

THE UNIVERSITY OF WISCONSIN
COLLEGE OF AGRICULTURE

Madison 6

DEPARTMENT OF GENETICS

July 17, 1953

Dr. E. S. Anderson
Central Enteric Reference Laboratory
Colindale Avenue
London NW. 9, England

Dear Andy:

Your two letters received within the last few days. The first did not alarm me; I had forgotten just when I had mailed my note, and assumed you would receive it in due course.

On the E_1 - E_2 business, Edwards has been trying to dig up the inter-transformed strains of his older experiments. They may be scattered from Ithaca to Lexington, and he may have to repeat the experiments from the beginning. I hear a rumour that Bruner has gotten alterations in the C sub-groups, but have no definite knowledge of it.

We can now tell you some more of our summer plans: we are driving out to California in about a week, to attend the S.A.B. meetings in San Francisco, in connection with a prize-lectureship. We will be back before the end of August. This will be a moderate interruption in work (which has been difficult anyhow on account of the weather) but can be written off as vacation. I hope you will manage to get to Rome and enjoy the proceedings despite the climate.

There will be some people here while we are gone to put any letters, etc. in the frig', but there need be no hurry in your sending any of the promised materials during the next few weeks.

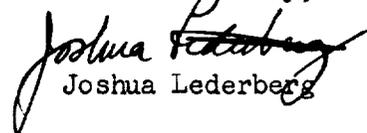
I did some surveys over a batch of phages from Edwards, and especially the paratyphiB typing phages. I had some difficulty in getting some of these to a decent titer (especially Dundee), and if it is not too much trouble I wonder if you would be kind enough to send me a set of authentic samples sometime, along with the typhimurium material. Howbeit, B.A.O.R. proved to work reasonably well in transduction, but with a host range, and recipient range, very much more narrow than PLT22. SW-666 (PB type Jersey, Gal-, non-motile, b:--) served very well as a recipient, for either the Gal or the motility marker. Your own experience on the host range of this phage would, therefore, be of more than ordinary interest. In addition to SW-666, S. dublin (non-motile) [SW-553 in Stocker's paper] was also trans[in]duced. (I haven't fought my way out of this linguistic problem yet). Cherry sent me a kunzendorf strain he says is readily lysed by Beccles and Taunton, but I have no data on this yet.

I can't recall any use I'd possibly have for anaerogenic typhimurium. Perhaps this concerns the question of the relationship of *S. gallinarum* (IX XII gm) to *S. enteritidis*, and I would be curious to know whether anaerogenic enteritidis (i.e. motile gallinarum!) was at all prevalent.

Would you have any comment on the following problem, or do you think anyone else at Colindale might have some? In some studies with abortus-equi and "monophasic" paratyphi B's, I have frequently been frustrated by finding that I could get one or two cycles of phase variation, and no further. Has anyone ever gone to the trouble (to your knowledge) of ~~carrying out a~~ following large number of cycles of phase variation with appropriate precautions such as single colony isolations at each stage? The frustrating results above suggest the possibility, or rather the question of the possibility, of a sort of exhaustion of variability after too many cycles. I have no idea what this would mean in any genetic terms, but would be curious if you or Felix or Joan Taylor have ever heard of any relevant experiments.

You may be interested that the Grand Central Radio shop, where you bought the tape recorder, lived up to the warranty and replaced the defective 6E5 tube without fuss. The machine is very useful and entertaining. The tapes are interchangeable with the Webcor, but the gain on this machine is rather limited, although its mechanical features are much better than on the Pentron. The latter is also more portable.

Yours sincerely,


Joshua Lederberg

P.S. Do you have much information on the reversal of specificity of the paratyphiB phages when these are grown back on type 1?

JL