

Dept Genetics
University of Wis/
Madison 6, Wis.

July 6, 1954

Dear Dr. Felix :

Thank you for your letter of June 29th. I am sure no explanations were required, and I am pleased to learn that you are now reestablished at your present station. I hope you will find that reprint from the Iowa group of some interest; this is not to say that I put any stock in their claims, particularly in view of their admission that their challenge strain of typhimurium is certainly mixed!

I deem it a privilege to assist in the deposition of your stock cultures, and am happy to accede to your proposal. Our stock collection is now maintained, partly on your own advice, in duplicate in lyophil tubes kept at 4° C., ~~AND~~ and in nutrient agar stabs at room temperature. If we could simply incorporate this material into our regular collection (of course preserving all the details as to their origin), they would be given as careful attention as is in our power. We have not used Dorset egg medium, and probably would find this inconvenient unless you feel this is indispensable— your comment would be appreciated. I would agree that this distribution is a prudent step for reasons far less catastrophic than an atomic bombing that God forbid will ever come.

May I take the occasion to inquire explicitly about the detailed history of one of your type cultures, S. typhi O-901? I do not have the reprints at hand at the instant, but one of your earlier papers refers to attempts, which did not succeed, to isolate stable O-forms; later, O-901 is referred to as a product of investigations with Olitski. I am of course quite clear that O-901 was obtained from H-901, but if the details can be resurrected I would be curious to learn precisely how it was first obtained and recognized — were any experimental manipulations involved, or was O-901 simply a casually examined single colony from an ordinary culture that proved to be non-motile? This may not be of more than historical interest, but I might add that I would be profoundly grateful for a reliable general method of isolating stable O-forms in E. coli, Salmonella, etc.

I shall be away from the laboratory from about July 20- Sept. 1, but if you wish I can readily arrange to have Dr. Bernstein look after your cultures should they arrive in the interval.

Yours sincerely,

Joshua Lederberg
Professor of Genetics

P.S. Could I impose on you for another favor, a task at which others have failed, but for which your present station at the Lister might be helpful? The E. coli strains now listed in the NCTC as 122 and 123 have been of some interest in genetic studies. They resemble each other so closely as to suggest a common origin, but there are equally impressive differences. Would there be any possibility of learning the history of these cultures— they were held at the Lister Institute, of course, before moving with the NCTC to Colindale.

JL