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THE CHILDREN'S HOSPITAL OF PHILADELPHIA
1740 BAINBRIDGE STREET
PHILADELPHIA 46, PA.

RESEARCH DEPARTMENT

Feb 20, 1956

Dear Salva:—

Thanks very much for reading the paper and comments. I have a little more data; the r DNA contains glucose alone (in addition to DNA) minimizing the contaminant possibility. And I got twice as much from the same amount of r DNA as from r+ DNA. Also the correlation of DNA ase resistance and glucose content is beautiful.

Concerning your remarks. I referred to Luria + Human attempting to link up DNA ase activation and nuclear degradation (albeit in too compressed a manner). It is not conceivable to me as an enzymologist that host DNA should go to smaller fragments without the action of DNA ase. DNA is almost inert to phosphatase before DNA ase action. There are smaller fragments Native formed from host DNA and there is DNA ase activated in the lung. Indirect but I would stake a quarter on it

In Sept. Potter organized a symposium in Houston in March at the Anderson Hospital on Tumors and Nucleic Acids. They would pay travel and $25 a day. I accepted since it would defray expenses in going to Berkeley (10 days of comparing r+ and r DNA in the centrifuge). The symposium looked pretty good, even as did your own. About three weeks ago I received a sheaf of documents from the business manager of the Hospital, which I now learned was part of the University of Texas. He wanted a notarized

loyalty oath as ~~part~~ prerequisite for recompense. Before I could answer Potter sent him a letter releasing us from our ~~commitments and~~ Arnold Welch said he wouldn't sign the damned thing. I of course agreed with Welch, with copies to everyone. The hospital backed down, promising to pay with other funds and apparently the symposium will go on as planned. Potter says that the symposium participants were almost unanimous in opposition and I think we had a real victory

I'm sorry that you turned down the Chairmanship because they have now turned to me. It would be a big help if you could let me know what you asked for and what they offered. We like living in our new house too and, what with other things brewing, it would have to be very attractive to make me budge.

Back to science. You allude to my remarks on cytosine viruses (p 7) as evidence against my HMC-biochemical shift hypothesis. I think this is a non-sequitur on your part based on the ~~fact~~ fact that you can still get the extreme parasitism with the T-odd cytosine viruses. However Dorothy Fraser (in the latest Microbial Genetics Bulletin) says that T3 to 32 can establish a temporary lysogeny in B. This is the difference – I don't think we'll ever see this with a T-even phage and in fact the last sentence of my review p 43 says "It is possible that conditions may be obtained in which some apparently virulent phages such as T3 or T7 which contain cytosine may be induced to show more temperate characteristics, if the conditions for their virulence can be specifically inhibited". Very astute

Best to Zella, Danny from Elaine + myself

Sincerely yours

Seymour