

CHEMICAL CORPS BIOLOGICAL LABORATORIES

MEDICAL BACTERIOLOGY DIVISION

Camp Detrick, Frederick, Maryland

14 July 1953

Dr. Joshua Lederberg
Dept. of Genetics
University of Wisconsin
Madison 6, Wisconsin

Dear Josh:

Since the Hayes MS that I have appears to be a final version I am sending you my extra copy. I have been impressed all along with the fact that Hayes' major conclusions do not differ decisively from yours and I regretted the fact that following Hayes' presentation at CSH a considerable number of people seemed to get the idea that Hayes' views were in conflict with yours. I did not feel that it was up to me to correct this impression during the actual discussion, since there were others present who were far closer connected with the actual work than I. But I pointed out privately to a number of people that, as far as I could judge, there were no major disagreements between Hayes' views and yours and, actually, you had stated many of these findings in prior publications. I believe that the initial impression of possible differences arose because references to your and Cavalli's studies were not as prominent in Hayes' oral presentation as in his MS.

In regard to the footnote of your letter of July 8, I fully agree that residual growth probably cannot account for the qualitative differences between F^+ and F^- ; we are merely trying to see whether and to what extent it may contribute to quantitative differences in regard to frequency of recombinants, independent of the type of differences that occurs in crosses with reversed F polarity. We haven't been able to follow up this question as rapidly as I anticipated, partly because we got too involved in an entirely different story pertaining to selective effects of DNA.

I am leaving for Europe on August 14. What are your plans? Are you going to the Congresses or are you going to attend the San Francisco meetings?

With best regards to Esther and you,

Sincerely yours,



WERNER BRAUN

WB/bls