



THE COLORADO FOUNDATION FOR RESEARCH IN TUBERCULOSIS

GERALD B. WEBB MEMORIAL BUILDING

4200 East Ninth Avenue
Denver 20, Colorado

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10/9/56
10/15/56

Dear Joshua and Esther:

Thanks for the letter and the enclosures.

I have looked over Kurahashi's MS and sent it on with a note to Kalckar. I made no changes or suggestions other than a couple of typo's. (I was surprised to note in your letter that you thought that the "genetic background" of W-3178 "is therefore less settled than for the members...").

I was surprised to hear that Kellenberger is working on heterogenotes. But he is not alone for Latarjet told me yesterday that Weigle is also working on this aspect of the problem. Beyond that I know nothing of his work. Kellenberger's letter is not very specific and I don't know whether he is working with K-12 and if so with any of the Gal mutants that we have. I hope that he gets the importance of doing reversion tests on Gal. I've never had a culture that went to Gal-Lp^{r/s} that either segregated Lp^s or its Gal reversions segregated. I'll get together a summary of the segregation studies on W2868, which I've studied fairly well (segregation, classification of segregants, and some good reversion studies). As you remember I'm scheduled for a talk in New York at the Academy on November 19. ~~XX~~ I've been debating just what to say there outside of the generalities of review statements. This might be a place to put down some of the r/s data. I'm not sure as to how it could be presented as an isolated paper, the most it could be would be descriptive, and this might be more easily handled ~~XX~~ in a review of the lambda system. I hate to lose the data because I don't feel that it has lead to logical conclusions, but some of these other people will publish it in incomplete form. From a statement of Latarjet I believe that Weigle almost has a manuscript on the production of transducing lambda. I infer this from a question that he asked about the burst size. I'm still not satisfied with my data; mostly from the standpoint of presentability rather than from the conclusions that I can draw. I hate to publish things hastily or before I'm ready to. Maybe I'll have to, to keep from losing the data. Some of the experiments that Kellenberger et al are considering I did two years ago.

I just got my rotator this week and can now make lysates and do lytic experiments. I've planned a couple of one steps this week and if they are satisfactory I'll try to get AMS together however brief it may be.

I've not had much success with Gal mutants in W3100. They have been few and none that turned out clear cut from ^{the} Gal₁ or Gal₂ groups. There were a couple non-transd. but what to do with them. I have also been looking at W3142 and its progenitors W2733 and W2734. This has been by crosses. I'm not sure now that this mutation is distant from the gal group. With 2733 and Gal₁ I get few protos but from 2-30% Gal+. With 2734 I get few Gal+ and many protos. This is in agreement with Esther's view that there is another marker in 2733. These cultures with the other Gals have given low percentages of Gal+. I'm at a state where I would not make any observations on distinctions between 3142 and the other Gal-.



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I've got some one helping me in the lab part of the time. Unfortunately this means that I have more trouble getting paper work done, which is what I really need most now. I'll have to hole up for a couple of days and get this stuff summarized.

I have been thinking about the possibility of stopping in Madison on the way back from New York but have not as yet made definite plans. Will you people be there at that time? I met Dr. Benjamin at a party the other evening. He is really a remarkable individual. The combination of interests that he has is amazing. (At this stage your letter inviting me to Madison came. I will try to make it and will let you know more later about my plans.)

This afternoon we are having a seminar from George Klein. I suppose that you have already seen him and talked over all his latest results. His topic here is general- "Exptl. studies on the mech. of tumor progression". I guess (as you experienced) that the audience will be small and subdued. A target was a bit disappointed I believe, because there were no questions after his talk. I told him later that the people most interested were the ones that he had spent the day with, and they had few questions left. I found his talk interesting but some of his conclusions were based on opinion. He was pleasant and Helvise and I had home to lunch one day.

Helvise has gone to work here in the medical school. In the Ob. dept. but not as material. She is setting up a tissue culture program, whose purpose is a bit obscure to all. An Ob who took Ted's course is in charge and he has not decided what to do. Most matters requiring decision around here gravitate to Ted so that I suppose he will give the project a direction.

We received our absentee ballots to day from Bareis, city clerk. We will vote them but I am still uncertain whether it is legal. You got any opinions?

As evr,

Larry

*I copied
your enclosed
letters - I
hope you don't
forget*