Microbiology
N. Y. U.
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Dear Josh,

Thank you for your letter, and New Year wishes, also for your Christmas card, which with others now adorns the wall of my apartment. I am most grateful to you for your most flattering invitation to spend a year at Wisconsin, but, very much to my regret, I do not think I shall be able to take advantage of it: I should probably have been able to get my fellowship from the Commonwealth Fund extended, but the difficulty is my teaching commitments in London. I got leave more or less on the understanding that I should be back for the beginning of next academic year, and I think that because of other staff movements, my department would be in some difficulty in getting my work done if I did not turn up. (I know this sounds like the delusion

of irresponsability on my part.) Thankk you also for your generous offer of a temporary appointment.

I am, of course, most anxious to see what I can of your work; I gather from Norton (who I only saw for ½ hour or so, owing to his having been unwell, & I having to go off elsewhere shortly after he looked in) that your lab. works at more or less full blast through the summer: my fellowship normally would expire in June, but I am fairly sure of getting a three months extension. If you think it a good idea, I should certainly like to spend those three months at Wisconsin: I had originally intended to spend the summer travelling round the country (I get a special allowance for travelling, and in fact must spend 2 months travelling as a condition of my fellowship.) I now realise that I am not going to get anything very great accomplished in the Pneumococcus transformation field in the time I have available, & that it might be better to go off round the country earlier, & spend more time with you, if that is feasible. I hope you will let me know what you think about this, I know that

3 months is not long enough to get anything much done but it would at least enable to pick up your techniques, & perhaps get started on something, to finish later.

I have not got any results yet on pneumo. transforming principle filtration; I have been using my time on trying to find other characters, i.e. growth requirements & fermentative characters which might be induced to give more quantitative data, i.e. something for which one can screen, so far without any results: even on the fairly complex semi-defined medium they use here it is difficult to get satisfactory growth on agar, and the cocci have an infuriating habit of dissolving & autolysing if conditions are not such as suits them: apparently noone has isolated any pneumo. phages, so there seems nothing left except antibiotic resistance. - Hotchkiss tells me that even penicillin resistance the quantitative aspects are full of doubts as to interpretation. I hate not being able to titre a thing to even a ×2 or ×½ accuracy, especially as it more or less rules out experiments on adsorption & saturation of cells, comparable to yours on Vortis or S. ty-mur. factor.

A propos flagella, I have no personal knowledge of the phenol suppression: I have been told, however, by people who run labs. making vaccines & suspensions

for agglutination tests that it is not uncommon to get a batch of nutrient broth which is satisfactory for growth but gives very poor development of H antigen, possibly this is comparable with the (reported, I have not tested it) failure of flagella to develop on a synthetic medium. If the latter phenomenon occurs it may perhaps be analogous to the low titre of nitratase a "non-essential" other constitutive enzymes of cells grown on synthetic agar medium.

Dr Packer of this dept. did some interesting (unpublished) work on swarming in Proteus which might be relevent. On synthetic agar + uracil there was good growth but no swarming, the most active constituent of complex media in restoring swarming was found to be glutamine (or its precursors).

As to the H-specific phage, Mark Adams tells me Rakieten is no longer at Brooklyn, & he does not know where he is. However, having recently heard from Asheshov of Brown Botanic Garden, that Bilgakov, (of the lab. du bacteriophage 75 Rue Olivier-de-Serres, Paris XV) who was of Thirk co-author with Sertic, was still alive & active, I wrote to him about it, & got a reply a couple of days ago in which he says he thinks he

will be able to send me the phage as soon as he has rescuscitated it. I will send it you when I get it I have always thought it a most extraordinary thing. I shall be interested to see weher it selects stable O's, or only (or mainly) unstable poorly-"flagellated" variants. I thought at one time that they might provide one side of a system in which one could measure mutation rates in each direction with an appropriate screening medium for each.

I was most interested, in fact fascinated, to hear of the experiment on transferring S. typhi-murium flagella to S. typhi. I am still brooding on the significance of the non-mutability of phase of the transformed type. If rate of mutation is controlled by a second single gene, either S typhi murium transformed to H by means of the filtrate of S. typhi (if this is possible) should readily mutate, to presumably to the second "artificial" phase of S. typhi.

I don't think I have anything else to say. Thank you again for your generous invitation.

Best wishes for 1952 to you and Esther

Yrs Sincerely

Bruce Stocker