WASHINGTON UNIVERSITY
SCHOOL OF MEDICINE
SAINT LOUIS

DEPARTMENT OF MICROBIOLOGY
EUCLID AVENUE AND KINGSHIGHWAY

Delbrück

Dear Lu:

The proofs arrived last week and have been ~~not~~ returned. The reprint order is enclosed.

I have been able to get a little work done since we stopped teaching (which turned out to be a much greater [i.e. more tiring] chore than anticipated). Currently, I am trying to finish up the UV effect on recombination. I got some closely linked r's (T4) from Benzer and ~~for~~ found that starting at a control recombination value of 10^{-3} the value varied linearly with the UV dose as far as I went (~4 fold increase, $P/P_0 \sim 10^{-5}$). This is like T2. Using this pair of r's, the UV increases the frequency (also the absolute number) of cells which liberate any recombinants at all. I take this as good evidence that UV does not

merely cause a selection for recombinants, produced at the ~~no~~ control rate, but rather increases the number of recombinations. I will talk these things over with Seymour when he visits this week. In any event, all I will do along these lines is a few more crosses of this type, and some crosses to see the effect of UV on the number of rounds of mating. I will write a short report of the findings and stop, since I don't see a further development of the work.

Much of these results and those on cross-reactivation would be easier to think about if we had information on the multiplication of the 'dead' phages per se. I am setting up to see if cells singly infected with UV T2 and P^{32} killed T2 can make DNA with HMC. ~~in~~ ~~order to~~ we will look for the small amounts of HMC expected by infecting in a medium containing C^{14} labeled glucose or acetate and looking for C^{14} in the HMC spot on chromatograms. Probably this will turn out to be mainly a biochemical exercise for me (eg. making C^{14} glucose), but that is all right, too.

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A further example of my diddlings is my attempt last week to get naked phage DNA into cells. ~~Since things~~ I think it will work with a little trick to be tried tomorrow. I will let you know if anything happens and only mention it here because at (e)last someone has tried it. The idea is to try and find out something about the physical nature of the three linkage groups.

I will try to come to Urbana the third week in April on my way East and will look forward to seeing you then. Please remember me to Zella and to everyone in the lab. We were all sorry to hear of Sol's accident and glad to learn he was back so soon.

I think I will describe the phage DNA experiment in the hope that you will have some suggestions to make. The DNA is prepared by osmotic shock from T2 h <u>Pf</u> (Pf = profloxine resistant). And is carefully cleaned by centrifugation

and anti phage serum so that the effect of phage are absent and the background of live phages is absolute zero. The preparation I have has 10^{11}/ml phage equivalents of DNA and almost resembles egg white in physical properties. I expose B to it at 37° after infecting with T2 r13 at 0° and high multiplicity, to maximize the usefulness of the leakiness ~~the~~ of the cells. After serum treatment I plate the cells on S(λ). r13 infected cells do not plate but if the DNA got in (it is r^+) they might. The check would be in testing ~~any~~ plaques for the other markers in the multiply ~~marked~~ marked DNA. The experiment is spoiled by the high background of mutants in all my T2r stocks which can plate on S(λ). UV'ing them does not help much because of MR. I am X-raying tomorrow. My T4 r stocks have the necessary low background and I will also try these with the T2 DNA. A success in this experiment would be pretty and I would appreciate anything you have to offer on the idea.

With best regards —

Bob