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*Phage culture*  
to

Dear Professor Lederberg,

Thank you very much for your letter. W. Arber, in my department, is indeed preparing a thesis on defective lysogenic strains and he now concentrates mainly on those obtained in transduction experiments. His main results are the following ones:

- 1) There is segregation, as you described, into stable Gal<sup>-</sup>"immunes", but for other strains also into Gal<sup>+</sup>Lp<sup>S</sup>, Gal<sup>-</sup>Lp<sup>S</sup>.
- 2) All such heterogenotes studied (6) are induced to lysis by UV.
- 3) Active phage is never produced after UV-induction (e.a. less than 10<sup>-8</sup>)
- 4) All strains studied recombine after induction with superinfecting phage (experiment made in using genetically marked λ's).
- 5) Two types of such "immunes" are found; one of them multiplies the defective λ-DNA after UV, the other does not. (By experiments of the Jacob's type: proportion of the recombinants in function of the time between induction and superinfection).
- 6) The morphological study of the lysates of 4 strains in the electron microscope showed neither tails nor empty heads. (In the opposite of 6 other "classical" defective strains already studied, about which we will soon send you a M.S.)
- 7) In genetic phage-recombination experiments, the stable immune obtained after segregation behaves exactly like the heterogenetic "parent".
- 8) When induced heterogenetic Gal<sup>+</sup> "immunes" are superinfected with active genetically marked λ, a part of the progeniture transduces Gal<sup>+</sup>. The proportion of transducers is about equivalent to that of recombinants. The Gal<sup>+</sup> obtained are mostly Gal<sup>+</sup>Lp<sup>F</sup> and heterogenetic.

If I have well understood your idea of a homogenetic Lp<sup>S</sup>, I think that these results do not fit. May be that your strains behave differently from ours; in fact, we do not use the same initial bacterial strains as you.

I would be very glad to continue exchange of results and I hope you do not mind us doing experiments in a similar direction.

I appreciated very much receiving your reprints. Having done research on globular forms of bacteria ourselves, we were very interested by your M.S. on protoplasts. (We prefer to use "globular form" as long as the deficiency of the membrane is not defined; we use "protoplast" for the Gram positives when a defined substance is dissolved by the lysozym.) For the last year, K. Gordon Lark (now at Microbiol. Dept., St Louis University, Medical School, St-Louis 4) worked here on the mechanism of the induction of globular forms by Penicillin. He used a strain of *Aerogenes faecalis* which had been once isolated by Bonifas in my laboratory and which has the peculiar feature

of being able to multiply in its globular form. We got globular forms also with K12, but did not succeed in having it multiplying in this form. For the "faecalis", Lark found a specific growth factor in tryptone medium; without this factor (in synthetic medium) ordinary globular forms are obtained which do not grow but are viable.

Many interesting problems had been considered but not yet undertaken: infection and lysogenization of globular forms by phage DNA without protein coat ; study of the eventually missing phage-receptors on the membrane; possibility to transform globular forms by DNA coming from genetically different bacteria.... For the moment being, we do not work on these particular problems.

I will be very glad to hear from you again.

Very sincerely yours



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