January 6, 1956

Dr. P. R. Edwards
Box 185
Chamblee, Georgia

Dear Phil:

Thank you for your note about the Ørskov's. I have had some other equally good remarks about them, and am now looking forwarding to their coming, if it can be managed at all.

I'm sorry I forgot to comment on transduction of variability, as I had intended. Your design would be a good test, and it might well happen that there are factors other than the H_1 and H_2 loci which control the ease of variability. Some of these factors might work directly on the intrinsic mechanism of phase variation; others might influence the readiness with which it is detected (e.g., as in the kunzendorf 1,5 → c, where the common antigenic factor between the two phases [somatic antigen? remember?] hinders the demonstration of the variation with some serums). The only reason I've shied away from this angle myself is the lack of a precise quantitative measure of the rate of phase variation, but this could be worked out. I can't say I'm convinced I've ever seen a real change in rate after transduction, though some of the —x abortus-equi results might fit this notion. I don't have any readily variable b:1,2 types from N25 or N97, unfortunately!

The Lancaster cultures sound interesting. Would it be feasible for you to test the various nonmotiles to see if they are the same Fla- mutant? I would be very pleased to have some of the Fla- when you are done with them.

As to linked transduction, the whole point is that Fla_1 and H_1 must be related to one another in a special way because while most double-transductions occur at a very low rate (so low they are generally not detected at all) these two factors tend to go with one another with an unusual frequency. For that reason, we believe they are located very close to one another on the bacterial chromosome, so closely that even a small fragment has a good chance to include bothe genes together. There is a detailed discussion of this in Stocker, Zinder and Lederberg, 1953, Jour. Gen. Microbiol., 9: at page 426-431. I am sure you must have a reprint; if not, yell!

I had forgotten about the 4,12 deh:enz_{15} strains, and did not know about the r,i's. We should try to tie them into our study of N25, and I would appreciate having them, if you have them on hand. We are saving the phages you sent, and when I pick up an extra hand or two, will try to find time to add them to our program. Perhaps one of the Orskov's might have some interest in them,

Sincerely,

Joshua Lederberg