

INDIANA UNIVERSITY

BLOOMINGTON, INDIANA

DEPARTMENT OF ZOOLOGY

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Dr. S. Spiegelman
University of Illinois
Department of Bacteriology
362 Noyes Laboratory of Chemistry
Urbana, Illinois

Dear Sol:

Have I misunderstood the statement in Lindegren and Lindegren, CSH Symp. XI, p. 118, paragraph 2, "With the help of Dr. Michael Douderoff we have carried out several additional experiments on the effect of continued exposure to substrate. These experiments all yielded negative results, for there was no significant increase in the number of asci containing four fermenting spores in the presence of substrate, over the number of similar asci obtained from heterozygotes developed in the absence of substrate." Conversations with Douderoff raised no doubts in my mind as to the meaning of this quotation. I need not cite for you Winge's papers which certainly cannot be ignored in this connection even if you criticize them.

As to the "repetition" not being carried out on the original strains, this point escaped my notice and I still find it difficult to determine. In your paper of 1945 in PNAS with the Lindegrens, you say "certain progenies of the *S. cerevisiae* by *S. carlsbergensis* pedigree were used" (p. 96). In the Lindegren's paper to which I referred above, they also say they used *S. cerevisiae* by *S. carlsbergensis*. Moreover, L. and L. say specifically that the original experiments were carried out with hybrid I of pedigree III. I cannot yet discover from the paper of L. and L. that different strains were used in the repetition.

From your letter, it is clear that you are displeased with my treatment of the status of this experiment. This I can well understand and I want you to know that I hesitated long before putting it in, and was very reluctant to do so. It was the only point which I discussed in full with the editors before finally deciding to do so. Frankly, the thing that decided us was the fact that the plasmagene theory based on your work had been so extensively set forth in the popular press that it would have been impossible to omit it in a popular discussion of cytoplasmic inheritance. Further, many specialists have wondered why there had been so little direct mention of the later developments--

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only the citation mentioned above. Of course, this became known to people directly interested, but I for one have wondered why you never discussed Doudoroff's failure to confirm in your papers. Or have you? I don't recall seeing it. It has been four years now since the L. and L. paper and that would seem to be ample time to have cleared up the matter if there had been more to say. If I am in error about the situation, please inform me. I will be glad to write a corrective letter to the editor for publication, or have you do so.

I am glad to hear about your new work. You know how pleased I would be to see the substrate stabilization story put on its feet. It was the main prop of cytoplasmic inheritance for a while, and nothing hurt the work and workers (including myself) on this subject so much as its later development. The tendency of many geneticists was to discredit all discussion of plasmagenes because of it. I ran into this time and again when discussing our own work at various places. If the story is after all correct it will be a great source of strength to all of us working in related fields. I look forward to an opportunity of seeing an account of your new work.

Believe me, Sol, I am, in spite of my handling of this matter, one of your ardent admirers and have always felt that whatever was lacking in the early work was probably attributable to the Lindegrens.

With best wishes, as always,

Tracy
Tracy

TMS:gg

P.S. - Feeling as you do, Sol, why didn't you write me when my article in Heredity came out? Or why didn't you talk that over with me in Columbus? There certainly was time to forestall any error - if I am in error - appearing again. Also, I understood that Luria had given you a copy of the mimeographed outline of my course on Elements of Microorganisms in which my views on your work are fully set forth. You surely must have known this before.