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Dear Josh,

How are things at Madison? Hot enough I have no doubt, but perhaps you are away. When are you going to send us a tape letter?

What do you think of the idea of writing the abortive transduction of motility paper now? Admittedly there are a great many more things which we could do, but the prospect extends indefinitely, and I think what we know at present will make a reasonably well-rounded-off piece, for the micro-manipulation experiments I think now tie-in satisfactorily with the trails, and so confirm the original hypothesis, and I think the quantal distribution of the "gene-product" is well enough established. The main gaps are (i) whether there is abortive transduction of other characters, (ii) whether quantally distributed gene-product is in fact flagellum (or flagellum-producing granule) (iii) whether decision trail or swarm is inherent in phage-particle before absorbtion. As to (i), these are some ways that might be worth trying, but this might be a long job, and only limited theoretical importance, so I think it might be left open. As to (ii), I am not making much progress, since I can't stain flagella reproducibly; and the good electron microscope at Mill Hill has just been burnt out. Anyway, I shall continue to work on this, and may have some results later. One point in one of your letters I did not quite understand, that is the <u>immediate</u> segregation of motility when a cell with 1 "gene-product" divides; I have only managed to observe the actual moment of cell division once in such a case (and the result was the expected). Do you mean that you have actually seen several such cell divisions, or are you inferring from cases where motile cell is found to have produced one motile and one static cell when re-examined a few minutes later? As to (iii), there are a lot of experiments waiting to be done, e.g. effect of multiplicity, lysogenicity, temperature and so on, but I don't see they need be done at the moment. Furthermore, I don't think we need go into the evidence for simultaneous expression of H_1^i and H_1^b in the case of SW543, though I think this is the best clue we have on what is going on. Incidentally, we have since found that the apparently similar effect in the case of SL28 is bogus, it was due to cross-reacting anti-body in the serum used; however, I have one other strain, I think SW966, in which casual observation suggests that antibody for donor's antigen reduces number of trails but does not abolish it. The question arises of how much theory, or speculation if you like, as to mechanism, should be included. I fell inclined to mention the two main possibilities (i) that the extra piece is deficient in

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some way so it can't be incorporated, and (ii) that it is incorporated in an abnormal way, perhaps as a side-arm. Your unsuccessfull search for cells which have new latent latent H_1 antigen in progeny of abortively motilised SW543 then becomes very relevant. Do you think they should be mentioned, or reserved for later?

If you agree it should be done now, I will start on a skeleton draft, and send it you for comment. I am getting a bit bored with pedigrees and so on. We seem able to get E cells fairly regularly now; in an experiment last week of 57 motile cells picked, 6 died, 10 gave non-motiles only, 31 gave between 1 and 10 motiles and 10 gave from 20 to 60 motiles, after overnight incubation, i.e. about 1/5 E results. Droplets of a collection of motiles collected at same time and plated on gelatin-agar gave 3 trails (c. 35, 60 and 70 col colonies) from about 14 surviving cells, some of which produced small groups of colonies. The only discrepancy is that no swarms were obtained from manipulator-picked motiles, though the phage-cells mixture gave about 1 swarm per 5 trails.

Not much other news. I and Clive S. are thinking of looking into mechanisms of motility. I can now get (aggregated) flagella of swarming Proteins to show up beatifully in darkground, but have not yet had any luck with Salmonella, though it can be done according to literature. The trouble is it is so pretty, it is hard to tear oneself away to do anything else. We are just making up methykallulose solutions of viscosity about 1,000 centipoise, to keep Reynold's number right for models.

Yours sincerely,

Love to Esther.
Have you any news?
Have you been on holiday?

Bruce

B. A. D. Stocker.