

Medical Research Council

MICROBIAL GENETICS RESEARCH UNIT

Hammersmith Hospital,

Ducane Road,

London, W.12.

3rd December, 1957.

WH/rr

Professor J. Lederberg,
Dept. of Genetics,
Genetics Building,
School of Medicine,
The University of Wisconsin,
Madison 6,
Wisconsin, U.S.A.

Dear Josh,

I find I have on my file at least three unanswered letters from you, one from Madison and two from Australia. Please accept my most humble apologies for not writing to you before, but as you know, I had an extended holiday this year during which I ceased even to think, which has been followed by a rather busy return to reality when I have been spending all my time with architects and catalogues of equipment in connection with my new unit, which should be properly housed about a year from now.

Many thanks for the typescript of your paper on "pedigrees of zygote isolates". I have sent this on to Paris as you asked. I found it very interesting but it is too complicated to discuss in detail in this letter. I do ^{not} think, though, that there is any real evidence in favour of reciprosity among recombinants issuing from a single zygote. I do not think that this will, in fact, be found; however you have provided some evidence which might tie up normal segregation rather neatly with some of the ~~pedigrees~~ ^{pedigrees} in your diploids - I refer specifically to the problem of the homozygosity for markers in which the two parents differ.

I was interested in your work on the question of the dominance of AZ^r in your diploids and I am glad that you were able to confirm the fact that on minimal medium, AZ^r appears to be dominant, though it is interesting that the reverse should seem to hold on nutrient agar. As I have indicated, I have had no time since my return to do any bench work and I am rather keen to make a start on something quite different from K12, at least as the principle line of research. Just what this will be I am not absolutely certain, but I have a hankering after diphasic variation, an interest in which brought me into genetics in the first place. At least, thanks to you, I have something to go on.

I expect you had a wonderful time in Australia. I had a letter from Hollisway

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who seems to have greatly enjoyed and got much value from your visit. Incidentally, I was very interested to see an abstract of a paper by Allan Richter recently. It certainly looks as if when the Hfr locus is present on the chromosome that F goes to it. I am coming round to the view that F may well be a ^{cyto}plasmic factor. I am rather keen on the idea, suggested to me by Gunther Stent a long time ago, that F may really be the vegetative stage of something not unlike a defective temperate phage.

I am sorry to say that for various reasons I will not be going to Stockholm next August, but will probably meet you and I hope Esther, at Royaumont. I hope, though, that you will find it possible to visit London for sometime on your way back to America. We now have a new house and would be delighted to put you both up. Please give my best wishes to Esther.

Yours,

A handwritten signature in cursive script, appearing to read "Bill", with a long horizontal flourish underneath.

W. Hayes.