

# OAK RIDGE NATIONAL LABORATORY

OPERATED BY

CARBIDE AND CARBON CHEMICALS CORPORATION

FOR THE

ATOMIC ENERGY COMMISSION

POST OFFICE BOX P

OAK RIDGE, TENNESSEE

Biology Division  
August 20, 1949

Dear Joshua,

I'm sorry to be so slow in replying to your last letter but it didn't catch up to me for quite a spell. I'm further sorry that I will be unable to do any further work until about the middle of September. I'm anxious to finish up this stuff with Hollander and get rid of it which accounts for my being here again. When I do get back to work on the hets again, I should be able to concentrate on it and try to get the job done.

I don't have much of our correspondence here and <sup>no</sup> of the data so can't too intelligently plan for further work. I'm curious to know if most of the Xylv cultures I sent turned out to be H-168. I received the copy, streaked it out and had to leave it. I should be able to pick it up when I get back.

Could I have accidentally isolated a ~~possible~~ segregating EMS colony of say H-72 on one of my transfers of "H-168"? I don't remember for sure but H-72 was not segregating for mannitol was it? At any rate, this is a possibility that should ~~have~~ be considered in trying to account for the loss of Xylv in the old "H-168". Naturally, I don't think any such error was made but it is always a possibility.

I don't grasp what you mean by the statement "I wonder then how much of the work of H-168 depends upon the persistence of the Xyl-".

As to future work, I will leave it to your judgement as to whether further work with H-168 is desirable. The question of the relation of lethals to the aberrant ratios could probably best be studied here but, if the cells are multinucleate, it doesn't <sup>seem</sup> too hopeful. I wonder if there's any way to isolate a uninucleate het strain, a double, triple etc. and then maintain them? Or does the number of nuclei per cell vary with say the growth stage of the culture, ~~say~~ log versus stationary phase? As I remember it, the earlier segregants occurred as sibs <sup>to</sup> het cells while later, <sup>usually</sup> occurred as sibs to lethals. I wonder what cytological study would have revealed if anything?

Certainly we want to study H-206 to get at your question "c". I succeeded in isolating the hets from it and they should be ok when I get back. Did you not mention a couple of other isolates similar to H206 in an earlier letter? If so, were they considered as still more suitable stocks for this?

I guess that is about enough rambling for this time. I plan to return to Ithaca about the second of September and then, come hell or high water, I am going to do some of the fishing which the wife and I have postponed two or three times this summer. Again, I'm sorry to have to delay this work but rest assured it does not reflect any loss of enthusiasm. Rather, I'm trying to get into ~~position~~ position to push it through. Apologies for my typing which is poor at best and which is not helped by my first experience with an electric typewriter.

Sincerely,

*M.A.*