

UNIVERSITÄT BASEL UNIVERSITY OF BASEL

INSTITUT FÜR HYGIENE UND BAKTERIOLOGIE

INSTITUTE FOR HYGIENE AND BACTERIOLOGY

BASEL (SWITZERLAND)
PETERSPLATZ 10

25th August 1952

Dr. Joshua Lederberg
Associate Professor
The University of Wisconsin
Department of Genetics
Madison, 6

Dear Dr. Lederberg,

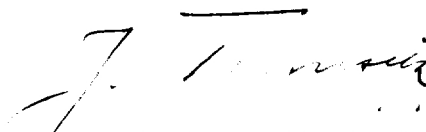
I have sent you about 1 week ago two papers summarizing my attempts on anthrax mutation and two others on the complex structure of the capsule of the "mutant" or, as we designated with an unbiased term, of Bac. M.

I started to work again on anthrax, because I could not believe the possibility of a mutation of *B. anthracis*, induced through the extracts of *B. mesentericus*, as reported by Manninger and Nogradi in the *Experientia*. As you can see I could not confirm their results. The only change I could observe in my first paper was that the anthrax strain of Manninger, which grow invariably in noncapsulated form on agar-medium under atmospheric O_2 tension, started to produce in the presence of mesentericus extract, in a certain percentage of the experiments, smooth colonies with typical capsulated bacilli. Since this strain was still slightly virulent for mice and gave a typical picture of anthrax infection and since at the time of its isolation showed no sign of motility, I closed my first paper with more or less negative results. The mucoid strains, thus obtained, were put aside in several subcultures in agar-plates during some months of the summer period. At the first replating in September, the colonies showed the same appearance, the capsulated bacilli revealed the same morphology, but they were avirulent in mice and were distinctly motile. These observations, published in the second paper could inasmuch be reconciled with the anthrax derivation of the strain, as the mucoid bacilli were agglutinated with an immune serum, produced with capsulated anthrax bacilli in rabbits to the same titer as virulent *B. anthracis* cultured in the presence of CO_2 in mucoid form. The fermentation reactions of this strain, different from any known type in the group *Bacillus* made it, however, necessary to initiate a more profound immunological study of this group, before further experiments on the "induced mutation" were reassumed. This work, published partly in the subsequent two papers lead us to unexpected observations regarding the complex nature of the bacterial capsule in our strain as well as in *B. megatherium*. In our current work we are clearing up the significance of the capsular transversal septa in the structure of the bacterial cell with some really surprising results.

Regarding the original problem, in which you are interested, I can not give you without further work a definite judgement. I acknowledge, that working with this group of bacteria, fallacies might occur, specially since I started my work with the original culture of Manninger without isolating single cells at the time I wrote the two first papers. Still I have to assume that similarly carried out "induced mutations" might lead to unexpected results. Presently we are mostly interested in a specific cell wall reaction, which we discovered in working with Bac.M. This bacterium is very sensitive toward lysozyme. If we separate through initial lysozyme activity the cell wall from the cytoplasmic membrane, we can demonstrate that the former gives a reaction with a certain polysaccharide antibody. In unstained preparations in phase contrast microscope the cell wall shows up distinctly. I hope in continuing this study we can develop some more "markers" which would make it worthwhile to reinvestigate the original problem of induced mutation.

Your fundamental work on bacterial genetics will certainly help us by planning this work. I thank you very much for sending me your recent works. I am specially glad to possess your paper on "recombination analysis of bacterial heredity" since the Cold Spring Harbor Symposia are not available in our Library.

Yours sincerely



Joseph Tomcsik, M.D.

Professor of Hygiene
and Bacteriology