Genetics Department,
University of Wisconsin
October 2, 1951.

Dear Dr. Klieneberger-Nobel:

Pending a morevdefinite indication of your plans, no fixed arrangements have been made for your visit here. However, I think that we can assure you an honorarium to cover your travel expenses from the East Coast here up to about $150, which should be ample. Professors Szilard and Novick at the University of Chicago are also very much interested in the possibility of a visit from you, and if you were willing to include a stopover with them en route to Madison, a more comforatble financial arrangement might be possible. Inasmuch as all trains and most air flights to Madison are rotted via Chicago, this would entail a minimum of inwonvenience.

We have nothing new to report on our work on L-forms. A new graduate student here is undertaking to introduce genetic markers into the "coli" 204, so far unsuccessfully. It is the first strain we have encountered which could not be made to yield streptomycin-resistant mutants. We have also noted that filtrates of T3 grown on B might appear to be sterile, and remain non-turbid, yet [illegible] produce nubmbers of colonies on plain agar. This suggests that "comebacks" may sometimes be a consequence of inhibition of bacterial growth by substances in the lysates. It would be extremely gratifying to be able to reproduce the L-cycle with this strain, for it would be technically the most appropriate for studying the possibility of sexuality from a genetic viewpoint.

I have just had occasion to read your paper in the August issue of JGM. The concept that bacilli regenerated from L-forms are better adapted to the adverse conditions than the original bacilli is not readily understood from a genetic point of view. If the initial culture is genetically homogeneous, aad we have every reason to regard this as the general situation, no amount of reassortment can produce anything different from the initial type.I would rather suspect that spontaneous mutations occur during the propagation of the L-forms, and that these may be subject to selection favoring better adapted forms, and adaptation that may persist after reversion to the bacillary form. This limitation would not, of course, hold for the recombination of factors between different strains, and this is, of course, the biologinal function of sexuality in all organisms For the same reasons, I would suggest that your reference to paper (128) [Tatum ahd Lederberg, 1947], on p/ 92 might be transposed to the p. 96. We have good reason to distinguish carefully betweem adaptive mutation and recombination of preexistent genetic differences. But I am looking forward to the opportunity of discussing these questions with you at closer hand.

Yours sincerely,

Joshua Lederberg