June 8, 1955

Dr. Thomas C. Nelson
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Dear Tom:

Your last letter was most disturbing-- I hope you have been able to make a respectable deal with W; though I admire your tenacity in going after what you want and deserve, I hope you will keep place for a limited amount of prudence as well.

At last I have been able to get to your ms.-- it was indecent to take so long, but believe me it was not for want of the wish, so you can judge what a pileup there is. Matters should improve over the summer (I hope!). The ms. definitely does not belong in "file 13"-- I would think either J Bact or JCCP will be glad to have it after it is polished a bit, which I assume is the main thing you want from me. Since you were able to provide two copies, the easiest course for me was to make pencilled comments on one. Could you let me have this back when you send any revision, so I can refresh my memory on my first reactions?

On the whole your style is good, commendably condensed-- in fact you are one of the few people that I would have to urge to expand a little at the right places. Most of my comments are on details, as marked. Don't take stylistic substitutions too seriously, especially my versions: this seemed only the most economical way of stating a criticism and you will probably do better in your own words.

By way of general comment, I would recommend that you right this paper as entirely precedent to the microscopic studies, which the work was. There will be time when this is published to refer back to your kinetic work. In particular I would be very cautious about "male" and "female", and would prefer myself that you not use these expressions (for which you have no direct justification) in this paper. In fact, to be precise, Hfr is not synonymous with male. Hfr is hermaphroditic, while F- can only function as female (in terms of the definition that male is equivalent to migratory gamete). Your discussion might want to say something on the bearing of your findings on the detailed morphological mechanism of syngamy; I do not see any statement that would rule out Hayes' old notions (he has fully recanted, at least privately), and there is really no need to make any special assumptions to present your data. I have not gone over your mathematics, as I thought I could save time if you would simply send me your derivations, both p. 12 and, for the diffusion calculations, p. 13, which I will check over carefully, as deserves to be done. Well, the rest of my comment is on the text.

Yrs. etc.,

Tâtle and introd. paragpaph:

Either physiology or mechanism, not both.

How about: Physiological studies of genetiw recombination in E. coli
or ...... sexual .....

since "genetic recombination" now covers a lot of territory.

1st para. is especially awkward, you were impatient to get it over with.

Suggest along following lines:

Most studies of recombination in bacteria have emphasized genetics rather than physiology, (1 & esp. discussion there with Westergaard), although mating types ( ) and chemical and physical influences( ) have been examined ((I shudder to put these in the same sentence!)). ((I don't think you particularly have to mention Kann, which came to nothing, especially if your first paper does; if you do, tag it onto this last sentence). A previous kinetic analysis ( ) gave experimental results which agreed with second order kinetics, that is
a theoretical model of rahdom collisons of two species of particles, the parent cells.