

507 EUGENIA AVENUE
MADISON 5, WISCONSIN

November 23, 1957

Dear Jim:

Esther and I are just back from a 4-months trip, primarily to Australia, but returning the long way round.

I found waiting here your letter of April 3 (sic) which had been addressed c/o Curt Stern at Berkeley, and must have found its way back here last month some time. I don't have my old correspondence at hand just now, and we may have covered the same ground since then, but I really was deeply moved at what you had to say about Harvard. Perhaps you've quite forgotten that letter by now -- its main point was "I was disappointed in that it appears that you may decide to go to Berkeley without first coming here to look us over."

It is going to be very easyh for me possibly to contradict the momentarily expressed sentiments of some months ago, but I would not want you to think that I had given Harvard or the possibility of moving to such a remarkable 'university' of colleagues short shrift. I felt I knew Harvard reasonably well already, and I was rather worn out by recent and prospective travel. What I later tried to say, and did not very well at it, was that I was then already involved with Berkeley, not with ~~with~~ any commitments, but in such a situation that would take well over a year from then to mature. I could not see the point for either Harvard or myself in dangling all that time. When the Berkeley matter did ripen, I could then look at alternatives then current. This is not an invitation to reopen the discussion at the present time: if or when that becomes apt, I'll do more than hint at it to you. But I am most anxious that the recent affair not lessen the cordiality of our relationships nor our mutual regard.

I am sorry that my Harvey lecture next month will come at a time that would be personally and academically rather unpromising for a visit to Cambridge. I have demanded a raincheck from Carroll Williams, and hope to use it before too long.

I heard recently of Pappenheimer's coming to your department-- what a clever way for Pap' to unload his administrative load! If you care to tell me about it I'd be glad to hear your department's current plans for genetics, insofar as they can be a matter of general interest.

I had rather an interesting time with Burnet. Influenza virus, despite the lack of a plaque method (or maybe there is one from Ledinko & Henle) is still as good an animal virus as any for recombination analysis. I came to the conclusion that Burnet's scheme of two linkage groups, within which no crossing-over takes place, is at least partly an artefact, from technical limitations in the selection of ~~recombinant~~ crossover genotypes. Someone like yourself ought to make a new start with flu; there's lots to be done, and much that can be done with a more precise analytical approach.

Yours, as ever,


Joshua Lederberg