Dear Doctor Neal:

I was very glad to have your letter and hope you will forgive me this delay in answering it. I have been devilishly busy and wanted to give the matter more thought than I had time for when your letter came.

To begin with, I am sincerely sorry that the footnote to which you refer in my paper with Doctor Wright is capable of the interpretation which you have apparently, and not unjustly, given it. The wording is undoubtedly ambiguous, but I want to state that the "us" referred to the departments of Bacteriology and Surgery and the hospital division, rather than to Doctor Wright and me; the work came from departments headed by Doctors Park and Wright, and I worked in both of them. The case was intensively studied during life and after his tragic death we had every intention, to say nothing of the extraordinary personal interest, to complete the investigation with the post-mortem material. That we had every reason, as well as right, if not sole right, to investigate this case can hardly be denied. As you may know, the material which I received from Doctor Park was not the only material available to us.

You say in your letter that "Dr. Holden isolated the virus first, etc.", and that you would like to know my attitude in this whole matter. It is perhaps incorrect to say that Doctor Holden isolated the virus first for several reasons. Actually we each independently and simultaneously isolated a virus from the tissue of the same case, but if one were to read Doctors Gay and Holden's reports and my reports, one would gather that not only were there different clinical and pathological entities being described, but it would also have to be proven that the two viruses are the same. (I think they are the same, and there is now sufficient evidence that it is not herpes, as claimed by Gay and Holden; Perdrau has been making a comparative study of the "B" and "W" viruses.) In my talks with Doctor Holden, I told her of the visceral lesions in the patient and experimental animals and of the conception that the case represented a unique clinical and pathological entity caused by a virus which was most likely not herpes, and even suggested some form of joint or simultaneous communication. The only answer to this suggestion was an early preliminary report in the Proceedings of the Society for Experimental Biology and Medicine. In their reports, the case was incompletely described clinically as an encephalitis, pathologically as "acute disseminated encephalomyelitis", and etiologically as herpes -- and none of these assertions, as you now know, is correct. The pathological diagnosis was incorrect even on the basis of the description which was given since it was distinctly stated that no peri-
vascular demyelination was present, and that feature is the outstanding
and most important characteristic of acute disseminated encephalomyelitis.
That the case represents a unique, hitherto undescribed, clinical and
pathological entity in which the skin lesions, the necrosis of regional
lymph nodes and viscera, as well as the C.N.S. involvement, form a part
of a single picture and are caused by a virus, with hitherto undescribed
biologic and immunologic properties, is a conclusion which one is led to
after a study of all the data. You will find that I have always referred
to the publications of Gay and Holden, and wherever possible attempted to
integrate their data with my own.

Please believe me, Doctor Neal, that I am far more
interest in uncovering a little more of the truth of certain diseases and
their causes, than in claiming credit for what I do. I have no illus-
sions about my meagre accomplishments. I regret very much that I did not
have the opportunity of discussing this matter with you personally in the
past, and hope that we may do so some time in the future.

Sincerely yours,

Albert B. Sabin.

P.S. I am mailing the reprints you requested under separate cover and
if there are any others you would like to have, please let me
know.

A.B.S.

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