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4th.May 1955.

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Dear Josh,

Many thanks for your letters of 11 and 20 April, for the "Genetics" abstracts, and for the draft. As to the latter, I have not yet given it adequate study, so I won't comment to any extent.

As to publication, I am inclined to agree that our different ways of writing things up, together with such discrepancies as remain and the laboriousness of having to correspond rather than talk make a joint paper hardly practicable. I agree twin papers as you suggest. From my point of view J.G.M. would be preferable, and as far as close contact with the editor goes I am O.K. here. I saw Standfast yesterday, he says that average delay is now about usual, i.e. something like 6 months from submission to appearance, and I think this is no worse than elsewhere.

As to joint paper and abstract for Genetics Soc., I am agreeable; it is hell to say anything on this subject in 250 words, and I would prefer to let you do this. As I don't disagree with anything you say, (but merely assert something further) I feel confident anything you might say will be O.K. by me, whereas the reverse case might not hold. I enclose the abstract (circulated, and will ultimately appear in Heredity) of my paper to Genetical Soc. here. This had to be 200 words only, which forced me to be less non-commital than I wanted to be. I hope the inverbed commas round "gene" etc. soften the effect a little.

As to terminology, I shall now revise and shorten my draft, and see if it can be done without neologisms; if not catenate and linear seem about equally good. On symbols, I rather fancy your ~~Crook~~ notion. How about E for exceptional cell, defined in terms of more than (n) motile progeny, and "epsilon" for the (hypothetical) particle conferring the E property? And similarly

M, or (perhaps with superscript number) for cell with less than (n) motile progeny and "mu" for the (hypothetical) mcps ? No doubt both Greek letters have been used before, but not I think in bacteriology in any related sense.

Could you sometime, let me have the full references which will go with your draft ? The papers I don't know are Jennings 1937 (or 1927) , Kallio, Lansing 1948. Sonneborn 1935,1954, Barton 1950, Wangermann, 1954. I am much impressed by your general discussion and mean to spend some more time on it.

One point on which I would like information (and which might perhaps be included in your draft) is p.4 "10% had 2 or more, up to 100". Do you have any detailed distributions?

Your 11:20 split does not worry me too much. My use of 10 as a discriminant is empirical, based on apparent bimodal distribution in SW541. If it had occurred in SW541 I should fairly confidently diagnose the 20 one as an E and the other as a non-E. However, without an estimate of n, which involves making an estimate of the "efficiency of detection" of uni-catenate cells, one cannot tell whether this is plausible.

You say you had not a notion of Q's work. I did in fact mention it in my letter of 3rd. February (para 5), however, maybe you mean no notion before this. This work is going ahead. He hopes to extend to other genera in which O strains are available. If all goes well, he, or he and I, might do a short communication on this, e.g. letter to Nature, in due course.

To revert to your letter of 11 April, I am glad to hear the Gal-duction story is out, and hope to read it in full soon. How do you explain the instability? I suppose by interpellation, forming a "re-duplication". Its hard to see why the same does not occur in Salmonella. In your one on phase-variation you speak of correlation between antigenic state of donor and competence of lysate; do you mean by this state of donor as to phase expressed at time of lysis ? You may remember that I did not detect any obvious difference in competence of lysate of TM2 in phase 1 or phase 2 when tested for ability to transduce i. I shall be interested to hear if you did finally demonstrate this effect. You say someone, it looks like Iino, isputting steam on again on phase variation. Who is this?

Only progress here is on double specificity of initials from SW543. Using my own sera, fully cross-absorbed, I am now convinced this is genuine, as tested by micro-manip. transfer into various sera in droplets. Only just started on this, so details later. I have belatedly started using 543 derivatives as donors, which makes yield much better; my thanks to you for this idea.

One last point, raised by Q, on your draft Page 13, suggestion 3. I hypothesise that the mcp is a particle which generates a flagellum e.g. basal granule, not the extra-cellular flagellum itself; because our (incomplete) acid-washing experiments indicate that treatment which destroys flagella does not destroy the mcp. A minor point, but as you are being so complete it might be as well to include it.

As to "crucial pedigrees". I sent these, in an admittedly ~~merry~~ messy state, on quarto sheets, some months ago. They need re-drafting, with index numbers for particular cells, to fit draft, this is not done yet.

That's all about drafts, drat them, Not much news from here. We have just got over a Soc.Gen.Microbiol. meeting, at which Guy Meynell and I had a paper on use of mixtures of tagged variants of a pathogen, to see if LD50 dose of, say, 10^7 represents the situation where the average probability, p, that an inoculated organism will multiply and infect is small, here 7×10^{-8}, so that for dose of 10^7, $e^{-pn} = 0.5$, where n is LD_{50} dose. This went off O.K., though our results are not as clear-cut as we hoped. Harriett Taylor is giving 3 lectures here next week, I hope to pump her on an easy way of getting up optimal conditions for Pn. transformation, the N.Y.U. method is too tricky I think. Its a pity Hotchkiss publishes so little details of his set-up.

I would very much like to spend some time at Madison, and must thank you for your kind words and invitation. Unfortunately I can't very well leave this place, for more than a holiday, for some time anyway. Why don't you and Esther come over for a long stay, and do some work here? We have room enough really. (And incidentally if you have anyone bright who wants to come and work in London for a while, let me know. There do not seem to be as many people here as one might expect coming into this field, which is a pity when one has space and could probably raise money). I think it is time you both visited this side of the Atlantic.

Yours sincerely, Bruce

B.Stocker.

P.S. Result of an unequivocal xenum expt. Recip. = SL160 (= SW666 mouse-passaged)
Donors. SL160 given antigens +/-, -/- etc
Lysates, titres 4x10^6. Sera, anti +/-, -/- and gH, titres 10,000 or better for homologous H, <1/5 O and het. H.
Cells treated c lysates, diluted at intervals at 37°. At 120', 7mm, conc. set up c trapping drop. 100-200 motile cells from trap shot into drops c 2mm 1/100 in broth. Result. Into anti-+/-. In all cases >95% immobilised (i.e. no translational movement) in <60"
Into anti-donors H xenum. do.
Into unrelated xenum or plain broth >90% motile at 60".