August 18, 1975

Robinow - Carl

Dr. Carl F. Robinow
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Dear Carl,

I had a student working with me this summer on a special project of historical research on the history of the idea that viruses might contain nucleic acids, prior to Bawden and Pirie's correction of Stanley's characterization of tobacco mosaic virus. One point that has occurred to me was the availability of the Feulgen reaction for cytochemical assay and the potentiality this afforded for a much earlier determination of the role of DNA in some virus inclusion bodies. We had some difficulty in tracing the literary history of this particular subject, but I was delighted to encounter your own contribution (Bland and Robinow, Nature, 1938). I believe you state there that Haagen, 1937, was the first to apply this technique. I wonder if you were aware of Cowdry's 1928 report (Science 68:40, 1928).

In any event, I would really be quite grateful to you if you could enlarge upon the contemporary images of that problem in the era just before and during the controversy generated by Wendell Stanley's isolation of TMV.

Of course, before viruses could be purified, and lacking the more energetic pursuit of the cytochemical approach, one could hardly do much more than speculate about the chemical composition of virus particles. I am nevertheless interested in tracing the history of the idea that they <u>might</u> contain nucleic acids and have not really been able to get a clear picture on that point. Wendell Stanley, of course, dismissed it totally and gives no corresponding references from the period of his own work, until he was pressed by the corrections of Bawden and Pirie. I was interested to discover that Sanfelice (1914, Zeitschrift für Hygiene 76:257) already made allusions about nuclear protein and even seemed to imply, though with little evidence, that the pure nuclear protein was infective. I have been unable to discover the milieu in which such observations were made nor how seriously they may have been taken by his contemporaries, and I wonder if you have any sources or recollections on that point.

While we are on that general theme, could you remind me who deserves priority for the application of the Feulgen reaction in bacterial cytology. Was it Pietschmann 1932 as cited in your addendum to Dubos's book? Of course, the challenge here is quite different since the demonstration of DNA in bacteria poses none of the problems of purification of source material that is reflected in the characterization of the virus particle.

These matters and even many more recent ones are fast slipping into historical mythology, and I would certainly be grateful to you for a discussion of them based upon your own specific recollections and perhaps any other correspondence or other documentation that might help to fill out the story.

Sincerely yours,

Joshua Lederberg
Professor of Genetics

JL/rr
Enclosure