10.xii.52

My dear Bill,

I have received, and not answered for a while, your letter of the 27th November. I was very pleased to hear that the various amendments have proved possible. O.K. for the various alterations you proposed; the one on ~~the~~ table 2 is exactly as it looked, I think, on the table I sent ~~to the Editor.~~ with the manuscript which was not understood by the printer. Re your questions : (1) on page 94, (#8) refers to strain No.8 of my collection. It is obviously of no significance to the reader, except that it is the strain on which I spent the last three months and may have to refer to this point in papers later. Would the symbol # be undesrtandable to readers as meaning "strain No." ? I thought it were a customary symbol, at least in the American literature. Substituting with the longer paraphrase would mean altering all the successive lines. There would be no great harm in deleting (8) altogether, if (#8) makes no sense to most readers. (2) Reference for Davis : B.D. Davis, Studies on nutritionally deficient bacterial mutants isolated by means of penicillin, Experientia, 6:41-50 (1950). (3) Reference to my paper, 1952 : Genetic analysis of drug-resistance, Bull. World Hlth Org. 6:185-206. Did I send you an offprint ? if not, one is enclosed.

Work: I have lost the last month looking for a presumptive linkage of my F^r (F-refractary) with methionine. A preliminary test had indicated the possibility of linkage; i.e., crossing #8 which is M- with TLB_1-S^r on minimal +st+methionine, most recombinants are M- and they seemed to be mainly all F^r (at the boundary of significance). I have therefore enlarged the data conspicuo

ly, and now seen that linkage with M- does not exist. F^r is still therefore independent of all markers tested, though I must wait for some more data coming on M-, and the next step I shall try is resistance to colicine E, as this also seems independent of the other markers, and perhaps independent of F+ polarity, according to data published by Fredericq/ coupled with mine. Another waste of time has been the analysis of a new F-agent, which seems to be different from the F of M-12 in that it can infect my F^r. I have no conclusive evidence that this is true. Recombination with the new F-agent happens at a too low rate to be of real use. I am sorry, because I hoped to test whether a different F-agent would determine a different pattern of segregation, i.e. a different probability that given markers reaching the F- cell. I have still a hope that the low fertility ~~of the~~ ~~this~~ be due to instability of the F-agent.

How is you work going ? Yours ever