

January 17, 1957

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

Dear Phil:

I hoped you would realize we had a standing invitation for you, and the red carpet ready to roll out. We are simply delighted that you can finally get around the silly obstacles and come to visit us. We hope you can manage to take two or three days for us, as there are two or three independent programs here that could benefit from your counsel. The very least time that you could manage to compress all that in would be a day and a half.

We can undertake to pay your excess transportation costs by air, Detroit to Madison and return, or if you are not being subsidized for the SAB already, all the way from Atlanta and return. We are also in a position to offer a reasonable 'consultation fee' in addition, but I understand that you might not be permitted to accept such. If that situation has altered also, please do let me know, either now, or during your visit.

The major costs of this are being borne through a project supervised by Prof. J. B. Wilson at Bacteriology. We would be delighted if you would use the occasion to give a seminar lecture on Salmonella, or the work of your laboratory, or any other related topic, so that our students and colleagues can see something of you. However, this is not obligatory on you, and we will be more than repaid, and very well pleased, by the chance at some leisurely discussion of current problems. As you may imagine, a considerable number of Madisonians will be going to Detroit too (probably not the Lederbergs—we're travelling too much this year, having been in California over New Years, and going to England and back in March, then to Melbourne June through September). I assume you will want to embark for the SAB on Saturday night or Sunday morning (Apr 27-28)—NWAirlines has a number of connections. If you would be interested to talk, would you let me know if Fri afternoon would be all right, and your title? I would like, if possible, to fix all the arrangements before we leave for England (about March 1); however, Professor Wilson will be here for any last-minute adjustments. So just let us know your schedule.

Your new bug sounds very much like the *S. salinatis* story. We never have gotten round to a transductional analysis of that, and though we might in due course, why don't you go after it. *Salinatis* might be better, because the group B-D phage (PLT22) works so nicely, but I don't remember whether the stock *salinatis* is susceptible to it (and therefore usable as a donor in transduction).

\*x On the other hand your new bug has the advantage of no cross-rx between the a, enx phases. I don't remember whether you have a group F phage usable in transduction or not.

My hunch is that these cases represent a duplication of the  $H_1$  locus, basically similar to CDC-137 and its derivatives. In addition, the normal mechanism of phase-alternation ~~ix~~ has broken down, perhaps because of the duplication, and the d antigen therefore shows up in both phases. Then *S. salinatis* would have the genotype:

$$H_1^d H_1^{eh} H_2^{enx}$$

At least some tests of this hypothesis are possible by transduction. For example (with, say, typhimurium as the other parent) we might expect the following:

Donor	Recipient	Selective serum	Expected progeny
1. i:l.2 -x	d,eh:d,enx	e...	d,i: d,enx a. d,eh: d,l.2 b. i,eh: i,l.2 c.
2. d,eh: d,enx -x	i:l.2	i;l.2	d:l.2 or d:d,l.2 d. eh:l.2 e. d,eh:d,l.2 f. i:enx g. not i:d,enx or d,i:d,enx h.

Now, c. is fairly critical, for it shows that the 'd' of each phase is simultaneously replaced by  $H_1^i$ . But it might come out i,eh:l.2 ~~ix~~ instead.

d. shows that the  $H_1^d$  of *salinatis* is homologous with  $H_1^i$  of typhimurium. It is hard to guess whether it would be expressed in both phases of the progeny or not.

f. could occur if the  $H_1^d$  were closely linked to its duplicate,  $H_1^{eh}$ .

g. shows that the enx is genetically independent of the d.

I would not expect the h. classes, as they would mean that the  $H_1^d$  was linked with  $H_2^{enx}$ . This could be true, but has no precedent. At any rate, you should not get both the h. and the f. result, and neither is most probable. Do you think you will get into this? We can discuss it better here.  
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I have all the data collected for that ' $H_1$  duplication' paper, but just haven't had time to write it up. If possible, I hope to have a draft that you can look at here.

Phil-- can you help us again with some serological typing? Iano has picked up some cultures in the course of studies with SW-1062 (the TM2 monophasic) that have been rather perplexing. My hunch is they're carrying an 'artificial phase' mutant

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of 1.2, — "8 or 9"? but they give a dubious reaction with anti- b, and this is causing us some trouble. We are sending, under separate cover, SW-1161 and SW-1162 and would appreciate your remarks on them. Now SW-1161 has a nonmotile phase, and might revert to give a motile-1 for that phase, corresponding to SW-1162. (See attached shipping ticket.) If you're too busy, please chuck them and let us know.

Iino was also interested in Joan Taylor's culture, Col 529-55, r,i: lw, that you had sent us last year. But he wasn't able to find anything but r:lw in it. Have you any suggestion about that? Did you study this culture at all yourself? Has she written anything up on it?

The Ørskovs have been a pleasure to have here; I had a chance to meet the elder Ørskov also earlier this week. Frits and Ida will be writing you soon about their hopeful plans to travel a little, and I know they want to visit you and Bill Ewing.

Sincerely:

Joshua Lederberg

Enc.: S/T SW-1161; 1162