

THE RICE INSTITUTE

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DEPARTMENT OF BIOLOGY

Dr. Joshua Lederberg
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Dear Dr. Lederberg:

Thank you for your letter of November 7 and for the references. In your letter, you have stated very clearly a point which I have been trying to make, when you say that you do not believe there is strong evidence for the location of single genes in isolation anywhere in the cell (I should make the one exception of plastids). In particular, there is no convincing evidence for a "plasmagene" - either in the older work on higher organisms or in the more recent work on microorganisms.

Moreover, in my opinion, there is a genuine distinction between an invader (whether symbiont or parasite) and a genetic element of essentially intrinsic origin, for even in a species cross the genetic elements which combine are still essentially of the same genotype and their combination results in a single genotype virtually all the elements of which interact to produce a single organism. It is true that a symbiont or parasite can influence the phenotype of its host, just as any other environmental agent might, but the two organisms still maintain their own genetic identities in the sense that on the whole their own genes still form one interacting system, separate from the other. The distinction between infection and sexual reproduction is ordinarily very sharp, and this is the distinction we are dealing with at present.

However, none of the above reasoning would be valid if it could be shown that a part of a chromosome in one organism could be incorporated into a chromosome of some other very distantly related organism, or that in the case of virus and bacterial host, the genetic elements of one could become entangled with those of the other. As for Sigma, I see no reason why geneticists should be discouraged at finding out that it is a virus - and then calling it a virus - unless they began their investigations with the preconceived idea that it must be something else, and that the importance of their work is lessened when they find out it is a virus. The same thing would apply to a symbiont. I can readily understand why Someborn was so reluctant to accept the symbiont interpretation of kappa, for much of the interest in his work was based on the extraordinary theories he originally advanced of the nature of kappa, and when it became evident that kappa was a symbiont, he still preferred to call it a plasmagene, as he originally did, but now of course he was using the word "plasmagene" in a different sense from the original. I see nothing to be gained by a confusing terminology. Kappa can still be of interest, even though we do call it by its correct name in classifying it.

On the above matters, I believe you and I are not so far apart. But on the question of autocatalysts we do differ. Autocatalysts of the ordinary kind are not so unusual. But one which has the capacity to change and to reproduce itself in its changed form is unknown, either in the living or lifeless world, except for the gene. It is upon this property of the gene, along with its capacity to reproduce, that evolution is based.

Moreover, the fact that genes are the only elements in the cell that do not arise de novo, makes them the bodies upon which the identity of a species depends, as well as the life cycle of the species. It is true, as you state, that genes did arise at least once in the past from lifeless matter, but this was either as the result of a long evolutionary process or it was an event of extremely small probability of occurrence, so that we do not see it happening to-day, any more than we see living matter itself arising spontaneously from the lifeless. But autocatalysts of the ordinary kind do arise de novo to-day under gene control. This they do regularly and in the normal course of development of any organism.

Yours sincerely,

Edgar Altenburg

Edgar A. Altenburg.