Madison, Wis.
February 12, 1952

Dear Dr. Burnet:

I was very sorry to learn that you would once again be ahable to include Madison in your itinerary of an American voyage. I shall be most pleased if there should materialize any possibilities of a future visit of longer duration.

My letter is primarily to ask whether you would be kind enough to send, or reserge for future shipment, a set of reprints of your papers with Lind on the influenza virus recombination. I was most pleased to note that you expressed some reservations about the fluid gene pool concept of virus recombination; I have (based on my own preoccupations with E. coli) always felt this to be an ad hoc hypothesis, not clearly required by any experimental result as an alternative to simple or repetitive matings.

You may be interested to learn that the premonitions indicated in your review with Fenner of the probable utility of immunogenetic studies with E. coli are being materialized. Dr. P. D. Skaar, who worked with Sonneborn for his Ph.D. has taken responsibility for this project, using the thirty odd new, interfertile and serologically distinct strains of E. coli that we have recently isolated, (about 2% of the strains tested). So far the somatic antigens have behaved like any other segregating marker. Very recently we have also gotten some first clues on the genetic factors controlling crossability, with some evidence of an incipient heterothallic system.

Curiously enough, Salmonella (typhimurium) has behaved entirely differently from E. coli. The only recombinational mechanism we have been able to find occurs quite generally among diverse strains, but contrasts sharply with the sexual behavior of E. coli. Instead, single markers are individually "transduced" by a filtrable non-cellular product, along the lines of the pneumococcus transformation. A great many mutant factors in several strains have been tested, and they all work in the same way. The agent seems to be an organized, DNAse-resistant particle about .1 micron in diameter; the particles are not gametes: they carry just one (or very few) genetic potentiality of the parent cell, so that, for example, we do <u>not</u> get recombination of unselected markers. The apparent rate of transduction is limited by the adsorption of the agent, and is not more than 1 per 10^7 cells per marker. The transductions are not at all strain-limited; probably all the XII-carrying serotypes (groups B and D) will participate. For example the serotypic #hybrid" ZZ IX, XII, i;— has been obtained several times from S. typhi treated with the transductive agent from S. typhimurium, and selected in d-antiserum agar. Mr. Zinder and I have a paper in hand on this work.

Even with the delays in surface mail, I trust that you will have received by now your copy of Papers in Microbial Genetics, and your surviving reprint of your 1936 paper with Lush on lysogenicity. Thanky you very much for your gracious cooperation on this enterprise. I used the book in my course last semester, and I think it proved quite useful to my studentsbas well as myself.

Yours sincerely,