B. Wright

CARLSBERG LABORATORIUM
KØBENHAVN, Valby.
Kemisk Afdeling. Telf. Valby 14 og 28.

Nov. 3, 1951

Dear Joshua -

Just got your "papers in Microb. Genetics," & want to tell you how useful I think it is. I've been running back & forth from libraries with heavy volumes containing many of these papers - it's really nice to own them, underline things, etc.

Now I Think I remember what happened to my reprints of your work. I'm not too systematic and don't put my name on things quickly enough. Although it's some time ago now, I believe I loaned them to a fond colleague at the Hopkins Marine Station not long after receiving them from you. By now he may well have his name on them - I'll write him a searching letter soon & find out.

Enclosed is a copy of a letter that has been accepted by Nature for publication in a couple of months: So I've committed myself about some of the "glycerol acid work." I've done all the crazy controls Luria, Delbrück & Ryan (who's not yet quite satisfied) could think of, after I was satisfied - so - with still some hesitation - I sent it off. When we discussed it at Pacific Grove, I remember you said: "whatever you do, don't publish a statement that you have a specifically-directed mutation!" Well, as you can see, I haven't really done that - there are 2 nice alternative

that can't be ruled out. The more exp's I do the surer I am about the thing. PH exps with tartrate (see letter) indicate that it is the undissociated acid that's effective; that over 1 pH unit, the mutation rate (tartaric give most wild types, not interm. types) is about 10 x higher – i.e., at pH 5.5 10x higher than at 6.5. ~~at~~ If you figure it out with pK's & things, there's 10x as much acid at 5.5 than at 6.5, and the conc. of the free acid is of the order of magnitude of the glycine or serine requirement of the bug for maximum growth, not that I place any particular significance on the fact. That's about all the definite new information – plus some more controls and such. Could you return that paper? At your leisure – it's the only one I have.

On the temperature-invariant clocks I'm not working at present – although a good deal more was accomplished for my thesis after we had discussed these "intermediate types" at P.G. In the long paper I'll start writing before long, there will just be some safe generalizations about them; I intend to start extensive work on it in 4 or 5 months. In my thesis (with Kee's permission) I concluded on 3 independent lines of evidence that a "mass transformation" of the population

CARLSBERG LABORATORIUM
KØBENHAVN, Valby.
Kemisk. Afdeling. Telf. Valby 14 og 28.

3/ takes place at the critical times. Simple selection is definitely ruled out. But I'm still unhappy about this, & have many plans about looking into the mechanism more critically.

I have a nice set-up here, using both Winge's & Tang's departments. I only lack people to talk to about problems of microbiology. I got a fellowship for another year's work here. Then our plans are indefinite; they are not (able to be) too generous with money & positions in Denmark.

Many thanks for giving my name to the U of W press.

I would of course be interested in your reaction to the paper.

My best to you both.

Sincerely,

Barbara