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EDUCATION: A HERITAGE AND FUTURE FOR OREGON

March 17, 1977

Dr. Joshua Lederberg
Department of Genetics
Stanford University Medical Center
Stanford, CA 94305

Dear Josh,

It felt very good to receive your letter last week. Among other things it brought to mind many old feelings of intellectual excitement. It would be great to see you again. My sister and her family have moved to Menlo Park, her husband being with EPRI so we should be coming down at least for that reason.

It was a strange feeling for me to rummage around in our old notebooks looking for records of the experiment you asked about, the one Leo and I did to convince ourselves as well as Luria and Delbruck that your claim for conjugation was correct. I was unable to find the letter Leo wrote to Salva and Max, but I did find the postcard you wrote after receiving a copy of the letter, and I enclose the postcard. I also enclose some page copies from Leo's notebooks. As you can see we crossed W-1/6 by 58-161/1 (f in our jargon), selected prototrophs and scored them for resistance to T1 and T6 (Also T5). Then we did the reverse cross, 58-161/6 X W-1/1, scoring as in the first case for resistance to the phages. We found that the fraction of prototrophs resistant to both phages in one cross was equal to the fraction sensitive to both in the other cross.

Luria replied to the letter that he was convinced. Max said he was still unconvinced and that we should continue our studies. As your postcard shows, you noted that you had already done an equivalent experiment, which could be found in a table in your Genetics paper.

My recollections of the Pajamo experiments are more complex. As I believe, Leo visited Paris shortly before, and I believe he proposed ideas which let to the experiment which was subsequently done. I passed through Paris in the Summer of 1958 on my way back from a symposium in Prague on continuous cultivation of bacteria. I aw the Pajamo results and was struck by the fact that B-galactosidose synthesis took off at its ultimate rate within a few minutes after entry of the lac genes. I argued with François and Pardee that this meant that either DNA is the template for protein synthesis, or if there is an intermediate it had to be short lived. François listened while Pardee as I recall told me that the results were "only kinetics."

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By the way, have you ever run into work by a Kenneth F. Schaffner, a philosopher or historian of science who wrote a paper on the development of the repressor model? I have an ms which he sent me circa 1971-2 which I can send you if you like.

It may be that your question about my involvement in the Pajamo experiment may be based on my slight involvement in the discovery of zygotic induction. I was at the Cold Spring Harbor Symposium in 1953 where you had a hot debate with Hayes and Watson. That fall I went to the Pasteur where one day I argued (to Peggy Lieb and François) that one ought to be able to settle your debate by directly testing your idea that all genes were present in the zygote but that some were later eliminated. If one of the genes were inducible, such as lac, perhaps one could detect its temporary presence physiologically, if not genetically. Peggy suggested that this could be done using a prophage and UV induction. We agreed she should do the experiment. Like most Americans in Paris she was not quick to do the experiment. Meanwhile, François evidently could not suppress his curiousity any longer and discreetly did the experiment. The control, without UV, lysed of course; and he was very quick to see what this meant. This was just the clue as you know which led to the interrupted mating technique, etc.

Peggy was pretty bitter since she missed making an important discovery. But I guess it could be argued that the discovery came in the control, not in the experiment.

Incidentally, it is not at all settled as to who first developed the m-RNA idea by putting together the Astrakan and Volkin observation of an unstable RNA in phage infected bacteria and the Pajamo kenetics. I've discussed this both with François and Sidney Brenner and got quite different pictures.

Regarding discretion, my only concern is the business with Peggy and François, and it would embarass me to have that aired. I think François should have been more sharing, but I do not want to state that publicly.

I'd like to go on but I do not want to delay this letter any further. I hope the above is helpful and let me know of course if I can be of further help.

Best regards,
~~Sincerely~~,

Aaron Novick
Dean

AN/ci
Enclosures

P.S. As I re-read this it sounds a lot colder than I feel. But a rewrite would delay — really looking forward to seeing you again