September 16, 1954

Dear Bill:

I have to ask your apologies for both the belatedness and immateriality of this reply to yours of the 4th August. I was fortunately able to take a sort of holiday for most of the summer, and while your letter was eventually forwarded to me, this has been my first opportunity. Unfortunately, I still do not have the time for as deliberate an answer as I ought to give, but you will understand it if I tell you that I have to prepare my lectures for this year's course, handle students, complete the moving into a new house (yes again, but now for the first time our own property) and dispose of the usual odds and ends of getting back to work. We have also the pleasant occupation of entertaining the Cavalli's, who arrived just a few days ago.

If I answer your detailed questions at all, it is with some risk of error, as they concern work in which I am presently concerned, and on which I am not in a position to quote final conclusions. I think I can say that we have isolated Hfr recombinants from single-celled zygotes, but that reciproeal redombinants are, at least usually, lacking. These findings are subject to revision with more extensive study, and I should not wish them to be quoted yet. So far, they are entirely consistent with the viewpoint embodied in the paper by Nelson and myself that appeared in the PNAS earlier this year.

Jacob was kind emough to send a copy of his ms. His observations are extremely interesting: I would interpret them as indicating that the "cytoplasmic state" of the sensitive parent results in the induction of a proportion of gametically introduced lambda-prophages. But like yourself, I do not quite see what this has to do with elimination or segregation ratios. In our experience these are not appreciably influenced by the substitution of Lp^s for Lp^+ in either or both parents, though the quantitative yield of viable zygotes may very well be affected. If, as we believe, all the gametes are complete, the "erotic induction" should have no effect on segregation if a random sample of the zygotes are induced, and there is so far no evidence to the contrary.

In my earlier letters, I had asked you for your results on the nature of F, because I had the impression from our conversations in Madison a year ago that this was to be the object of your work at Caltech. I was sorry to learn that otherwise, but am gratified to learn you are going back tomit. Unfortunately, like yourself, I am somewhat at a loss to suggest just what to do, The only approach we visualize at the present is a closer study of diverse strains, whose compatibility (and I hope elimination) systems may differ slightly from that of K-12.

In an earlier letter, you mentioned some very interesting results with T3, which promptly interrupted fertility of Hfr. Without your explicit approval, I did not feel that I could quote them publicly, despite their important bearing on the nature of the gamete. Have you done more with this? Do you intend to publish it soon? In 1946-47, I had had a similar result in a few experiments with T1, but did not pursue this line as there was not then any conflict of interpretations for which this would have been relevant. Since I am not sure now of the F status of the cultures used for that particular experiment, it may be of no particular force how either, and I mention it only as the basis for being in no way surprised at the outcome of your experiment. In due course, I am planning to try to extend the design of your original S^t experiment to some other undoubted sexual systems (yeast or Chlamydomonas). I shall be astonished if toxins are not found here also that will separate vegetative from sexual vitality: their use might be in diffenentiating the cytoplasmic contributions of the two patents.

Yours sincerely,