June 27, 1953

Dr. P. R. Edwards
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Dear Phil:

I am returning the "edited" ms. It has been marked to correspond with a corrected version just sent to the Journal, with my request that the changes be put in before the paper is sent to the printer, if possible, (as it should be). You will note that virtually all of your editorial corrections have been included, and then some.

There is one change of substance: a review of the transductions to —:1,2 types cleared up a misrepresentation in the tables. As now stated, the diphasic derivatives had come from SW-960 (the Berlin culture), while SW-959 (Hines) gave monophasic substitutions. I don't know that this means a great deal, but these are the facts. The yields of transductions from either of the cultures were rather low, and the data may not be extensive enough to warrant any generalization. The main point, of course, is that at least one of them did give diphasics.

I sent Moran's cultures (1966 and M66) to be verified— I thought she ought to be apprised of it. Her note was quite explicit that these were supposed to be abortus-equi, and the stock numbers are equally clear. I am sorry about M66: I put it in the shipment as an afterthought, and thought the label would be self-explanatory. It is another one, like 1966, from Moran. Her shipment also included "1967", which likes very much like "1966". I have no idea of the history of these strains.

For further work, I am going to use only #26, and "Peru 818" which you sent not long ago. In recent experiments, they have both been rather obstinate.

I am trying to repeat the experiments in which SW-1003 was generated, and so far very little has happened. I do not doubt that V is present in SW-1003, but do not like to generalize from a single case, especially on the correlation of V with diphasic behavior. At the instant, SW-1003 is an isolated curiosity, and there is no telling whether it can be consistently obtained. If you want to write it up as a <u>possible</u> example of transduction of somatic antigen, I'll go along with you, but I would frankly prefer to put it off temporarily.

I assume you are still working with 1042A1, etc. (experiments with S. abortus-equi Meyer). In my hands, the untreated culture went (slowly): a:enx:-; but there was only a single test; phage-treated cultures went as far as :a:enx:a, but again very sluggishly indeed. Do any of these have V ?

I think I understand your views on the relationship of IV XII and IV V XII--namely that the latter should not be regarded as IV XII + V, but as a distinct "substance", albeit with numerous cross-reactions. I would have thought that the possible "Form Variation" of V (e.g. in Iseki's paper) would have put this on much the same basis ~~of~~ as, e.g., the XII fractions XII_2-XII_3, but even this would be consistent with your point.

Could I be of any help to you in going over the Iseki work? It would be of the greatest interest to me to determine whether a phage per se can alter the serotype, aside from transduction. If you haven't the time, I could at least try to demonstrate the phage Iseki talks about,and send at most a few lysogenized cultures to check the somatic antigen. I would have to have the inter-transformed cultures from your previous experiments for the clearest results. If these are ~~p~~ not available, pairs of anatum:newington from single animals might do as well, though less certainly related. If this is too much of a load, I think that either Spicer or Anderson could be interested in it.

Yours sincerely,

Joshua Lederberg