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DEPARTMENT OF SOCIOLOGY

Fayerweather Hall

September 16, 1975

Professor Joshua Lederberg
Department of Genetics
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Stanford, California 94305

Dear Josh,

Your batch of items arrived in somewhat disreputable shape, having gone through the damaged mail section of the San Francisco Post Office. The items that survived were your memos on fathers and sons, peer review, Tatum and Garrod, Disciplinary Differentiation Exemplified in the Establishment of Sections in the NIH Granting Mechanism and your note on the NAS biographies. As usual, all of them were illuminating and I have ordered Arthur's book so as to read his version of the episode I asked about.

Otherwise there is not much to report. Both of us are still in a transitional phase and trying to find room for the accumulated books and papers acquired at the Center. I have checked with a travel agent about places for us to meet on the bacteria book and will send you folders when they are available.

It also occurred to me that I might not have given you the correct reference on that book Women in Science. It's H. J. Mozans, Women in Science, Cambridge: M.I.T. Press, 1974. The original was published in 1913 and should amuse you.

Apropos my query on fathers and sons, I noted in the obituary for G.P. Thomson that his son recently married Bragg's daughter and this links together two ^{Nobel} grandchildren. More seriously, apart from the names you mention, I do not know of other multi-generation scientific families in which the parents and children are distinguished. There are, of course, a fair number of scientists who have scientist-fathers (and the reverse, of course.) A fair number of the American Nobelists had fathers in one or another scientific occupations. I suppose I would add to your list John Bardeen, whose father was Dean of the

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Medical School at Wisconsin and was an anatomist; F. C. Robbins whose father was a mycologist and Director of the New York Botanical Gardens; J. H. Northrup whose father was a zoologist; and R. S. Mulliken whose father was professor of organic chemistry at M.I.T. I quite agree with you that not much should be made of the relationship except insofar as such fathers provide appropriate contexts of socialization. I would suppose that the possibilities for intellectual competition and confrontation are immense, especially because of the tendency toward cross-generational conflict in science that exists in any case. As far as I know, Galton did not catalog such pairs or trios.

We should talk further about the Olby book. I am told that it is full of errors or more precisely that Alex Rich said that Olby got nearly everything wrong that he told him. What is your reading of its veracity?

Best to Marguerite, David, and Annie, of course.

Sincerely,



Harriet Zuckerman
Associate Professor
of Sociology

P.S. Comments on the Lindzey memo and your notes to follow.

HZ:pb