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27th May, 1953.

(Copy to Norton also)

Dear Josh,

Thanks for your letter of 30th April, also for a bunch of strains, and for copy of stock list.

Paper has now gone in, further amended as you will see from copy sent you by sea mail. As soon as I get comments from J.G.M. I will ask for estimate of cost of reprints and let you both know.

I see I was wrong in thinking you did not specifically define transduction. I agree there should be a name for the general phenomenon but I think present draft leaves this open O.K. Dobzhansky's comment very bizarre.

As to species I have with some hesitation over-ridden your objection to use of the term. Reasons as follows (i) both Cowan and J. Taylor consider my usage O.K., (ii) main reason is that the point is one of nomenclature only; both current usage (here and I think in U.S. also) and current international agreement (last Int. Microbiol. Cong.) are use of binomials and at least implied specific rank. The current arguement is not about what is the correct usage, as of now, but as to what should be chosen as usage by next international agreement. I feel sure present wording is clear, which is the main consideration, and will not, I think, annoy anyone.

I have nearly come round to your feeling that "gene" suggests a naive acceptance of unit with pre-determined borders; I have worked in some "genetic factors" which logically is no better perhaps but may as you say sound better.

As to prediction, the prediction was not that all flagellar characters would be linked but that as several independent variables were to hand, there was probability that some of them might be linked. As to that I still think your original 2-step aromatic exacting mutant is better explained as result of additive (or multiplicative) effect of two mutations of very closely linked loci both controlling one character.

I also picked trails from SL13, but have found them as refractory as the parent strain. K. has written to ask for SL13 to look for $XII_2$, but have sent it, also two S. paratyphi A O strains which I can't motilise.

Am now reconciled to TM2.

Felix says the 1930 Lancet paper is the earliest mention (by him) of O.901.

Have dropped idea of MGB piece.

The reason I put in double transduction of SW553 is that I can then say that attempts to do it to other O's have failed, and thus emphasise the idea that only some Fla. loci are related to $H_1$ locus.

I see you ask for the t-m strains of measured "mutation"-rate; I will look out and send, also the few new O strains I have here. I don't think N.C.T.C. have any more. I will write to Le Minor. I don't know Schütze's paper on N.C.T.C. 3045, what's it about? I never did any more on the plating medium for H and O. There are several earlier papers on it I think. The apposition of experimental datum (= bacteriological term) and interpretation (= genetic term) was in part deliberate, aimed at enlightenment of medical, etc. bacteriologist. I have cut out a few and will leave rest to Editor J.G.M.

We evidently disagree as to definition of "mutation", but I have re-worded in non-commital way.

I have retained L. et al 1951 as ref. for lac. loci, as it avoids adding another reference.

I agree about antigen B Vi, and have re-worded.

I could not find the mkt for your note 23:14. Have I overlooked a misprint? Found it.

Your note 33. I agree, but don't know how to fix better than as at present. Have just looked up New Yorker and agree again.

SW553 included for reasons stated above.

I think that's all about the paper.

The micro-manip. experiments proceed slowly. A lot of single cells behave oddly, e.g. lysis, prolonged lag, or etc I am about to repeat with SW541-lysogenic, but the yield of motilised cells is lower. I am very pleased with success of very obvious trick to simplify isolation of motilised cells, that is an adjacent hanging drop of sterile broth brought just into contact with the drop of treated cells. Even at 37° there seems to be about 2 hours lag before invasion begins. I have now managed to pick the single motile cell from a drop with ?$10^4$ non-motiles, all progeny of 1 motile cell, and got it to repeat the process. Also now 4 instances in some 40 single motile cell isolations of the equivalent of 2 or 3 trails arising from a single cell,

J Hunt

and have confirmed their occurrence on semi-solid agar, using both SW541 and SW541 lysogenic as recipients.

More later.

Ref. for semi-solid medium for diffn. of O and H cells. Li, C. P. 1929 J Exp Med 50-245

Also if you know the expected H formula could you not do pour plates with a lot of anti H? and get haloed colonies?

28/5. Have just had letter from Cavalli asking if I will do a 40' paper on transduction, in general, at Rome. Here I shall say yes, & must now think what to say. It seems, as I take it you intend, a good place for re-iterating your definition of "Genetic Transduction" & arguing that ~~DNA-mediated~~ Pneum. & H. flu. cases are only special examples etc. Have you any suggestions as to what should be said?

Yrs Bruce