

THE UNIVERSITY OF WISCONSIN
COLLEGE OF AGRICULTURE

Madison, 6

DEPARTMENT OF GENETICS

Winder +

Francis →
Semi-
Ramus

October 4, 1951.

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

Dear Szilard:

Thank you for so expeditiously looking for arrangements for Klieneberger. I just had a letter from her suggesting that she might come to the midwest about the first of December, to give her time to visit on the East Coast. I had not previously mentioned the possibility of her stopping-over at Chicago, but feel sure that she would like to.

I am not sure that I understood just what kind of work you had in mind with the "L-forms" (if that is what Zinder's granules are) although there is certainly an unlimited range of possibilities. Since you have expressed your interest, I wonder if I might not bring a constructive possibility to your attention.

Zinder will be "graduating" this Spring, and looking for a job. He will be available July 1, having earned his Ph.D. in Medical Microbiology and Genetics. What would you think of the possibility of his working with you either as a postdoctoral fellow, or preferably on some sort of staff appointment? You have probably seen him enough to be able to form your own impressions of his capabilities. He has finally learned how to design an experiment; he has always had a quick intelligence and been an industrious worker (at least for the work that interests him). From what other people have said, I may have an inadequate appreciation of him myself, possibly from having had to live with him during his more formative years. I have no doubt that as he matures, he will become an increasingly good, possibly an excellent researcher. He is well capable of responsibility in a research program, but I think can still benefit from a sympathetic but stern handling of his captious ideas. He is a very congenial person in the laboratory. I have no hesitation in recommending that you take him on, if you could work it out. I am also sure that he would benefit from the association.

One additional desideratum of such an arrangement would be the possibility (though of course not the obligation) of collaborative work on aspects of mutual interest.

Zinder supports two dependents, and would probably regard \$4000 (possibly somewhat more for a HCL area) as a minimum salary.

Both of us would appreciate your comments.

Sincerely,
Joshua
Joshua Lederberg

5650 Ellis Avenue

October 8, 1951

Professor Joshua Lederberg
Department of Genetics
College of Agriculture
The University of Wisconsin
Madison, Wisconsin

Dear Lederberg:

Many thanks for your letter of October 4th.

I appreciate your letting us have an opportunity to consider Zinder. After talking this over with Novick and looking into the question of funds, I shall let you have ^{my} ~~all~~ comments at an early date.

Sincerely,



Leo Szilard

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Geo Saitz

Secretary

You have my comments at an early date
and looking into the question of funds. I shall get
contacted. When we're talking this over with you
I appreciate you, telling us how an opportunity to

will think for you, letter of October, 1951.

Dear Professor:

Richard, Mason
The University of Wisconsin
College of Agriculture
Department of Genetics
Professor James G. Thompson

October 9, 1951

2000 Hillside Avenue

to Norman Zinder

Please return for my files
L. R.

1155 East 57th Street
Chicago 37, Illinois
December 12, 1951

Dr. Joshua Lederberg
Department of Genetics
University of Wisconsin
Madison, Wisconsin

Dear Lederberg,

I am writing to say that both Novick and I were very much pleased with Zinder's visit. After we had talked to him for two days, we asked a few people to come over and listen to him giving a seminar. We wanted just a few people but apparently word gets around on the Campus and Zinder spoke before a distinguished audience which filled the seminar room. His talk was very good both in substance and in delivery, and the people were quite impressed with his maturity.

I looked through the draft of Zinder's manuscript which I understand he has not yet shown to you. It seems to me that it contains the nucleus of an historically very important paper, and because this is so, it might be worth while to give some thought to its organization and also to the place where it may be published. I believe that if the paper is properly organized and all extraneous matter left out of it, such as references to the possible connection of the phenomena with L-forms, for which I could not see any real evidence presented, the paper could be quite short and lucid, and if it were short and lucid you might consider having it submitted to the Proceedings of the National Academy of Sciences where it would have a much broader audience than in any of the more specialized journals.

As to organization of the paper, it seems to me that if the paper started out by telling the story of the discovery; i.e. by explaining what was attempted, and how the "contraceptive experiment" gave a startling result, and if this were then followed by clearly setting out what the subsequent experiments in the author's opinion have established, it would then be much easier to understand the individual experiments that are described and the results ~~which~~ of which are consistent with the conclusions presented at the outset of the paper. The presentation of ^athe paper is, of course, entirely a matter of individual taste but I find that unless I know in advance what the author thinks he has proven or made plausible, I have to read the paper twice instead of only once, for until I know what the author thinks he has proven it is not possible to read with the proper critical attitude the individual experiments which are listed in support of the author's contention.

As to the substance of the matter, what I would be most eager to know is the following: Could it be that the transducing agent, which is of course of bacterial origin, is somehow incorporated in a phage particle which is *released* from the donor bacterium, and that this phage particle acts as a vehicle of transmission? This would of course mean that the phage released by the donor strain must in all cases be absorbed by the strain which is transduced even though this phage need not grow in the strain which is transduced. Zinder tells me that typhi which is transduced is not susceptible to the phage of the donor strain, but the question still remains whether it absorbs the donor strain.

One of the reasons why this question appears to me as having some urgency is the sad fact that if indeed the phage should act as a vehicle of transmission, then further investigation of FA will necessarily remain a much more restricted field than it would be otherwise. So for setting out a general plan for

Dr. Joshua Lederberg

-3-

December 12, 1951

the investigation of this field, the question which I am raising here is a rather crucial one. ^PI told Zinder that we would like to have him in Chicago and that we shall see what we can do about this, ^{but} ~~and~~ that he should in the meantime not neglect other opportunities which he may have, ^{and} ~~but~~ just keep us informed of how his plans develop and what opportunities he thinks he may have elsewhere.

I had a letter from Mr. Pomerat of the Rockefeller Foundation about the Klieneberger-Nobel plans for a visit to America. I assume that he wrote you also but if not I shall forward you his letter after it has been answered.

With kind regards,

Sincerely yours,

Leo Szilard

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THE UNIVERSITY OF WISCONSIN
COLLEGE OF AGRICULTURE

Madison 6

filed

DEPARTMENT OF GENETICS

December 17, 1951

Prof. Leo Szilard
Institute of Radiobiology & Biophysics
University of Chicago
Chicago 37, Illinois

Dear Szilard:

Thank you for your letter which just arrived with its comments on Norton Zinder and the paper we are writing.

Zinder may have left you with a misapprehension. Although I have not gone over this draft of our paper as yet, I have spent a good deal of time and thought over it, and am willing to accept any onerous responsibility for its organization.

I agree with you that a somewhat fuller statement of our conclusions should be included in the introduction. This is the supposed function of a summary, and I always read this part of a paper first. Some of the English journals have adopted the happy solution of placing the summary at the beginning. But in general, I think it does no harm to read a paper twice: first for perspective, then for detail.

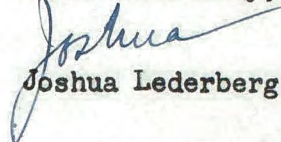
As to the place of publication, I hope you will agree that this is a matter of personal (and editorial) preference. So long as we do not publish in the modern equivalent of the Abhandl., Verh. naturf. Ver. Brunn, it does not really matter a great deal. A preliminary account of this work is included in our ms. for the Cold Spring Harbor Symposium which will possibly appear before our definitive paper. It will not be possible to present the experimental evidence in the detail necessary for other workers to repeat the experiments in less than fifteen or twenty pages. The irrelevancies to which you refer occupy about 1% of our intended manuscript. I admit that we could have made a brief, preliminary announcement in the Proc. Nat. Acad. Sci., and we would undoubtedly have done so if the Cold Spring Harbor Symposium had not interposed an equivalent opportunity to bring the work to the attention of the most interested people (like yourself). At the present time, I am inclined not to present a summary paper that will be followed immediately by a fuller account. Either Genetics or Journal of Bacteriology should be satisfactory, and we will consider both— the latter probably first since it has a wider circulation, prompter publication, and (to me) a more pleasing format. I might point out further that with a few enlightened

exceptions (such as the University of Chicago), Zinder may have to depend upon his reputation among bacteriologists for the advancement of his professional career. I want to point out again that Norton has yielded to me on this matter and stands entirely neutral (I hope).

The experiment you suggested (on the relationship of phage to FA transmission) is an important one, and Norton intends to carry it out. There are strong hints of it in the refractoriness of certain lysogenic derivatives in intra-strain crosses. Unfortunately, our definitions of bacteriophage are limited by experimental criteria. The phages lytic for *S. typhimurium* may not be adsorbed by *S. typhi*, but this does not preclude the participation of other "nonlytic" phages-- the semantic difficulties are obvious. Have you ever speculated on the interesting results of a lysogenic association in which a phage might occasionally burst the bacterium in which it's growing, but which could not initiate plaques (massive lysis) when introduced to other, uninfected bacteria. Such an adaptation is predictable, if not detectable.

If Norton does join your group, I had hoped (in agreement with yourself, I am sure) that he would continue to study just this aspect of the problem: the physical nature and organization of the FA. I am very pleased at your indicated favorable reactions to him, and hope that this possibility will materialize without undue delay.

Yours sincerely,


Joshua Lederberg

THE UNIVERSITY OF WISCONSIN
COLLEGE OF AGRICULTURE

Madison 6

DEPARTMENT OF GENETICS

December 16, 1956

Professor Leo Szilard
Institute of Radiobiology and Biophysics
University of Chicago
Chicago 37, Illinois

Dear Szilard:

Each year, the Graduate Biological Division of the University of Wisconsin sponsors a visit by an outstanding scientist to the various departments engaged in biological research on this campus. As a rule we ask that the visiting lecturer spend two or three days here, to allow a leisurely contact with his friends and colleagues here. We also ask that he give a public lecture on a topic of general biological interest, in addition to one or more special lectures or seminars for more specialized groups. We have available a stipend of \$300 to serve for an honorarium and expenses.

On behalf of the lectureship committee, I would like to extend a formal invitation to you for this lecture-visit, and to add my own urgent solicitations for your acceptance. We are especially interested in your views on the nature and prospects of "Theoretical Biology" and hope you would consider this an appropriate topic for the general discussion. I myself have heard many comments from you in this area, and hope the time may be ripe for a more systematic exposition. I hesitate to make any concrete suggestions of a specific topic, since you have worked in so many areas that would be of immediate interest to various of my colleagues here.

I can assure you of the warm and receptive spirit in which you, and your remarks in a difficult area of speculation, would be received, and can add to them my own hopes, and those of your other good friends here, that you will be able to accept this invitation.

We would like, as far as possible, ~~the~~ to suit your convenience in scheduling the visit. However, Esther and I will be away ourselves for most of March, and the Spring recess extends April 19-28. May I open our discussion, then, by suggesting the intervals April 1-15, or May 1-15, for your first consideration, perhaps preferably the former.

Yours sincerely,

Joshua Lederberg
Joshua Lederberg
Professor of Genetics

answered

P.S. Please don't hesitate to let me know if we can do anything to make your visit more pleasant, above all, more certain. We will be happy to discuss any other arrangement you might propose. Best wishes, Joshua.

P.P.S. I hope you can understand this - I've given up the ball-point pen.



International Hotel

N. Y. INTERNATIONAL AIRPORT ★ JAMAICA 30, L. I., N. Y.

5 Dec 58

My dear Szibud —

I'm sorry I didn't have much time to look at your ms. I did send it back to Jim Crow and think you should discuss it with him. His work on the effects of consanguinity (PNAS a year or 2 ago, with Morton + Muller) is one approach to measuring incidence of faults.

I have to read the theory more carefully. The idea of "vegetative genes" may have some support in ~~some~~ nullisomic cell lines of tumors in Chinese hamsters (Yergeman) — I will send you a discussion of this (Gallinburg sympos. in press) on my return.

One point may be taken care of but I want to see. Certain homozygous faults are not fully ~~the~~ lethal but as one-gene effects do cause specific shortening of life: (gross examples, hemophilia or diabetes). Isn't an oversimplification



to talk about genetic effects on longevity
as mainly due to the distribution of numbers
heterozygous "faults"? But as I say
I have to give your paper more than a
casual reading.

If you haven't done so, I suggest you
include a discussion of longevity as of (inbreeding
coefficient) according to your theory as this may
furnish another test.

Yours
Joshua

The University of Chicago
Chicago 37, Illinois
April 7, 1955

Mr. Joshua Lederberg
The University of Wisconsin
College of Agriculture
Madison 6, Wisconsin

Dear Lederberg:

Many thanks for your letter of February 18. I am so swamped with correspondence as a result of my Letter to the Editor, that I have difficulty in catching up. I would like to come to Madison, but I have no immediate plans to go to the Mid West, (hanging up mostly in the East in New York at the King's Crown Hotel, 440 W. 116th St.) and lately also in Washington.

Still I shall keep your letter in mind and perhaps something can be arranged.

Enclosed you will find a clipping and also a copy of a memorandum which states further what I am trying to do. I have little hope to be able to raise the necessary funds from any of the large foundations.

Sincerely yours,

Leo Szilard

THE UNIVERSITY OF WISCONSIN
COLLEGE OF AGRICULTURE

Madison 6

DEPARTMENT OF GENETICS

February 18, 1955

Dr. Leo Szilard
University of Chicago
Chicago 37, Illinois

Dear Szilard:

I saw your letter in the N.Y. Times a few weeks ago, and I must say that I shared your pessimism about the course of coming events. At the time, I must admit too that I thought your proposal that the savants of the universities address themselves more coherently to the problem of world peace was naive; I must have been thinking subconsciously about it since then, your proposal now seems merely desperate.

I am sure that there is nothing in desperation or otherwise that I can individually do to help, but I did want to offer you my moral support. Can you do something of the same in return? I have talked to a few people here at Madison about it, and we agreed we should like to hear more from you on the matter. That is, I think that Madison and this university might be fertile ground for your proposal, and I say this in hopes of attracting you to visit us to elaborate on it. Do you think you might have the time to spend a day or two here, and if so, when would be convenient for you?

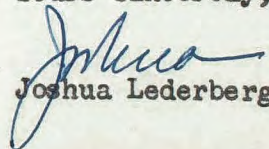
This, so far, is purely unofficial, and if you wanted to limit your visit to an entirely informal one, with individual discussion and perhaps a group luncheon with the local F.A.S., that could be arranged with hardly more than an affirmative nod from you. If it would interest you to give a more formal lecture, on any relevant topic, I am sure this could be readily arranged, though it might take a little longer to clear the finances. It goes without saying that we will meet your expenses, whether personally or officially.

Frankly, it hardly seems likely that any solution is to be discovered; if this is taken as a fact, perhaps it raises its own questions in turn, whether there would be anything left to salvage, would it be worth it, could this be helped. But however futile these efforts, they might be a contractive catharsis in contrast with the neurotogenic load we all carry of repressed not-thinking about the H-bomb, and so might have their immediate benefits too.

Do let me hear from you, and I hope favorably on your visit.

Yours sincerely,

P.S. Have you any copies of your letter. I am sorry I forgot to clip it and would like to have one or more to show around, (which must have been your purpose in writing it).


Joshua Lederberg

Washington, D. C.
March 3, 1962

Professor Joshua Lederberg
Professor of Genetics and Biology
Stanford University
Palo Alto, California

Dear Josh:

The attached letter is meant for you and those others whose names are listed in the memo "The Next Step". I should be very grateful to you for reading the attached letter and the enclosures, and for advising me as soon as possible whether you are willing to serve as an Associate.

Sincerely,

Leo Szilard

Hotel Dupont Plaza
Washington 6, D. C.
Telephone: HUDson 3-6000

Enclosures

P.S. I am enclosing the revised and final version of my speech, which will be printed in the April issue of the Bulletin of Atomic Scientists.

LS

JOSHUA LEDERBERG
716 ALVARADO ROW
STANFORD, CALIFORNIA

file current

AGO

March 6, 1962

list

Dear Leo -

As you know I am deeply interested in your
purposes, and in supporting the right of
your own intelligence on these issues. However,
I have enough reservations about the tactical
conceptions of the movement to wish not to
be formally identified as an associate at this
time. I hope to remain informed of your
ideas, and perhaps to use other channels to
further common objectives.

Perhaps the enclosed contributions, to use in any
way you see fit, will be of some use.

With best wishes

Joshua

Washington, D. C.
March 3, 1962

you and those others
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nt Plaza
6, D. C.
HUDSON 3-6000

P.S. I am enclosing the revised and final version of my speech,
which will be printed in the April issue of the Bulletin of
Atomic Scientists.

LS

9 March 1962

Joshua Lederberg
716 Alvarado Row
Stanford, California

Dear Josh:

Many thanks for your prompt answer to my letter of 3 March.
If
If you are not an Associate the only way I can assure that you will be kept informed is to list you as a potential member of the Movement along with some 700 others who have expressed strong interest. As you may know, you will be regarded a member of the Movement if you spend 2% of your income on campaign contributions after having listened to the advice of the Lobby. You would be regarded as a supporter of the Movement if you spend 1% of your income for campaign contributions.

I am grateful for the check which you sent me but I am returning it enclosed because I have no way of handling checks made out to me personally. If you feel you would like to contribute to my current office expenses you may send me a check made out to the University of Chicago in which case your gift will be tax deductible.

With kindest regards.

Sincerely,

Leo Szilard

P.S. Please call me up the next time you are in Washington if your time permits.