

Department of Genetics
University of Wisconsin
Madison 6, Wisconsin
March 28, 1952

Dr. C. C. Spicer
London NW 9, England

Dear Dr. Spicer:

Your letter of the 24th has just been received. Needless to say, I am gratified at your interest in working with us.

In principle, your proposal meets with my enthusiastic approval, and I just wish that we had the facilities that would permit an unqualified concordance. Unfortunately, we are literally crammed together in our small laboratory, and must give very careful consideration both to our needs and those of a new entrant. However, there should be some alleviation of this pressing condition sometime this fall, so the situation is not entirely hopeless.

Thus, I can see no possibility whatever of your working effectively here during the rest of this academic year. Next September, we will be a little less crowded, but still tight for space, and there would also be the contingency of some interruption from the remodelling of the laboratories (I hope!). The most likely period would be, probably, from about March 1 - June 30, 1953. With some advance preparation, it should be possible to do a good deal in four months. Would this schedule suit you? At any rate, let me know the constraints on your plans so that we can look for common ground. You might be interested to compare notes with your colleague E. S. Anderson.

There is no dearth of potential problems worth considering. In view of your recent experience, it might be profitable to look at some other serological transductions in our present system involving groups B and D (more precisely, XII₂-carrying Salmonella). These are of more than horticultural significance, as I think they may shed some light on the genetic basis of flagellar phase variation. The other possibility would be to look for transductive or other recombinational systems in group C and E. I am a little suspicious (as they were) that Bruner and Edwards' anatum-newington results may have been transductive. It would not hurt to prepare for your visit by collecting strains in these groups, and perhaps by starting to produce auxotrophic mutants in some of them, with a special look-out for lysogenic types. How about thompson? Or would you prefer potsdam in view of your studies on potsdam x typhimurium? (How's that coming along, by the way?) Well, we can discuss these details later.

Sincerely,

Joshua Lederberg