March 9, 1954

Dr. Norton Zinder
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Dear Norton:

This is to acknowledge your letter of March 1 and to answer your question on the places where our presentations for Oak Ridge might overlap. The report that I sent you is pretty much of a preliminary outline of what I had in mind to say, though of course I would be relieved of the burden of much general explanation of transduction on account of your own remarks. You should, of course, feel free to draw as you please on any work that has been published to the fullest extent necessary for you to develop your own argument. I do not think either of us has to be too deeply concerned about duplication. The purpose of these discussions is to develop not a unified party line but to bring out different viewpoints, sometimes on much the same sets of facts. In my talk I am therefore planning to go into some detail about recent developments in K-12 and also to discuss those aspects of Salmonella that are touched upon in the report, but not if I can help it to restate the general and historical background of that work, for which I am relying upon you. I am also hoping to give a general sort of perspective on recombination mechanisms in general, tying together and contrasting the Salmonella and coli stories.

The single-cell studies on abortive transductions are turning out to be quite interesting. There is a pattern after all in their behavior, which I think reflects a distribution of polytenic parts rather than an irregular replication of the transduced piece.

Might I at this time suggest a reconciliation of terminology that might help to eliminate some somatic confusion in transduction. To my mind transduction has been defined to cover all of the related phenomena whether mediated by phage or by extracted DNA.

It should be remembered, however, that transduce means transfer and one should not therefore refer to the transduced cell. I see no objection to referring to the altered cell as having been transformed, the otherwise vague meaning of this term having been by this time defined by the context of the discussion.

In connection with your nutrition problem, I continue to be somewhat struck by the non-specificity of the effective primary challenge. This makes one wonder if it is not related to the general alarm reaction. Have you tried such experiments as using treatment with cold or sub-lethal doses of, for example, formaldehyde as the primary challenge two or three days

before the secondary challenge with virulent Salmonella? The response seems altogether too prompt and too nonspecific to be of the usual antibody type.

Norton, I am afraid you are up to your old tricks again, and I must admit that I could not follow many of the summary comments that you made on your transduction work. If I were not looking forward to seeing you at Oak Ridge, I would ask for a more straightforward explanation of your chloromycetin effect.

If I might summarize the way in which our prospective talks at Oak Ridge are likely to differ, it might be that you seem to be more preoccupied with the initial stages of the transduction process, the way in which the bacterial genetic material gets into the bacteriophage, while the burden of my remarks will concern the terminal stages, the implantation of this material into the recipient genotype.

Yours sincerely,

Joshua Lederberg

JL/mg