LONG ISLAND BIOLOGICAL ASSOCIATION
COLD SPRING HARBOR, NEW YORK

THE BIOLOGICAL LABORATORY

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Dear Josh,

Alan Garen has suggested that he and I do a tracer experiment to demonstrate DNA exchange between mating coli. This much seems feasible, even with Nfr strains. Possibly other things could be shown, particularly unilaterality. In some ways, K-12 x B crosses are advantageous. In other ways, ofcourse, Hfr crosses are prohibitive favorites. I don't know your present position on the Hfr strains. If you could provide us with one, we could, if desired, let you know as precisely as possible just what we intend to do with it. If, on the other hand, your feelings on the subject are as before, they are appreciated, and no explanation is necessary. Perhaps the most direct approach to the problem would involve radioautographs of individual exconjugants, but we have no illusions about attempting this.

As you know, Tom Nelson was over here last fall and gave a good talk. I talked all too briefly with Cavalli when he was through. I was apparently unable to convey to him what I think to be the value of the K-12 x B data I have, and he was unable to convey to me any very coherent hypothesis of recombinational events. He speaks of the second regular point of breakage between S-mal and xyl-mtl, yet admits to the rather strong linkage between S and xyl in certain crosses. At one stage, to explain this, he invoked the premise that it is not always the F+ chromosome which breaks, but rather the F- chromosome (at the same spot). This seems to me to lead to a rather chaotic picture. Maybe not, or maybe the picture is chaotic.

I have spent most of my time lately on the mutable strain. Unfortunately, some freakish results led me far astray and wasted a good deal of time. For awhile, it seemed very likely that rate of exchange of the mutability factor far exceeded the rate of exchange of other genetic factors (barring F). It now seems fairly clear that this is not the case, and in fact the mutability locus has been located with a fair degree of accuracy between V_1 and L, considerably closer to L. I intend to locate it with respect to Az and Val. I can find no more precise information with regard to Az than that it is about midway between V_1 and L; has anything more exact on this been published? You and Cavalli say that val is linked to M, whereas Rowley et al say it is closely linked to TL. Have you any comment on this?

Regards to Esther and the rest of the lab.

Dave Shaw

P.S. Experiments with Engelsberg seems to show that the action of the mutability factor is depressed under anaerobic conditions. This is where the assay locus is S; where V_1 is used, there is suspiciously little evidence of such an effect.