



COLLEGE OF AGRICULTURE AND HOME ECONOMICS
AGRICULTURAL EXPERIMENT STATION

DEPARTMENT OF ANIMAL PATHOLOGY

LEXINGTON, KY.

August 21, 1946

Dr. Joshua Lederberg
Yale University
Osborn Botanical Laboratory
New Haven, Connecticut

Dear Doctor Lederberg:

Your letter of August 4 arrived while I was on vacation therefore has not been answered at an earlier date.

Frankly, the ideas which you advanced are not those which I have held regarding phylogeny and evolution in the *Salmonella* group. However the fact that I am not entirely in agreement with your theories does not mean that they may not be absolutely correct.

As you will see in some of the enclosed reprints, we were able to change some antigens and transform some types by induced variation. We have unpublished results in which similar changes were accomplished. Also, we observed loss variation taking place in some cultures and were able to uncover lost phases in other cultures. Therefore, we have visualized the development of *Salmonella* types by the processes of induced variation and the loss variation hypothesized by White in 1926. I must admit that these theories leave many facts unanswered and, while I am not too enthusiastic about your theory of recombination, we will be glad to collaborate with you to the extent which we are able. Having long been interested in *Salmonella* phylogeny, Bruner and I would not wish to leave any stone unturned which might yield valuable information.

The cultures and serums for which you asked have been sent you. The *S. abortus bovis* cultures are normally diphasic and should be examined before you use them. You probably will find these phases mixed. I have never worked with this type to any extent and do not know much about the stability of the phases. The *S. typhi murium* cultures are monophasic and you will find them as labelled. Whether these are what you want, or whether you prefer cultures which are diphasic I did not know. If naturally diphasic cultures are used you would have to confirm the condition of the culture at the beginning of each experiment. Let me know your pleasure in this regard.

The amounts of serum sent were small but they can be used at 1-1000 so 1 or 2 ml will suffice for many tests. A very convenient method is to make 1 or 2 ml of a 1-20 dilution and add .02 ml to 1 ml of antigen. A 18-24 hour broth culture which has

Dr. Lederberg - 2

been diluted with an equal amount of 0.6 formalinized saline serves well as antigen. I have asked that a copy of circular 54 be sent you. The methods are explained therein. The serums may be diluted 1 to 50 or 1 to 100 and used in slide tests to determine the phase of individual colonies. This should save you a good deal of labor.

The organisms are only mildly pathogenic and if ordinary precautions are observed you should have no difficulties with them.

I was in New Haven in March and passed by your laboratory at that time. Unfortunately, I do not expect to be in the east again in the near future. If you run into too much difficulty I would suggest you talk to Borman or Wheeler in Hartford. Both are solid men and both understand Salmonella serology. If I have not sent what you need please inform me.

With best wishes,

Sincerely,



P. R. Edwards,
Bacteriologist

PRE:d