April 12, 1949.

Dr. R. Latarjet,
L'Institut du Radium,
26, rue d'Ulm,
Paris 5, France.

Dear Latarjet,

I was very pleased to hear from you. I can spare another copy of my article in Heredity, and am sending it along, together with some reprints for the benefit of Dr. Elias.

Lately, our work has taken three directions. I have been muddling through the problem of gene-enzyme relationships, parallel to what Monod is doing, and he knows all about this. Then, lately, we have found peculiar mutant stocks of E. coli K-12, from which heterozygous diploid progeny can be isolated from crosses (rather than the immediate segregation usually noted). These heterozygotes undergo segregation occasionally (about once in every ten fissions) and we have been studying the genetics of an aberration that may be associated with the persistence of the diplophase. Finally, it turns out that K-12 is lysogenic, carrying a phage that I have so far been able to detect only on a sensitive, non-infected mutant from K-12. The sensitives can be reinfected, and become lysogenic (about a quarter of the cells surviving exposure to the phage). I have been able to find a phage in sewage which is interfered with by the lysogenic phage. Thus resistance to the sewage phage can be conditioned either by gene mutations (V_6^r, as it turns out) or by the acquisition of the cytoplasmic "character", the lysogenic phage. Aside from this, the lysogenicity has no bearing on recombination. One of my students has been working to look for recombination in Salmonella, but although the results are very encouraging, lysogenicity is so common as to interfere very markedly in the experiments. I have been trying to disinfect lysogenic bacteria with radiation (see p. 155 in my review article), but so far with only sporadic success.

I wonder if you can suggest where the famous lysogenic coli of Lisbonne & Carrière might be obtained. Monod did not have it. If you could locate this strain, and a suitable susceptible indicator, I would be very grateful. Meanwhile, I wonder whether you might not have considered that mutations in the phage carried by lysogenic bacteria would not be especially pertinent to cancer? (Viz. Burnet & Lush). Potter, et. al,, here seem to think they have some fairly good evidence that the exhaustion of some cytoplasmic particle underlies the induction of liver cancer with azo dyes. My own feeling is that cancer is some sort of genetic change, but that the question is still open whether it is nuclear or not. Let us hear from you again,

Sincerely,