October 20, 1952

Dear Norton:

Last rec'd: yours of the 14th etc.

The SW- cultures you asked for are in the mail, as follows:

a) lyophil tubes, when there remains at least one more in apparently good condition. Some of these had incipient cracks, but were intact and should doubtless be opened promptly.
b) vial-slants, when this subculture required the opening of the ultimate tube.
c) the following for your interest:

SW-663: SW-543 + FA(SW-553: dublin-0) gp transduction
SW-664 : " " san diego eh or enx transduction
SW-665 : Xyl- from SW-541. The Xyl reactions of the transductions to + from FA(SW-541) or FA(LT-2) are very baffling, possibly an anomalous temperature response. The strain is self-lytic (occ. plaques) on EMB-xylose.

SW-175 and SW-248 are recorded as having not been saved. SW-176 (not -177 as previously recorded) was lost.

Other notes: I am putting Edwards' cultures down as SW-701 -900, 701 corresponding to his #1 (Ky. Ag. Exp. Bull. 40), and so on. SW-703 may be self-lytic. On the other hand, a non-b,1,2 component is present, picked up in routine checks of antiserum-agar, and later in direct streaking and picking. I don't know yet whether this is a recent contaminant or an ancient mutant.

It is reassuring to hear that phage may have some DNA, after all!

Concerning differences between galduction and Salmonella transduction, I had thought we had had all that out, but perhaps this was with Bruce. Of course, any of a number of things are possible, and have to be tried. We certainly should not generalize from the possibly unique behavior of individual phages, individually grown. This is essentially the underlying principle of my further interest here, and I think you are very wise, on the other hand, to consolidate our sketchy information by an intensive study of the system at hand.

As to facts and hypotheses re SW-543: hypothesis first! The results on non-linkage in the progeny test pointed to just the picture you queried in your letter, that SW-543 carries a specific b allele which is dependent on B+ (or to say the same thing, suppressed by B-). In this connection it was interesting to test the spontaneous motile reversions to b, whether they were distinct alleles A^{b1}, or B+. The facts are rather more confusing. My first scorings of the transductions of SW-543 were based on the selective action of i-serum, and all the transductees were immobilised, while various controls showed that the serum passed b phases. In subsequent tests, however, b phases have appeared fairly frequently (about 1/3) in the progeny tests, so that this evidence does, after all, support the linkage hypothesis. This would make the story rather parallel

to the auto- and allo-genic transformations in pneumococcus (horrible terminology!-- In desperation, I would prefer to see "hologenic" and"oligogenic", which is almost as bad.) I can't say yet whether the discrepancies between immobilization and slide-agglutination are trivial or important. They may reflect an unsuspected complexity of the b antigen, and require a thorough antigenic analysis. There has been some hint of this in transductions from abony (b-enx) to para B. The b transinductions are of two kinds, one typical b. The other gives buds and swarms in b serum agar, but the outcome still agglutinates, less strongly, in b reagents.

The transductions of several othervtypes to SW-543 have been checked, in each case the FA type is recovered as well as b. Some of them have been sent. They include: gm, gp, eh(enx?), r, and possibly c, as well as, of course, i and 1.2. Some others are in the works, especially second-phases.

The SW-543 story has now to be gone over very carefully, with all of the derived stocks presently at hand, and thoroughly checked. Until this is done, I would not want to be committed to any definite conclusions. I intend to do a progeny test with one of the other transductions, probably r, as well as i.

The Berry-McCarthy-Plough story sounds as phony as the alternative-requirement mutants. [illegible] I hope Bernie can be persuaded to check the sexauxotroph recombinants.

Transduction certainly does not prove a chromosomal prophage, but it does speak for a hitherto unexpectedly close relationship of the phage with the genetic material of the host cell.

Good luck on your election campaign.

Sincerely,

Joshua Lederberg