

June 10, 1955

Dr. Nils Aall Barricelli
Matematisk Institutt
Blindern, Oslo, Norway

Dear Dr. Barricelli:

Thank you for your reprint from Acta Biotheoretica.

I would comment that I find myself, on the whole, entirely sympathetic with your point of view, though my position is perhaps not so advanced as your own. At least, the concept of hereditary symbiosis should be considered as at least one of several, not necessarily mutually exclusive approaches to the nature of the organism and the origin of life.

For this purpose, however, I do not find it necessary to assume that the genes of any contemporaneous organism are so devoid of integrated structure as you seem to propose, and I doubt very much whether we have left today any certain examples of independent, isolated "single genes".

As to your interpretation of crossbreeding in bacteria, I would also submit a more eclectic approach. The evidence that recombination in *E. coli* is sexual cannot be so lightly dismissed— if by sexual you mean the fusion of entire nuclei (i.e. genomes). Lately, we have evidence that this is accomplished not by the fusion of entire cells (as in the algae) but rather by temporary conjugation (as in *Paramecium*). We perhaps arrive at a question of definitions— your genophore here becomes what I would call the gametic nucleus, but the point is that the latter is an intact genome, not a fragment as in the transduction system. The latter is, moreover, as you know mediated by sub-cellular agents (phage or DNA) in distinction to the K-12 ~~sexual~~ sexual system. But I do not see why your theory should exclude the possibility of a fully evolved sexual system, as well as the more fragmentary techniques of genetic recombination.

You will, I think be interested in a recent progress report from Hershey's laboratory (Carnegie Inst. Yrbk. 53: p.216) which records experiments to test the effect of the host bacterium on the quality of the phage progeny, as well as in references (in reprints (sent under separate cover) to lysogenic conversions. As concerns the interpretation of transductions in *Salmonella*, I think the phage-genes in this case must be considered as distinct from those of the host bacterium, since the transducing competence of any given crop of phage is determined entirely by the genotype of its most recent host. This distinction does not hold for the lysogenic conversions, which are more a propos your argument. The reconciliation that I make is that the phage nucleus is homologous with part of the bacterial nucleus. For the most part, the association between the two is adventitious (in *Salmonella* transduction) but not in the lysogenic conversions. The Gal-Lp system (see reprints) is intermediate.

May I inquire whether your interest in this subject leads you to

consider working experimentally with these materials?

I would be indebted to you for reprints of your other papers. I would also be interested in any clues as to how I might obtain access to the other publications (or perhaps microfilm of them) that you quoted in re "sybiogenesis". (Keller; Kozo-Poliansky). I have marked reference numbers in the reprint of my review (Cell Genetics and Hereditary Symbiosis) which you may find especially pertinent to the general discussions.

Yours sincerely,

Joshua Lederberg
Professor of Genetics