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9th April, 1953.

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Dear Josh

Many thanks for your letter and returned M.S.; I agree with a fairly high proportion of what you each suggest, and have the necessary alterations in hand. I shall do first the only bits on which there is much change to be done, i.e. cutting down introduction, and putting in progeny tests. I think it might be easier to omit double transduction of SW553 from this paper. I suppose it will go well enough in some later paper of yours. This mainly because it has been so laborious to find a form of description of the linked transduction that both accurately states our hypothesis, and is easily comprehensible to the non-genetical bacteriologist. When I have done these major alterations (incorporating your redrafts, but rewriting to some extent, partly for uniformity of style) I will send you the results, and while awaiting your comments will attend to the smaller alterations required in the rest of the paper, and if you both agree the re-written parts, and if photos and tables are by then complete I will push the whole thing in to J. Gen. Microbiol. I think there will be time enough to deal with any further points that arise from your reading of the M.S. as sent to the Journal.

A few points of detail. All semi-colons queried by you both come out, all the rest I will re-consider and probably take out most of them. (L) I, of course, agree that all 3 names should appear, and will do the acknowledgements on the lines you suggest, and for simplicity will put L.S.H. & T.M. after my name and Madison after both yours, and will work in statement that I did half my work in your laboratory into the acknowledgements.

I do not intend to say anything about the

relation of transduction to recombination, but I think we must include in introduction just a line to recall the experimentally observable differences, i.e. filtrates inactive and multiple characters from each parent. My reason for thinking this is that an intelligent but non-genetically minded person who read the M.S. less introduction (not then done) at once wanted to know what the relation between the two phenomena was, of course we don't know absolutely but I think we should have at least a memory-jogging sentence on the differences between them. I am not too keen on asking Felix's opinion on OH ~~and~~ versus H as it would look odd perhaps not to take his advice if we decided against OH. I prefer O and H which is the most common usage I think, and historically correct, though O so strongly suggests somatic antigen that OH has some attractions.

As to terminology, transduction for DNA-mediated changes would be a new usage (in Z and L no explicit definition of the term is given but the implication and your usage one would conclude it was intended only for the S. case). Theoretically also it seems to me there is a case for reserving transduction for phage-mediated cases, for then one may say the phage leads across the genes (and the character). However, I think we can hedge on this by writing in such a way as to leave the matter open, that is I will excise the verbal antithesis of transduction and transformation, but will refrain from speaking of the DNA cases as transduction. Then if you still consider that transduction should be applied to both cases you will still be able to re-define it elsewhere. While I agree that transformation does not now seem a good descriptive term it has the sanction of custom over many years.

(Z) Occurrence of spontaneous isolated deep colonies in SW545 must be mentioned unless we drop the strain altogether.

(L) I am unconvinced on flares; partial roughness may or may not be necessary condition for their formation; but an explanation of why a rough cell swims out and then produces a static colony is still needed. I agree that we can avoid detailed discussion more or less in the way you suggest. I don't understand what you say about margins, but anyway have in mind to do some experiments which may clear up situation perhaps.

(L) I will replace Glasgow O by O.901 as example of O mutant. Glasgow O is out anyway; I sent to N.C.T.C. for Glasgow H, its alleged parent, to see if it was a slow-swarmer. It is not. Glasgow O was isolated from an animal

inoculated with Glasgow H, so possibility of picking up independent carried strain arose. Felix has checked phage-types and Glasgow O and H differ. (This surprised and impressed him I think).

SW541 and SW545 turn out to be O strains isolated or received as such, so I must transfer them to the correct table. I am having great trouble getting any swarms from some of these t-m-6 strains using lysates of strains lacking i and 1, 2. If you succeeded in the past with SW541, SW544, SW548 or SW549, I would like details, to save attempted repetition.

(L) I have tried various alternatives to "species" of S., but all lead to circumlocation. I shall put in a sentence disclaiming approval (or otherwise) of current usage. I think this will cover the point O.K.

(L) What is difference between "gene" and "genetic factor"?

(L) Don't like "combinati~~onal~~orial", though a good word, since the test is "permuatative" as well. Any alternative?

(L) Sorry you don't like my suggestion about probable very close linkage of genes which can be transduced together. I think it a good hypothesis as it successfully predicted discovery of linked transductions of flagellar characters. However, will cut out or shorten and water down.

I think that's all about the draft.

J.T. is sending me anti 2 and anti 3 sera, and if tests work O.K. I will include results in tables, even though 3 is dropped from K.W. scheme, as results should strengthen case for "latent antigen". Felix is now convinced on spont. H mutants of (his old stock of) 0.901, and has taken one away to do serology, etc.

I also had long trails from t-m X SW553 (I think, have not notes here). Some were still increasing in length when I left Madison. I agree no branching ever.

As to SW970 and SW972, are you convinced that failure to transduce some Fla - results from mutation of same or linked genes? I have had such poor yields from some inter-specific combinations Fla + ~~X~~ Fla - , e.g. one swarm, no trails, from several ml. of culture spun down, that it seems difficult to exclude effect of, say, phage host-modification unless one has a Fla+ variant as control donor which of course you can't get if the Fla- donor is doubley mutated.

However, a transduction of some other factor would be a good control I take it. I have more homology tests to do than I have time for at present, but hope to do some some time. However, don't wait for me. I got no swarms from gallinarum H.901 and its i transducee, did you have any luck? Did you ever try SW543 lysate on SL13? You once said you wanted to try it. I will try it too. As you say, yield from SL13 is so low and variable that only + would be significant.

I will write to K re XII_2, etc.

Bernstein was at L.S.H. while I was in U.S., I gather he was O.K.

The transductions reported in page 3; SW971 done with lysate of LT2? I will work results into tables, if you like, but I think results with SW970 and SW972 might be held back from present paper.

I don't see much advantage in altering name of LT2, which I regard as just a strain label; but will do so if you wish. Thanks for the details of origin. Will seek out ref. on 0.901, from Felix if I can't find it otherwise.

SW588 and 534 phage type differs from that of SW703.

I agree SW552 is too rough to work with.

Ratio of linked to single transductions (i and r) of SL13 was about 1:8 as I recollect. I don't think SL18 acts consistantly on SW573, I think I will have to put in a symbol for rare, irregular, effects.

I have done one successful experiment on picking up single motilised cell. It is easy enough when drop has been hanging for a few hours, as all the non-motiles sediment. The one I got out gave a trail, but was swamped by a contaminant Bacillus. Hope to have more to tell you soon now I can make pipettes O.K.

Are you going to be at Genetics Congress? I propose to be, and thought I might read a paper on transduction of flag. characters, either bits from present paper, or if you are there and talk about this, I should have data on micro-manipulation and trails by then. Abstract must be in by end of month. If I send abstract based on present draft, will join both your names as co-authors if you both agree. Some considerations about Rome. Also I think I might do a boil-down of present paper for M.G.B., under our 3 names, for next issue, if you agree.

I am exhausted by this writing. Have type-writer now but at present too slow for words, so having this done in department.

Re to O.901, I have been using a culture Felix sent me as no ②, from old stab culture of his dated 1-2-1938., as in small experiment it seemed ? more stock than his no ①. No ② is my SL 78

Yrs Bruce

Could you give me origin of SW535 which I took no note of when at Madison? I find LT2 → SW536, its X-related O form, (to give d ↔ 1 ... quite satisfactorily so I suppose we might include it also.

The missing pages were ones on which I had crossed everything out, removed to save wt.