

November 5, 1951

Professor Paul R. Burkholder
Osborn Botanical Laboratory
Yale University
New Haven, Connecticut.

Dear Dr. Burkholder:

Ever since I was at OBL, I have wondered whether a genetic analysis of actinomycetes might not yield results as fruitful as on *E. coli* and other bacteria. I am sure that this thought is not unique, but am not aware that anyone else has been doing anything about it. Klieneberger-Nobel's morphological study of some streptomycetes has seemed as good a starting point as any, although her account of a sexual stage in the formation of the aerial mycelium is not entirely convincing, and seems to be virtually ignored by some actinomycetologists. A little while ago, I decided to try my hand at a genetic study with limited objectives; a) to confirm or refute Klieneberger's interpretation of the aerial mycelium as diploid, and potentially heterozygous, and b) less ambitiously, to determine whether (following Carvajal) hyphal anastomoses occur, resulting in heterokaryosis. All we need, of course, are some good markers, but I don't know at all whether the methods worked out for eubacteria will be successful.

I am writing, essentially, to ask if you have any thoughts on this matter-- in particular whether you might not yourself have contemplated or executed such a study. Perhaps you could also suggest a species that might be especially favorable from the point of view of rapid growth on minimal media, dispersibility of spores, and so on. Barring that, I had planned, of course, to work with *S. griseus*, but if you have any suggestions, and can provide a culture accordingly, I would appreciate it very much.

With best regards,

Sincerely yours,

Joshua Lederberg
Associate Professor of Genetics