

Columbia University  
in the City of New York  
[NEW YORK 27, N. Y.]  
DEPARTMENT OF ZOOLOGY

12/16/47

Dear Josh,

This long delayed letter is designed to answer two of yours (Oct. 7 & Nov. 30) & to give you a little news about your alma mater.

Thanks for the copy of the 679-680 summary. The variation in total population from tube to tube is, as you point out immense. For that reason it is a poor experiment. The  $10.02 \pm 1.99 \times 10^9$  cultures were probably contaminated if I can judge from Lelliand's note in the record book that in one case there were 92 surface colonies present. Excluding these, there is still too much variation compared with what we get with histidineless where the total number is constant not only from tube to tube but from month to month. The reason for this variation, which will be easy to avoid, lies in the conditions of the expt. ~~23~~ 23 hr. complete medium cultures ~~were~~ were centrifuged & resuspended twice in saline & allowed to stand 8 hrs. before plating.

The culture was too old, washing introduced some variation & & has in saline we know is quite lethal. Next time we'll do better.

But when is next time. I want try to outline for you our histidineless data. They become more & more interesting & more & more complex. The complexity, as you will point out is a function of our ignorance, but I think we are on the verge of having a pretty good approximation of an explanation. I hope in about a month to let you look at a manuscript or two. (Probably you will see them in 1950 - realistic statement.) However in my optimism I hope to get to Y53 this spring. I want be at Chicago (Perkins will) so why not come all the way east for a visit & we can talk over this business. Right now - the only way I could explain histidineless is to write a manuscript or talk with a blackboard & a couple of hours time.

Dave is busy with his corn smuts, now has a lot of biochemicals. He knows about the Sphaerolotheca stuff & is about ready to go to town on recombination in vivo & in vitro. He hopes to be finished this

summer. If you need, or know someone who needs, a first rate man, keep Dave in mind.

Kim is now getting in stride. I think the most interesting thing he's studying is an adaptation of the drier technique to determine the weights of the same mycelial clot at different stages of growth. As you know, he has plenty of imagination & ingenuity. I'm trying to hammer home just a little discipline. I seem always to have gifted problem children, don't I? I like them.

Sheng is very busy with ~~courses~~ (alas, no) courses. He has, however, spent some time on the asparagine adaptation in R12. He cannot confirm what I think you told me i.e. that  $C_4$  acids will substitute for asparagine & aspartic acid. Do you plan to follow through on this? I think Sheng is interested in doing so. There is a peculiar inheritance of the adaptation.

Kim, by the way, is another father, as of Sunday night - a girl named

June. All are well. Carl Clark, as an additional by the way, gets married this Saturday.

Mrs. Leuchtenberger & Peg Diehl are learning Neurospora techniques in '60 of 219. As a small problem they have studied the segregation of nuclei into branches in a heterocaryon. Segregation does occur. Tom Nelson just handed in a report on the respiratory metabolism of Neurospora consisting of work he did in 219 last year. He's toying with the idea of following through on that. There's a bright & industrious Coy.

So you see the 10th floor is humming.

We've nothing spectacular like Evelyn's salt mutations or Gus Doermann's ~~work~~ <sup>arithmetic multiplication</sup> on linear duplication of phage but we're just beginning. Lil is getting mighty pleased with our physical setup, which is improving. In a year or two we'll hit a jack pot. Just this month have I really gotten back in the lab. The administrative crap, departmental but mostly College, has risen to an all time high. I'm getting very fed up for its only by brute strength.

• not intelligence that I get any time for my own research. And as you know I get a very large kick out of a hand in a clean experiment. I don't like to talk, especially about "high" policy, like whether the pre-engineers docket should follow them into Engineering school or should remain in the Deans office. So much for that or I'll love you to death.

Returning to your letters Rubin has contacted me but as yet we have not gotten together. Glad to hear that your facilities are developing favorably or that you're getting things done. You have, by the way, finally converted Beadle to your cause. He still yawns thru the city every once in a while. Did you know that Mitchell found that Paves nicotinic precursor is OH anthranilic acid? DeBrock is apparently organizing the mathematicians & theoretical physicists around Biology & is considered a real big asset. I'm sure he is.

What's the story on the microfilm of the Stanford theses? Is the next

move yours or mine? Also- are there  
any really promising young Crockerists  
coming thru at Wisconsin?

This letter has been interrupted by as is  
interrupting the determination of growth  
curves of shaken cultures of  $10^8$  +  
that's too damn bad So -

Regards to Esther, Snell & any  
one else would deserve & appreciate them

Sincerely  
Francis