Cold Spring Harbor
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Dear Josh,

I'm sorry I've been in eclipse for so long. Thanks for your comments on the sequential transfer manuscript. As you have seen, they came too late to be considered, but fortunately some of your corrections had already been caught. I think that most of the remaining disagreement is polemic. I hope that the paper is not misunderstood. For example, I went into some detail on the Het story with the intention of making clear that there was evidence for post-zygotic elimination, and that in fact it might also occur, in addition to prezygotic elimination, in non-Het crosses. Also I intended that the expression "breakage" subsume such a thing as a point of stress. Thus I used the word "discontinuity" in the summary. Having agreed that these points of "breakage" or stress were variable, the question of whether they are breakage or stress points was considered. The end conclusion [illegible] is ofcourse that they are breakage points (since this is simpler -or so my argument goes). As to the role of spreading in separating pairs, I don't see that this is called for by the data. If spreading does do this, however, my argument would be that thermal agitation alone can also probably do the same thing. I'm not clear myself on why there is a discrepancy between the kinetics of pairing as det'd by JW and ourselves. My attempts at communication with Jacob have not been too successful. I snet him a summary of results about a year ago; he replied noncomittally but scooped us a few months later on the reverse sequence of transfer.

As you may know, I've been working on the linkage of tryptophan mutations in coli. Nine B/r mutants have been analyzed; all are clustered closely and seem to be in the proper functional order. There is some messiness however, which I have been struggling with and hope to clear up soon. B/1 tyrtophaneless are apparently deletions for the entire tryptophane synthesizing region. Yanofsky and Lennox have obtained almost identiacl results, but we are in amicable communication. One of my difficulties is in obtaining high titer lysates of the mutant P1 which I use for intra-B/r transductions.

With regard to the F* to F- conversion, I have a result to report which is either an amazing coincidence or a significant finding. The F- strain which I obtained when I first came to CSH (by swarming) from 58-161) has been found to have an additional requirement for proline. I have been stewing for a chance to look at this phenom. further but haven't had a chance. Maybe it would be wise to check all of the independent F- derived by swarming which you have, to see if this or any other requirement has been added. I can think of several wild but interesting possibilities. (The F- strain I mention is called CS2; it is truly F- and the additional prol- is appar. linked to TL, prob. to right of Lac.)

I am definitely in the market for something next year. I'm on an NSF grant which extends till Jan. 58, but this is an awkward termination point. I'd be grateful to hear about anything interesting. I wrote to Georgia for more information; I also had a letter from Florida State about a somewhat similar position.

I was disillusioned about the reprint-providing-capacity of Detrick, and consec. have almost run out of "Binary Mut" reprints. Were any of those you requested to be sent out? If so I have enclosed a list of people to whom I have given reprints in to prevent duplication.

(over)

Some time age, I asked you to check with Bill Stone about those cross-rx. experiments. Both he and Wilmer Miller had indicated that they were intersted in doing something on the subject to round them out for public. I was interested in getting straightend out who wanted to do what. Did you ever speak to Bill or should I write him at long last?

Best regards to Esther,