

Adelberg

UNIVERSITY OF CALIFORNIA

DEPARTMENT OF BACTERIOLOGY
BERKELEY 4, CALIFORNIA

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Joshua Lederberg
Genetics Building
Department of Medical Genetics
School of Medicine
University of Wisconsin
Madison 6, Wisconsin

Dear Josh:

Your letter arrived at an opportune time, as I was just about to write to you. I've been waiting all year for you to get back from Australia so that I could make a personal request for a checked-out, bonafide het culture. The one we received some years ago (W-478) was forwarded to me in Paris, where I tried many times to get diploids. I was crossing it to a glutamateless xylose- F⁻ (methionine and glutamate markers are very closely linked) and selecting for xylose⁺ prototrophs. Only rarely did I see any signs of segregation for Xyl⁺/Xyl⁻ and the presumed diploids did not behave according to prediction when streaked on various media. It was suggestive, however, that five isolates which segregated for xylose on EMB nutrient agar were pure lac⁻, although the parents were lac⁺/lac⁻.

Rather than continue with W-478, I decided to ask you for a sure-fire het. My purpose was (and still is) to study Hfr x F⁻ crosses involving a het parent, and to see if interrupted mating produces diploidy for differing lengths of chromosome.

I now think it will be best to work with a het F⁻, carrying as many markers as possible, so that it can be crossed with different F⁺ and Hfr stocks. Can you let me have a het stock that is F⁻? I'll put the other markers in it, myself, of course.

Your letter sounds like I may be duplicating some of your own intended experiments, but I can't see that it will hurt to try. In answer to your question: my own experiments were inconclusive and poorly reproducible. I was left with the strong conviction that properly starved cells will not cross in buffer, and that in fully-supplemented minimal, mating proceeds only after an extensive lag period. In other words, I believe synthetic mechanisms must operate, contrary to Fisher's findings. The difference in results stems from the fact that he used broth-grown, 30-minute-starved cells; I used minimal-grown, 150-minute-starved cells.

The work of Francois Gros indicates that only the latter treatment can really stop nucleic acid synthesis.

I have been meaning to repeat my work here, inoculating properly starved cells into supplemented minimal and comparing restoration of mating ability with growth phase. I expect that mating ability will be restored parallel with cell division, but this is just a hunch. I shall do this experiment, but you are certainly free to do it yourself. My only advice is: grow the cells in minimal and starve them at least $2\frac{1}{2}$ hours, if you are going to study effects of metabolic condition on mating!

I can tell you that there has been much consternation on the campus since the news got out that someone high up in the administration killed the chances for your appointment here. The Genetics Department is in an uproar, and no one can find out what went wrong. Do you know, or can you say? We're all most unhappy about it, you can be sure.

I hope my information will be of help, shaky as it is, and that you can send me a het F⁻ stock.

Best regards to Esther,

as ever,



Edward A. Adelberg

EAA:ms