Dear Joshua:

Just got your "papers in Microb. Genetics," I want to tell you how useful I think it is. I've been running back and 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 didn't put my name on things quickly enough. Although it was some time ago now, I believe I loaned them to a fellow 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. I 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 "glycemic acid work." I've done all the crazy controls first; Debruck & Ryan (who's not yet quite satisfied) could think if, 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 alternatives.
That can't be ruled out. The more cops I do the sure I am about the thing. pH 6.0 and Tarts (see letter) indicate that it is the undissa-
ted acid that's effective; that over 1 pH unit, the mutagen rate (tartaric gives most wild types, 
wt interm. types) is about 10 x higher - i.e., at pH 5.5 10 x higher than at 6.5. If you figure 
it out with pK's & things, there's 10 x as 
much acid at 5.5 than at 6.5, and the ease 
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 particlu-
lar significance on the fact. That's about all the 
definite new information - plus some more controls 
and such. Could you return the paper? 
As 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 those "intermediate types" at P.G. 
In the long paper I'll start writing before long, 
there will just be some soft generalizations about 
them; I intend to start extensive work on it in 4 or 5 months. In my thesis (with Kees' permission) 
I concluded on 3 independent lines of evidence 
that a "mass transformation" of the population
It takes place at the critical times. Simple selection is definitely ruled out. But I'm still not happy about this. I have many plans about looking into the mechanism more critically.

I have a nice set-up here, using both Winge's and Tung'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 general, with money & positions in Denmark.

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

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

My best to you both.

Sincerely,

Barbara.