April 2, 1952

Dear Tom:

I have the PHS documents, and will take care of same right away. I hope you weren't too disappointed about Merck-- I had never thought too much about that prospect. Of course you should apply for the PHS fellowship, but you will have to evaluate the risks yourself. I do hope you make; we should have a lively group here next year, with a couple of other postdocs. (from England-- on their own steam). Let me know before you do anything too drastic out of discouragement.

As to your paper, I wonder if your two masters are not already enough. Most of my comments about the paper still hold, and I don't see that it would do any good to amplify them. If you want to include kinetic data, you ought to revise the title. I don't think this type of reemphasis is going to mean very much re Delbruck's misfortunes, and if they can reproduce your results under your conditions, why where's the quarrel?

Why don't you refer to Clifton's paper? I haven't received your cultures just yet, but expect them. As to the genetic data, I've only gone over the first cross W-1177 x B6. There looks to be a fairly straightforward linkage of "s" to Xyl; less directly to Mal and S (see Newcome and Nyholm 1950: Am. Nat.) If the other data agree, why don't you simply report this table and the results of pairwise contingency tests. I am a little sour about mapping in the Mal-Xyl neighborhood until we can get the segmental elimination cleared up, in relation to F.

Your analysis of Hayes is almost exactly what I wrote to him myself. He has a second paper in press (Nature) on the Texas effect [which works on the 58-161, not W-677 -- F?] where he gently drops the idea of the phage gamete. The rest of it is just quibbling, (I don't mean so much Hayes' speculations, which he is entitled too, and whose force is limited by his own background, but the Parisian acceptance or distortion by people who ought to know better. Who is everybody in Paris: Ravin, Harriett and who else?) Esther's statements about nonlysogenic crosses are not explicitly directed at this question: MGB-1; Genetics 36:560. However, I thought I emphasized them at CSH last summer, and I discussed this question in detail with Monod in correspondence some time ago. Your light effect sounds most exciting. I have not so far been able to demonstrate any hormonal interactions (barring the genetic effect of F+). Also rutin, which is a potent inhibitor of sexuality in Chlamydomonas and Forsythia (Kuhn-Moewus) has no effect whatever in coli. This does not exclude a biochemical approach. The F story does look like it's turning into a scheme of relative sexuality: F+ stocks are interfertile depending on the difference in their "potency".(Maybe.)

Sincerely,

Joshua Lederberg