

November 12, 1951

Dear Tracy:

If we hadn't just returned from a weekend visit to the Novicks, I'd be tempted to arrange to meet you in Chicago this Sunday. But, in fact, it might be better for me to study your syllabus and course outline, and think about problems of organization in some detail before we took time for a full discussion. May I let you know just when I feel ready to go into it? But if you can find your own way clear to pay us a visit, we'd be delighted to see you, book or no.

I'm still not sure whether I feel up to "full co-authorship", for I still look on any participation I might have as an assistance to you. But this is the one thing we shall have to iron out. A cooperation on this basis would minimize any problem -- if there were one -- of differences in outlook on particular subject. Our interests have been just divergent enough that this is unlikely to arise.

There are two points on the syllabus in Neurospora: the first is bisexual heterokaryons. Although attempts to make them often fail, I found a number of combinations which worked quite well, although, like *sitophila-crassa* combinations, they tend to dissociate readily. I don't think that Sansome's generalizations (your p. 6) are correct.

Your speculative support of postreduction of centromeres is quite interesting. Rizet (*Rev. de Cytol. et Biol. Veg.*, 11, 1949) suggests that the tetrasperma type of heterokaryotic spore formation results from regular ~~mitotic~~ reduction of the mating type locus, and postulates an obligate single crossover. Centromere postreduction is no less objectionable. Lindgren, in a paper he wrote 10 or 15 years ago, but never published, had some evidence of a stock carrying a pericentric inversion, which when heterozygous caused centric postreduction. This might be reasonable, if the inversion prevents proper synapsis, and therefore regular disjunction, of the pericentric region. I rather like the idea.

Sincerely,
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Joshua Lederberg