Dear Dr. Verlinde:

This is in reply to your letters of October 4 and November 26 - for both of which I am most grateful. You have accumulated a great deal of interesting information and I certainly have no objections whatever to your publication of the results of this study at the earliest possible moment. As a matter of fact I think that it would be highly desirable. Since the details that you will publish represent data of a type that I have been accumulating for a long time but have not yet published in this detail, I would appreciate it if you could make reference to the results that I have shown you and transmitted to you as a personal communication. Some of your results are at variance with mine and I believe that there, undoubtedly, is a very logical explanation. I have in mind, for example, the large number of individuals with preexisting antibody who showed an antibody rise without evidence of excretion of virus in the stools. In very extensive studies in very large numbers of individuals I have never found this to occur. Whether the explanation is that my methods for detecting virus in the stools are more sensitive than the ones that you use I do not know. Also since the incidence of reinfections based on increase in antibody titer is very much higher in your groups than in those that I had studied I wonder if you find any difference when you separate the children from the adults.

In the presentation of the antibody response, the levels of antibody achieved are also distinctly lower than those that I have found especially in children. Again I wonder if it may be of interest to analyze separately the results obtained in children and the results obtained in adults. I sent you a table with data of high and low avidity antibody some time ago. Here again the question of your technique of the virus neutralization test may perhaps influence the levels of antibody that are demonstrable.

Your observations on the neurotropism of excreted virus are entirely similar to those that I have reported in the paper published in the July 1957 issue of the J. A. M. A. except where you say that "none of the type 1 and none of the type 2 strains showed any increase in neurotropism." You will recall that I was able to demonstrate an increase in neurotropism in certain instances with all 3 types. Your findings with the type 3 strains are identical to those that I reported in the paper mentioned above and I hope that when these data are reported reference to it may be made. I also hope
that when you report your data you will, of course, indicate not only that the test was carried out with undiluted or 10⁻¹ dilution of tissue culture but the actual number of TCD₅₀ that were inoculated. Of particular interest to me were the observations mentioned in the third paragraph of your letter of November 26th in which you find that when virus particles of increased neurotropism can be demonstrated by the methods used in a stool obtained at one time after infection that subsequent stools do not yield the same results. I have found the same thing on several occasions and refer to it in my publications as indicating that the alimentary tract does not selectively favor the overgrowth of virus particles with greater neurotropism. I think that this is a very important point and I was very interested to see that you were able to observe the same phenomenon on 3 different occasions. I think that it is worthy of special emphasis when you write your report.

I was also very much interested in your data regarding the ECHO 9 strains. Infections. I presume that when you speak of the pathogenicity of the ECHO 9 strains for mice you have reference to the viruses passaged in tissue culture rather than the original material derived from patients. Dr. Eggers in my laboratory has recently completed an interesting quantitative study on the mouse pathogenic property of the ECHO 9 strains in original material as well as in that passaged in tissue culture. These results show that even after tissue passage only doses in excess of 10⁵ or 10⁶ TCD₅₀ are paralytogenic in mice. There was not a single instance in which the amount of virus present in the original material was in this range and accordingly also there was not a single instance in which the original material produced paralysis in newborn mice. The committee on ECHO viruses, which has recently been reorganized into a committee on enteroviruses (including also Dr. Dalldorf), has decided to leave ECHO 9 where it is and not transfer it to the Coxsackie group - a publication on this will appear in the December issue of the American Journal of Public Health.

I hope you may have no objection to my mentioning the results of your tests for neurotropism of excreted strains to other people who are interested in this subject in this country. If it is at all possible I would appreciate it if you could send me a full protocol of the actual tests performed indicating the amount of virus inoculated, the numbers of monkeys used, time after ingestion of virus from which the specimens were derived, as well as route of inoculation. I hope that this detailed information will be contained in your report and if so, perhaps I might have a copy of your report prior to publication.

With best wishes and kindest regards.

Sincerely yours,

Albert B. Sabin, M. D.