Air Mail

YALE UNIVERSITY
OSBORN BOTANICAL LABORATORY
NEW HAVEN, CONNECTICUT

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Dr. Joshua Lederberg
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Dear Josh:

An adequate answer to your last letter is indeed difficult, and I will in a sense delay answering until I see you in New York. I believe you read into my activities much more that is sinister than is truly warranted. Indeed, competition with you in a sense had not particularly occurred to me since we got started on coli from work with little bearing on your field of interest. That we overlap to an extent reflects primarily the complexity of coli and of adaptation. I could write an answer, but letters are subject to misinterpretation, which in turn leads to ill will. I suspect we can rapidly iron out the questions you have raised and to everyones mutual satisfaction orally over a spot of beer in New York. I was pleasantly surprised to find that you would indeed consider me, and my poor lab, real competition.

The reason for the preceeding remarks hinge simply around the fact that you have staked out a tremendous area. Obviously we cannot steer entirely clear of it with K-12, since much of our serological work requires purified enzymes and a knowledge of the physiology of adaptation. Indeed with the mutants it is difficult to remain entirely clear of such an all-inclusive field. The serological differences need to be correlated with enzymatic differences. Such information is as useful after all to you as it is to us.

I realize full well the discomfort of having some one grab the precise problem one has under investigation. That question, however, I don't believe really is involved here, in a field as large as this. I can assure you, though, that any strains you may send us will be used only for the purpose of establishing any serological differences or peculiarities they may exhibit. Further work that we might want to carry on with them would depend upon the outcome of the serological work and upon discussion with you.

Regarding the serological work--this has been carried out by Gabe Lester. Lester has found that antisera from rabbits injected with lactase grown cells does not directly inhibit enzyme activity. The antibody can apparently combine with the enzyme without affecting its activity. However, the enzyme-antibody complex can be precipitated by centrifugation under conditions that the enzyme itself remains in solution. This, in substance,

is his technique. He finds as I wrote earlier, that antisera to glucose grown cells does not display this effect upon the enzyme, which is in agreement with our ~~venture~~ failure to find lactase by our methods in such cells. Y-53 grown on glucose, we find similarly lacking lactase, yet antisera to such cells has a detectable amount of what we call incorrectly, "antilactase." Lester has found, by the way, that whole cells are very much poorer than are ground cells in terms of the serological differences that I mentioned.

With best regards,

Dave