181 Claremont Avenue #51
New York 27, New York
June 1, 1950

Dear Josh,

I'm taking the liberty of sending you a copy of what turned out to be a Ph D thesis. It's the material I told you about at the AAAS meeting when you dropped in with the diploid slides - the kinetic proof of cell fusion. You'll probably have to read it anyway since it was sent to Genetics and they may ship it on to you for refereeing. It slipped past Ryan, Atwood, McClintock, and Witkin at the oral exam so there can't be too much out of line in it. Don't worry about returning it.

Kim has already left for Woods Hole. Ryan is leaving tomorrow for his year and a half sabbatical. That will leave Columbia dead for some time.

That succinate mutant that I was working on turned out to be too much for me to handle. How it arises I don't know. The manometric data, once they're corrected according to Karlsson's method, give the same results as Karlsson got on Azotobacter glucose-less except the block appears from malic to pyruvic (or really lactic since pyruvic is not attacked, probably impermeable) in coli. 679-680 and Y 24 are s plus (grow on and in succinate, fumarate, and malate), Ryan's parental strain of K 12 and all the mutants derived in this lab are s minus (tested by serial reversion and growth tests on the appropriate carbon sources). Hence the parent K 12, s minus, can be crossed with Y 24 and 679-680 on fumarate or malate agar (succinate agar is screwy - s minus gives colonies on it but it's due to the agar since no growth occurs in liquid succinate unless a small amount of agar is added) and the s plus prototroph picked up. Mapping, such as has been done, puts the s locus between the cystine-phenylalanine and leucine-threonine loci. Crosses with Y 53 put the s locus between the lac locus and the leucine threonine loci tho' I have no decent quantitative data on it (only about 100 isolates tested). The s plus strain on which all the biochem has been done hasn't been crossed yet - no markers in it - but your azide resistance sounds like an easy marker to get so I'll try it. However it must be coli - sensitive to the 1 through 7 phage in the correct K 12 pattern for this lab's parent strain.

The only info I have on recombination around here besides what's being done here is some dope from Davis' lab. There's a chap there taking a year off from med school and working on proof of Mendelian segregation and 4-strand crossing-over. He apparently has the right ideas - use of two sets of biochem markers, say ab double minus at one end of the chromosome and cde triple minus at the other, and scoring in medium containing C, D, and E to pick up all the classes.

He'll probably try the resistance markers now that you've picked up the two best.

Ryan got rid of both T. C. Sheng and myself this year. Both of us are going to Cal Tech. Mitchell will be my boss so it will probably be biochemistry. However there will be time to work out a sure fire paper on the rate and final amount (saturation) of zygote formation as a function of the culture phase using media in which the culture phase can be precisely determined - nutrient broth is out of the question. From what data I have on supplemented minimal medium it looks as if there is little syngamy in the log phase - most of it after the maximal phase has been around for a couple hours - and a gradual decline. The results are based of course on the number of recombinants in a given time under given shaking conditions per so many viable parental cells.

That's the news from this end. So, until this summer and California, even if Berkely is 400 miles away.

Sincerely,

Tom Nelson