Dr. Albert B. Sabin,
Children's Hospital Research Foundation,
Elland Avenue and Bethesda,
Cincinnati 29, Ohio.

Dear Dr. Sabin,

Thank you very much for your letter of September 30th. Please accept my sincere apologies for not having answered this letter before. In your letter you put a number of questions concerning the work which Professor Stuart-Harris and I had done in Sheffield. Your comments, I would like to say straight away, were most penetrating and apposite. Many of the points which you brought up were of course given our immediate attention and the answer to many of them you will find in our publication of November 15th in the British Medical Journal. I trust that by now you will have had an opportunity of studying the results in full and this should give you a much clearer result of the work than I have been able to present hitherto.

If I may deal with the points that you raised in your letter in turn:—
I value very much the information which you were able to send me about the result of intracerebral tests on viruses from stools of vaccines in your, Verlinde's and Smorodintsev's experiments. I do not think that there is a great deal of difference in our results. It is possibly a case of looking for different things and thereby finding different things in the end result. I believe that we examined more samples of late excreted virus than had been studied, for example by Verlinde. We also worked entirely with the intracerebral test whereas Verlinde worked largely with the intraspinal test. If we had done our experiment exactly as Verlinde had done then we might perhaps have had the same results as he did. However, we chose to look for virus in a slightly different way and our results are slightly different.

I think the point about the culture fluid which was used to inoculate the monkeys has been clarified sufficiently in the paper to indicate exactly what we used. In summary, I would merely add that we always used first culture fluid but in some instance where, for example, four culture tubes were inoculated with stool extract and all four culture tubes showed degeneration we may have used only two culture tubes for material to inoculate into monkeys. In each instance, so far as I know, it was the rule to collect material for monkey inoculation from cultures which received the highest concentration of faecal material as inoculum; the higher dilutions from the titrations of faecal virus were not used. The tissue culture titres of this material is given in some detail in the paper and I think that no further comment is required. Stuart-
Harris and I are convinced that no wild Type 3 virus was introduced among the children in this test. The conditions of segregation of this unit were such as to make this virtually impossible. Moreover, the division of the children into two groups for inoculation, the failure to find virus except after feeding and the exhaustive search for virus in stool samples collected for several weeks before-hand and amongst the contact children subsequently, makes it extremely unlikely that a wild Type 3 virus was concerned. I certainly agree that more work of this type is required in the future and I hope that both of us will be able to contribute by way of future study of this particular strain. However, at the moment, my personal preference for the next immediate experiment would be to carry out a similar investigation using your Type 1 and possibly also your Type 2 strain. It is a possibility that the Type 3 strain is the least stable of the three that you have selected and that if we use the Type 1 strain in a similar way we may get completely different results.

I would be very much interested to learn what you have found on intracerebral testing of viruses from stools of children receiving Type 3 virus after Salk vaccine as well as from contacts.

I would finally like to add my personal opinion that the results that we obtained do not in any way contribute evidence that the use of this strain in trials should be abandoned.

Turning now to the Herpes and B virus question, we have just completed our first set of tests on material from monkeys hyper-immunised with two strains of B virus, one of which was isolated by Dr. Wood from monkey kidney tissue culture and the other the strain that you sent me from rabbit kidney cultures and with Herpes simplex virus from rabbit kidney cultures which you likewise sent to me. Our results on cross neutralization tests show substantially the same as you had reported to me, namely, that antisera against B virus neutralized both itself and Herpes virus whereas antiserum against Herpes virus did not neutralize either strain of B virus. We have also completed a survey of antibody in human sera and here we find that those individuals with a history of recurrent labial herpes do produce low levels, up to 1-16 say, of neutralizing antibody to B virus, whereas individuals without Herpes antibody and without history of Herpes infection do not have any antibodies to B virus. This study has also confirmed the identity of the two strains of B virus and has also shown a good agreement between complement fixing antibody and neutralizing antibody to B virus in both animal and human sera.

With kindest personal regards for Christmas and the New Year to yourself and your family.

Yours sincerely,

Alan P. Goffe