June 17, 1948.

lr. Lois Dickinson, Department of Bacteriology, University of Manchester, Fingland.

Dear Dr. Dickinson.

Having been led to it by Werner Braun's recent review on bacterial dissociation, I have just now read your paper " The influence of substrate on the variation of Br. bronchiseptica " which appeared three years ago in the Journal of Pathology and Bucteriology. If, as you would seem to have concluded, agents can be found which have an antimutagenic effect in an otherwise "normal" environment, it would be very remarkable indeed.

Your experiments undoubtedly tend to this conclusion. Your "reconstruction experiments", in which small inocula of the V form were added to cultures predominantly N are particularly impressive. For some time now, I have been belaboring the necessity of such experiments as controls on mutagenic activity.

If your conclusions were not so remarkable, I would not raise the issue, but it seems particularly important in such a study as this to climinate every possible source of arror. On p. 291, you report that "In chloride-free medium mixtures containing up to one loopful of V did not reveal V colonies after 20 subcultures..." This would seem to mark one loopful as a critical concentration of V cells for their establishment in a culture. Later, you comment that "In maleicate medium ... the weakest mixture tested contained one loopful of V and this rapidly became pure V." From this you would conclude that the maleicate did not influence the selection dynamics involved in the establishment of the V form. I would like the suggest the importance of extending your observations to increasingly smaller inocula of the V form in mixed culture with I, in order to show that maleicate does not simply known the adverse selection which V seems to suffer in mixed cultures in chloride-from marketing -free medium.

Your comments on this discussion would be appreciated, as would a reprint of the paper.

Yours sincerely,

Joshua Lederberg Assistant Professor of Genetics.