Alfred.

Jan 2

Dear Josh,

I am sending you a copy of our ms. on some of the serial transfer experiments I mentioned to you last spring. We now suspect that periodic selection may be due to a series of types producing different colicines (cf. Fredericq), and are starting exps. accordingly.

I have just seen the Novick + Szilard opus, but its too late to refer to it as the ms. was sent off several days ago (to PNAS). Their finding of types more "fit" is obviously the same thing as we found.

Recent exps. with UV on heterokaryotic conidia show that they are <u>not</u> essentially different from your K12 case with respect to production of isogenicity. While the numbers of survivors on minimal and supplemented media are almost the same (as I reported last spring), the colonies on supplemented turn out to be mostly homokaryons, as in your case. This means that an injured nucleus will

nearly always recover in the presence of an uninjured nucleus, but where such recovery is unnecessary for growth (as on supplemented medium) the uninjured nuclei have a head start and produce a lot of homokaryons. In your case the central spots of lac+ which appear in many of the older colonies make sense according to the above hypothesis.

Do you think Novick & Szilard really mean to imply that their data on time proportionality of mutation is inconsistent with the two hypotheses they mention, namely error in duplication and monomolecular reaction? If this is so I think their reasoning is a little sloppy. The first idea remains perfectly tenable if we assume that some critical period during which errors can be made is greatly prolonged in their experiments. Furthermore it would be consistent with the failure of mutants to accumulate in stationary cultures, a datum which they seem to have ignored. I don't see why they are worried about the value of A ~~for~~ mutation being much lower

than for "known monomolecular reactions in condensed systems". In fact, this strikes me as being a rather stupid comparison. The range of A values for such reactions is simply a reflection of the fact that reactions slow enough for A $\gtrsim 10^5$ sec. cannot ordinarily be detected (measured). Thus the value of A can by no means be considered a general characteristic of such reactions. Radioactive decay (probably the only true monomolecular reaction) is easy to screen for, like mutation, and consequently A much less than that for mutation is often found. Anyway, N. and S. are certainly not denying that mutation is first order, and this is probably all that Delbrück and Timofeeff meant.

If the above remarks seem overly snide, I will admit to being somewhat piqued at Szilard for not sending me his usual pre-publication circular.

In regard to your question about Peg, I think she is clear-headed and industrious. She has applied for a fellowship to go to Italy & work with Buzzati-Traverso, but

this may be impossible by then. It would be worth while to ask Delbrück his opinion of Peg in a few months. The Columbus performance was due to the problem she was working on.

Werner Maas reports that the selection of non-temperature-sensitive pantothenicless mutants in K12 consistently gives complete loss of crossability.

Ryan writes that he has hot stuff on lac⁻ mutagenesis with some lactose analogue, but gives no details.

The Buks were alive.

Best to you and Esther

Kim

P.S. The army is breathing down my neck (Tim IA), but Columbia may be able to keep me out temporarily. Would you please return the ms. when you are finished with it?