

YALE UNIVERSITY  
OSBORN BOTANICAL LABORATORY  
NEW HAVEN, CONNECTICUT

July 12, 1947.

Dear Evelyn:

Your more extensive paper, in Genetics, finally appeared, I was glad to see. If you can spare it, I would appreciate a reprint.

There is only one point in your published work which I can bring myself to dispute about, and even this is more in the nature of heckling than a serious criticism. On pp. 233-234 you discuss the question of the induction of the "/r" mutation by ultra-violet. The application of a dose of 3800 ergs/mm<sup>2</sup> was reported to increase the frequency of "/r" from .001-.005 % to 3.3 - 4.9 %, measured in the incubated population. In view of your very excellent selection experiments, we may take it that differential multiplication plays no large role. The major site of action of selection would be during <sup>ir</sup> the radiation, by selective killing. In order to evaluate this factor which as you say, certainly "exaggerates" the values, one looks for a comparison of the survivals of B and B/r using this dose. Figure 1 however extends to doses of 1800 ergs/mm.<sup>2</sup> only. At this dosage, pS = ~~xxxxx~~ ca. 3.5. From the induction experiment, pS (d=3800; coli B) = -log 30/2x10<sup>8</sup> = 6.8. To account for an increase of a thousandfold, pS (B/r) would have to be 6.8 - 3 = 3.8. Since pS (d= 1800) seems already to be 3.5, such a contingency seems quite unlikely, but I would have liked to see more specific data. To restate the argument, there were let us say .005% x 2x10<sup>8</sup> mutants or 10<sup>4</sup> in your original sample. After irradiation there were 4.9% x 24 or about 1.2. For these mutants to have originated as a sample of the original 10<sup>4</sup>, the killing of B/r with this dose cannot exceed ~~xxxxxxx~~ .012%. At d= 1800 there is already .005%. Could there be so sharp a break in the killing curve between 1800 and 3800 ergs/mm<sup>2</sup> ?

This question may arise in some other people's minds also.

To my mind, your "population" experiments were the best part of the work. Have you tried at all to isolate inhibitory materials from mixed cultures held ~~for~~ for varying lengths of time? After all, there are "natural" radiations, which should have some slight selective action, accumulating over long periods. The metabolic basis of the adverse selection which you observed would be, I think, very important for the detailed interpretation of the mechanism of radiation resistance.

I am writing a review of bacterial genetics, partly for my thesis, and partly for publication. If you should happen to have a carbon copy of your manuscript, or perhaps positive prints of the lantern slides which you showed at the Symposium, I should be most appreciative to receive them on either loan or gift. I am particularly interested in what you might have to say, either for or off the record regarding the zero- and end-point manifestation of your induced mutants.

Best regards,

Yours sincerely

A handwritten signature in cursive script, appearing to read "Josh".