Columbia University
in the City of New York
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DEPARTMENT OF ZOÖLOGY

January 6, 1954

Dr. Joshua Lederberg
Department of Genetics
The University of Wisconsin
Madison, Wisconsin

Dear Josh,

The manuscript I've enclosed has a reference (footnote 12) to a note of yours in the M.G.M. which I would like to quote "by permission of the author". This phrase was suggested by Evelyn Witkin. I'd also be happy, quite naturally, if you cared to scan the manuscript and send me your comments when you return it to me (I'll need the copy with its figures).

Now that I've gotten over my business objective let me say how much I enjoyed your letter of a month ago. We were sorry not to see you and Esther abroad. I think I knew pretty well why you were not travelling, having heard your present (summer of 1952) feelings on the matter and knowing you better than many do. Rest assured that Betty and I spent a lot of time countering rumors and answering questions. But please don't feel from Ephrussi's remarks that there were malicious rumors. Your renown and your work are such that people are naturally curious and can't be expected to have the patience you are exhibiting in your research.

Someday soon I hope you have a chance to meet Cavalli. He is a grand and interesting person. We had fun with him at Bellagio and especially at Rome. Our summer abroad was ideal on all counts (except that it's over). Imagine, studying living Amphioxus at Banyuls-sur-mer, wandering about the Alhambra, seeing cave paintings in southern France, the contents of el Prado in Madrid, the Impressionists in the Louvre, the Byzantine mosaics in Ravenna, the Alps, the Pyrenees, the Spanish coast, having lunch chez la Baronne de Rothschild at Chantilly and with a real live Russian in Rome---all in one summer! Some day I'll tell you more. For the time being, let me just conclude that I'm really not cut out as a true scientist; rather, I should have been born of a well-to-do English family when the world was their playground.

Dr. Lederberg January 6, 1954

It was interesting to hear how nicely your work is progressing. From what you say the issue of the timing of the eliminations in K_{12} seems about to be wrapped up. Did Hayes stop by on his way West? His talk at C. S. H. was beautiful; he's obviously an able man.

We, by the way, have repeatedly failed to get F^+ into strain 15 (although Szybalski says it's a snap to cross K_{12} with B). We have a phenomenon in fitter types that in one way is reminiscent of F^+. The fitter type practically prevents the growth of inferior strains in mixture, but we can get the cause of this suppression despite our use of the Davis U-tube and more. Thus far, we have not been able to kill the fitter type gently enough to retain its inhibitory effect.

Reinhard Kaplan has been here since October, busy with delayed mutations in Serratia. Because it is inherited through dozens of generations, he is exploring the hypothesis that the instability after radiation is in the genetic material. I'm pleased with him. Hourly I expect Moewus to join us. This will also be fun. On January 29 we expect Jacques Monod for the Jessup Lectures. Saw Lil and Stan Wainwright at Christmas along with Dave and Dot Perkins, Ravin, Gordin Kaplan and hordes of other old friends.

By the way, please tell Tom he's owed me a letter since January 25, 1953. And please give Esther my sincere apologies for overlooking her in that Scientific American paper. It's partly your fault, Josh; you loom too largely in my mind when I think of work from Wisconsin.

With best personal wishes to you and Esther and my other friends out there.

Sincerely,

Francis

Francis J. Ryan

FJR:k

nothing in the medium that could do