Stent

April 24, 1958

Dear Dave:

It is good to hear from you. I don't know what to advise on general procedure in looking for assistants except to let your friends know and to advertise at places like CSH. In fact, I just had the enclosed letter from a Mr. Bullas who might interest you. His main interest has been in Salmonella phages and serology but he would like to broaden his training. He seemed sound enough, not world-shaking, but eager to work. He is looking for the possibility of working for a Ph.D., or for a more temporary assistantship. I don't think you are likely to get much more useful information than this from further inquiries; if you really don't have any more obvious candidate, I don't see how you'd lose anything, by taking him on. He was a very pleasant chap and at least moderately intelligent, though not too familiar with contemporary work.

Luca Cavalli is here for a couple of months and we have been going over the various experiments on timed interruption. I only just realized how one could get such precise information on timing by extrapolating to the x axis, and the kinetics of the model is just becoming clear to us. We are trying to tidy up some of the second order quantitative effects, on the one hand, and to find out whether the process that is interrupted is chromosome transfer or progressive pairing on the other. (One can still imagine that the nuclear transfer occurs in a few minutes, but that pairing progresses from one point down the chromosome; markers in unpaired segments are lost by their linkage to the terminal deletion. This would also necessitate that zygotic induction of prolambda also needs pairing.) I really don't know what to think of these alternatives, and am having some trouble designing really critical experiments to choose between them. Among other things we are looking for ways of separating the pairs <u>without</u> interrupting the pairing, so far without conspicuous success (as you and Francois would doubtless predict). Protoplasts are mating beautifully, and protoplast ♂ x rod ♀ give a very nice method of interruption by lysis in water. But of course, moderately vigorous spreading on agar does very well too! We have succeeding in diluting mixtures in broth gently enough not to interrupt.

There is one method that I hoped you might be the one to tackle: to go back to your old experiments on P_{32} transfer. But the way to handle it is by stars, of course. I would mate protplast ♂♂ * x rod ♀♀ and lyse at various times, plating for stars. If there is gradual transfer of DNA this should show up very well in the distribution of sizes of stars. You would probably have some background of unlysed Hfr* ($\sim 10^{-3}$ or 10^{-4}) which would show as full sized stars throughout, and of course there would be small stuff that wouldn't count at all. How about it? The P_{32} experiments so far measure the amount incorporated (by its suicidal effect) not the amount transferred.,

We had a symposium here ~~last week~~ a couple of weeks ago, and we were down to Gatlinburg just before then. We still haven't recovered, and the pressure of time has been unusually harassing this season. Not much else now here: we made a bid to get Al Hershey for Genetics and Chemistry, but it didn't pan out.

Wait till you get your 4' blizzard in mid-May!

All the best to Linda and Co.-- when might you be travelling through this way? We'll be here from now on till late July, when we're scheduled for the Royaumont-Stockholm-Montreal cycle.

Yours,

P.S. Another personnel possibility of a different sort just occurred to me; she explains herself in the second enclosed thermofax. I have already mentioned her to Larry Morse, but don't know whether he has responded in any way. Her stuff on conjugation in Spirillums looks interesting, and they are fascinating bugs in any case.

enc: Bullas
Williams
Luthra