institute of Technology
Pasadena, California

Dear Max,

Since I wrote you about a week ago, I looked at known amino acid sequences in polypeptides in the hope that they would throw light on the mechanism of polypeptide synthesis. The attached memorandum gives the conclusion to which they lead. Since my manuscript was to be held until further notice, I can now withdraw it without any trouble, which I shall do without delay.

I am pursuing further the considerations relating to the rate of protein synthesis, and shall probably incorporate parts of the manuscript which relate to this problem into another paper that is in preparation.

With kindest regards,

Sincerely,

Leo Szilard

m

P.T.O.

P.S. Could you drop me a line to say where you will be when between now and August 15th? I am going to spend September and part of October in England and Germany (I am supposed to give some talks in Germany - Hdidelberg and Berlin -) and I might want to come to see you and have a heart-to-heart talk with you about Germany in general and in addition certain details which are of particular interest to me.

Some time ago Teller told me about a conversation he had with Weizsäcker, in which Weizsäcker suggested that the Ten Commandants ought to be rewritten inasmuch as they have been written a very long time ago and have apparently never been revised. I thereupon made an attempt to write a new version which I cam across a few days ago in going through my files. You will find a copy attached to this letter.

Sz

Encl.

Dear Trudi:

in thinking what I could do to express my affectionate regard to you and Leo I decided to send you a book "The City beyond the River" a German post-war book, which Gunther Stent had shown me, and with which I then lived for a while, and Manny lived with it too, when we were in Köln, during the Summer Semester of 1956 (it was then still a city of many ruins). Perhaps you know the book, perhaps you will not like it, nor may Leo want to look at it: no matter, I just wanted to send a greeting, and one that was not just Science (that will come, too!)

Yours

Max D.

Jan 25, 1960

M. Delbrück
1510 Oakdale St.
Pasadena 4, Calif.

May 4.
Pasadena.

Dear Szilard:
Thank you for the book of short stories. I read the first two, but before I got a chance to finish the others, Max passed it on to a traveller on his way to Texas. (He had asked a question so pertinent to the ideas of the first story, this was the answer he got).
I retold the stories to Jonathan (who is now fourteen) last Sunday, as we climbed a mountain together. As this is the type of thing Jonathan likes to think about (and has his theories too), we got to the top without noticing the climb. It appealed to us to "freeze up" for awhile (as in the Marks Table

Foundation Story, but were worried ~~that~~ over unfreezing might be overlooked.

It was fun to see you a few months ago in a TV show ~~with~~ being mischievous with Teller, and to hear from time to time ~~that~~ you've been seen in places far from home, and ~~that~~ you're very active. Keep it up, it's very good news—

The Delbrücks will descend on Europe (with ~~the~~ concentration on Köln) this August, for two years. The new Genetics Institute ~~there~~ is almost ready to go into operation. Meanwhile we savor the deserts of the West, and the man-less wilderness ~~that~~ is not far from us, and we have fun every day with our year-old baby, Tobias.

Live well!

Yours Manny Delbrück

1510 Oakdale St - Pasadena.
Feb 8-1960

ack May 15/60

Dear Szilard -

Ever since I heard of the bad luck of your illness, I've been wanting to send you a response; the feeling never seemed to form into words, though — but, really, want am I stalling about?

It's been great fun and very interesting to know you, and I count it as fortunate for me that we happened by coincidence to co-exist in the world. Including you, the boundaries of what is the human family have seemed to extend, to make me more at ease in this family.

Maybe a strange thing to be thankful for, but I am — so I thought I'd let you know.

Sincerely,

Manny Delbrück

D

Mrs

M. DELBRÜCK
1510 OAKDALE ST.
PASADENA 4, CALIF.

A1(

CALIFORNIA INSTITUTE OF TECHNOLOGY
PASADENA, CALIFORNIA

DIVISION OF BIOLOGY

February 8, 1960

Professor Leo Szilard
Rockefeller Institute for
Medical Research
66th street and York Avenue
New York, N.Y.

Dear Leo:

I have to take back what I wrote in the P.S. of my last letter. It is indeed easy to construct graphically a case in which the system has three equilibrium states and to see graphically that the outside states are stable.

Choose $e^* = 2$
$K^* = 10^{-2}$
$K = 10^{-4} = (K^*)^2$

Then the critical enzyme concentration is about 1 and the critical repressor concentration is 10^{-2}. Looking at the three graphs proposed in my letter, and choosing $e_a = .5$ and $e_b = 1.5$ as test concentrations of enzyme, one obtains from the dissociation graph the corresponding repressor concentrations r_a about 1/2, r_b smaller than 10^{-2}. Then from the de/dt graph one obtains that de/dt is negative for e_a and positive for e_b, Q.E.D.

I still think, though, that this kind of rigorous treatment of a model based on primitive mass action kinetics is pretty much a SPIELEREI, and that the more essential thing is to point out the required topological characteristics of the curves describing the equilibria.

Yours,

Max

Max Delbrück

MD:mc