November 21, 1952
(evening)

Dear Dr. Edwards:

Your letter of November 13 arrived here while I was out of town earlier this week. I wrote to you this morning, in reply to yours of the 19th. I must have sounded rather unresponsive— having just found your letter of the 13th, I hasten to answer it.

The S. typhimurium parent of SW-672 and SW-674 (your 5743 and 5744-52) was SW-435, a nutritional mutant that has been developed some time ago (much of the work described in the ms. by Zinder and myself was done with this mutant). This accounts for the behavior on Simmons' citrate (you will find that it will do no better on Simmons' plus glucose, a test that should, in my opinion, always be run in parallel). I have offered to send you SW-435 in the past, and will do so again if you wish) it now. In addition to the nutritional characters, SW-435 is also galactose- and xylose-negative.

The S. typhi used in these experiments was, indeed, H-901. I have not explicitly tested this, but would suspect that the growth on Simmons' citrate is conditioned by a requirement for tryptophane rather than an inability to utilize citrate. Fildes showed some time ago that this requirement was reversible by "training", and more recently accepted the concept of spontaneous mutation for the reversal.

I apologize for the reversal of 680 and 681, which I noted (independently) in my last letter.

When I sent you SW-926 ("eox:1,2") a few days ago, I was somewhat fearful that this would turn out to be an artefact of mixed culture. The combination has held up, however, through several single colony isolations, so that if it is an artefact, it is serological. Has a thorough serological analysis been carried out with #157? If alternative phases have been looked for, have they included selection with absorbed anti-2 serum, as well as the anti-1,2.. complex? I envisage the possibility of constructing such monstrosities as 1,2..1,5.., if there were any point to it.

Yours sincerely,

Joshua Lederberg