October 17, 1953

Dear Luca:

I had hoped to defer writing you until I had the time to go very carefully over our correspondence and set us up to date on the various interesting points that had come. Unfortunately, I must interpolate another request. I mentioned that W-583 had arrived, broken. It was not recoverable. Can you send me another W-583 as well as W-945? In fact, if it would be convenient for you to send back any other multiple-marker stocks I may have sent you before, I think they should be compared with our present stocks.

Esther and I have had no difficulty comfirming your most interesting finding on the linkage of Hfr to Gal (whether Gal_1 or Gal_4 is hardly material!) both by prototrophs from M- Hfr x TLB_1- F- Gal-, and by a pair of Gal+ S^r M- isolated from M-Hfr x M-F-Gal-S^r on EMB Gal+sm As I remember Esther's result, there were of course very few Gal+ prototrophs, about half or more of them proved to be Hfr when backcrossed to W-1177 on minimal+ sm. Only 2 Gal+S^r were isolated in the second experiment, both Hfr; this cross otherwise gives only F-. However, no evidence for the transduction of Hfr, along with Gal+ could be seen in experiments Gal+ Hfr —x Gal- F-; Gal+ F+ —x Gal- F- had been tried before, and the present results were equally negative. However, as the Gal-transduction system has lately given an especially competent sub-system, we may try some further attempts, but I am quite dubious of it. Tom Nelson had previously confirmed Hayes' finding that F+ x Hfr will give Hfr recombinants when the Hfr parent is effectively F- (viz. subjected to the aeration phenocopy, while the F+ parent is treated with streptomycin).

Hayes visited us briefly last month. He is a charming person as so many people had said. But for all of the residual difficulties with a post-zygotic story of elimination, I am surer than ever (after talking with him) that the pre-elimination idea will not work at all.

You indicated some doubts about "efficiency of transfer of B_1+ from Hfr". In Hfr M- x F- TLB_1-, on thiamin-minimal agar, I find the same ca. 10% B_1+ as with comparable F+ x F-. Are your data different? Nelson reports, however, that with Hayes' Hfr (and not ours) the addition of thiamin has a disproportionate effect in increasing the apparent count of recombinants, possibly owing to increased residual growth and plate recombination. The cross M-F- x TLB_1- F+ is giving some unusual results (almost all B_1+) not yet fully confirmed.

Helen Byers brought back some very pleasant photographs of the Congress and of yourself; we were especially pleased finally to see these.

Do not feel obliged to make an immediate reply (except for sending W-583) as I hope to write more later. Of course, I will be pleased to hear what is going on now that the burdens of the Congresses are over! Can you tell me the status of our paper on drug-resistance?

I am spending most of my time now on HfrxF- cytology, and am very discouraged. For a month I could not make again a decent Giemsa slide (!) but it looks now as if it was only the wrong pH of the buffer.