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29th December, 1952.

Professor J. Lederberg,
Department of Genetics,
University of Wisconsin,
Madison,
WISCONSIN, U.S.A.

Dear Josh,

Thanks for your strains which arrived today all in order. There are two or three more which I would like, at your convenience. They are:- SW 665, the Xyl - SW 541, which I would like to use in micro-manip. experiments.

SW 703 S. para B of Edwards, the alleged parent of SW 534. I did not take it with me, and I want to send it to Felix for comparison of phage type with SW 534, and SW 588 and SW 546.

SW 926, the e n x/12 type you said you were sending, but I do not find it in the parcel. Joan Taylor seems specially interested in this one (having heard about it from you).

I have two more t-m Os, new isolates from Joan Taylor (probably identical with SL 15, 18, 20) and a new O strain giving b (not yet tested for second phase), which I will send off very shortly. The b O is a good stable one.

I think I might send the S. dublin (?) O strains to William Smith, to see if they phage-type as dublin despite their second phase.

As to the paper, I have heard from Norton: I told him that I thought that all my work rested so directly on his that it would only be fair that he should be co-author, and he says he also feels that he should be co-author, and of course this is OK by me; he says he thinks the paper should be by S., Z. and L., in particular because he thinks that your progeny tests on the 543 double-transducers should be put into the paper, so as to exclude the suppressor hypothesis. I am not quite convinced that this material should go in the present paper; it might perhaps be more at home in a paper describing the H_1 and H_2 loci, which I take it you will be writing later. Even if these particular experiments do not go i in, I think, if it is to be a joint paper, perhaps your name should be over it too, (if you don't dislike the draft too much).

Norton is not too happy about my double transduction of slow and Fla in SW 548: I am not sure I follow his arguement, but there are of course a number of possible experiments bearing on the possibility of suppressor loci being involved: I have not done any of them yet, but can attend to it while writing the paper in full. N. also feels that in O x O transductions, the hypothesis of suppressor loci should at least be mentioned. I am not sure that I agree, as it involves a hypothesis which could be proved (e.g. if any motile strains failed to transform a particular O strain to motility, though able to affect it à propos other characters), but cannot be disproved (or as far as I can see), ~~but~~ and is unnecessary to account for the findings so far. Perhaps it should just be referred to in passing, to pacify the real geneticists. I would like to keep the paper relatively free of "formal" genetics, so that it remains comprehensible to ordinary bacteriologi~~cal~~sts (even medical ones) as far as possible.

N. also feels that my section X is wrong way round, he would prefer 543 data, para A. O data, slow data, hypothesis and comparisons. I am not sure that I agree. The explanation of the findings is the most difficult point in the paper to put across, and I think it may be easier to follow if the presumed explanation precedes the data, and as I came to Madison hoping to find such double transductions, I do not think it dishonest to put it the way round it is.

I hope you will let me have your views on these points, and the draft generally, soon, but I shall go on to write a full-length draft anyway, and modify it later.

Everything here has been messed up by Christmas, and I am now disorganised by having to organise two weeks of practicals, etc. on genetics for the Dip. Bact. class: have been out buying velveteen for your replica tech. When you send strains, could you also include the K 12 derivative having high rate of mutation to S^R? Also, if you have one available (for teaching only) a persistant heterozygote segregating for Lac? I can't make EMB of satisfactory quality. Do you know if the batch of Eosin Y you once recommended is still available? Meantime I am trying various adsorbtion indicators, tri-iodo-fluorescein and di-iodo-di-methyl-f are not bad, but I think a little inhibitory.

What are you thinking of doing about the results of serological tests (detailed) by Edwards (and Joan Taylor)? It seems to me that to put the whole thing on a sound basis, the full serological analysis, including absorbtion tests, (and minor absorbtion tests if done), of at least two or three of the "synthetic" strains should be published, by the serologist(s) who have made the analysis. J.T. has now done fairly full tests on some strains: she does not suffer from any desire to see her name in print but it seems to me that if Edwards, or you and Edwards, are planning to publish results of full serological analysis of a few strains, it might be a good idea to get her as co-author, so that results can be pooled. Otherwise I think the present paper should just indicate where serology has been confirmed, and perhaps indicate that results will be published elsewhere. I would be glad to know what you think of this: I would emphasise that J.T. examined the strains I sent her for fun, and for my information, not with the idea of publishing about them, but I do think that the stuff should be published by someone.

N. has also sent me details of some nice expts. he did long ago on the "transformation delay" for various biochemical makers; they seem to represent something analogous to trail to swarm delay.

Have just come across two English abstracts of German papers. Caselitz, 1951 Zeitsch. f. Hyg. u. Infekt 133-113 who apparently claimed ultra-sonic survivors of Salm. were sometimes in opposite phase and Sagebiel, 1951 Arch. f. Hyg. u. Bakt., 135-221 who says *S. typhi* loses its flagella in chloromycetin broth. I don't know if he means genotypic change; when I can get hold of someone who will translate the relevant bits I will try to find out if they are significant.

Yours sincerely,

Bruce

B.A.D. Stocker.

Best wishes for 1953 etc
to E. and you, also [illegible]
Are you reserving "camping space"
for the Genetics Congress, or will you be
in one of the various grades of hotels?