Department of Genetics
University of Wisconsin
Madison 6, Wis.

July 9, 1951

Dear Dr. Inoki:

I have your letter of May 25. I regret not having replied sooner but could not owing to a business trip.

I am very pleased to hear of your work on trypanosome variation. Many of us have long suspected that the cyclical changes found in infected animals might have a bsis similar to the changes in Paramecium, but of course the possibility of natural selection of spontaneous changes has had to be ruled out. I shall look forward to reading the details of your work.

You mention that fission is not required for the development of serological changes. Does this mean that a limited time of treatment with antiserum suffices, or more strictly that the changes can actually be detected in the same treated individuals. Several examples have been found where a limited exposure of cells to an external agent resulted in influences which persisted several generations, owing to adsorbed agent; metabolic alterations, etc., —therefore this question.

Have you considered another similar problem with trypanosomes: the mechanism of formation of aparabasal forms under the influence of acridine dyes? The published work suggests strongly that this is a directed effect also, and not selection of spontaneous mutants, but the work that has been done has not really been adequate to settle the point. Since, in this case, the "plasmagene" is a visible particle, it might afford considerable advantages in the analysis of these directed effects. I have hoped to find a student who would be interested to work on this problem, but the occasion has not yet arisem, and is rather unlikely to for some time.

Under separate cover, I am sending whatever reprints are still in supply, and I will be happy to add your name to our mailing list for exchanges from time to time.

Yours sincerely,

Joshua Lederberg,
Associate Professor of Genetics