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 So long as we do not publish in the modern equivalent of the Abhandl., Verh.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, premonitory 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