November 1, 1953

Dr. Selman A. Waksman
Department of Microbiology
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New Brunswick, New Jersey

Dear Dr. Waksman:

Thank you for your note of October 16, renewing your cordial invitation to offer a seminar in your department. I hope you will not find my lack of response as ungracious, but I must reply in the same vein as before. Travel schedules from Madison even to New York (not to mention further destinations) are rather irksome, and for the purposes of a seminar I would prefer to defer such a trip to a more convenient occasion. I am very sorry that my plans this last year did not, as I had previously hoped, create a congenial opportunity. There is, however, a very tangible likelihood of my travelling East next Spring, and if this develops I will hasten to beg confirmation of your invitation at that time. I hope you can sympathize with my reluctance to interrupt a busy research (and teaching) routine if this can be averted to our mutual satisfaction. It will be no news to Dr. Waksman that such interruptions ramify several days before and after.

One season I would particularly like to make this visit is to discuss the urgent possibilities of a study of actinomycete life cycles from a genetic approach, such as that summarized in the enclosure. Do you have such a program in your department? Rutgers would be a logical place for it, but my further remarks presuppose that this has not yet been developed. I did do some introductory experiments some time ago with Streptomyces griseus— only enough to make it reasonably certain that Klienberger-Nobel's description of the aerial mycelium as an obligatory diplophase arising from syngamy of different hyphal nuclei is incorrect. The incidence of prototroph formation in conidia from mixed (almost certainly heterokaryotic) auxotroph mutants is far too low. However, there was good (if circumstantial) evidence for the formation of heterokaryons following hyphal anastomosis, and for the regular segregation of uninucleate conidia (a point on which I had previously been rather suspicious) on the basis of Stanier's work on an unstable S. coelicolor). I would like to see this study completed, be it here or elsewhere. I would prefer to have a graduate student interested in a Ph.D. in genetics and microbiology, but would entertain applications from prospective post-doctorate fellows. If you happen toknow of any eminently suitable candidate, and were willing to relinquish him, I would be indebted for your courtesy.

Yours sincerely,

Joshua Lederberg