Your letter of January 23 reached me on January 30 and your manuscript arrived the very next day. I read it with great interest and would have forwarded it directly to the British Medical Journal except that I thought your present way of dealing with the reported cases of poliomyelitis during the vaccination campaign and immediately thereafter might be misleading and needed some clarification. It was not clear to me, for example, whether when you were speaking of cases of poliomyelitis you referred to those that had been confirmed or were clinically compatible with the diagnosis of poliomyelitis, or to those that were merely reported as suspect cases and were definitely not poliomyelitis as, for example, the 3 cases reported after July 1 that we went over together during my visit in your laboratory. Is it still, for example, true that there had not been a single confirmed case of paralytic poliomyelitis after July 1 - if so, I think a statement to that effect would appropriately fit after the second line on page 26. In general, I think it would be very confusing and it would give rise to considerable misinterpretation of what happened in Hungary if you did not distinguish between cases reported as "suspect", cases that were not confirmed clinically, cases of isolated facial paralysis, etc. You must realize that this would be the first report of the Hungarian vaccination experience that would reach the outside world and failure to deal more clearly with the cases that occurred during the campaign and shortly thereafter can very well have undesirable consequences.

My suggestion would be that they be dealt with somewhat more extensively under a separate heading of: "Viruses Recovered from Suspect Cases during and after the Vaccination Program." It would be important to point out that the vaccination program was begun as one of the most severe epidemics of poliomyelitis was coming to an end and that
cases were still occurring in various parts of the country, for example, on page 17 in reading about the "30 poliomyelitis cases" which occurred between 15 December and 1 March, I would have wanted to know whether they were all clinically confirmed and, if not, I would want to see the data separately for those that were confirmed and those that were not confirmed.

In the DISCUSSION on page 26 you state that type 3 strains were isolated from some cases that had been fed only the type 1 vaccine. Yet on page 17 (at the very bottom) one obtains the impression that the type 3 strains were recovered from "cases" only during the second half of January and in February, which is presumably after the feeding of the type 3 vaccine. Similarly, one would like to know a little more about whether the only type 2 strain that was recovered from a "case" late in February was from a clinically confirmed case, whether it occurred in a person who received the vaccine and if so how long after vaccination, and finally whether or not this was a case of type 2 paralytic poliomyelitis or whether it was some other disease with merely excretion of type 2 poliovirus.

On page 18 one would like to know whether the few T^- and T^+ strains that were recovered from "cases" were from patients with clinically confirmed paralytic poliomyelitis or with some other disease? And if it was from a clinically confirmed case, was there any evidence of concurrent infection with more than one type of poliovirus, so that the virus that was isolated may have been ingested by an individual who might have been previously infected by another type of poliovirus. Furthermore, one would also like to know whether these strains were recovered from persons who had received vaccine, and especially the type of virus that was isolated, or from persons who had not received vaccine. Then, again, on page 18 you speak of "partially purified data" on "cases" that occurred from 1 March to 31 December. Here again, I think the expression "partially purified" is not very clear and the results as they are presented give no indication of how many of these cases were clinically paralytic poliomyelitis rather than something else, or facial paralysis etc. I think this is particularly pertinent in the only patient from whom both type 2 and type 3 poliovirus was recovered. Here, I think it would be important to mention the age of the patient and to point out that there was a third campaign of vaccination with trivalent vaccine in May and June for children who were born after December, 1959. A statement about this campaign is also necessary for a proper appreciation of the results obtained in tests for virus on specimens collected during the summer and in October.
I have spent so much time in raising various questions about your virologic studies on reported cases because a proper analysis of the surveillance of reported cases during a vaccination program is of the utmost importance - and a mere report of viral isolation without the other important associated clinical data can confuse rather than elucidate the problem.

I am sending you herewith a copy of the report that I just finished of our studies in connection with the Cincinnati vaccine program, and I would particularly like to direct your attention to the section on clinical and virologic surveillance.

On page 19 you speak of the two strains that were recovered in August from healthy children as being T-, while in your letter you stated that they were T-"d". If you have tested the d character I think it would be important to also give it as T-"d" in your manuscript. Furthermore, I think it would be important also to know whether these two strains were recovered from children who had no vaccine at all, or from infants who received trivalent vaccine in May and June, or from children who received the 3 types of vaccine separately from December to February.

On page 22 you quote Drozdov et al. to make the point that excretion of type 3 virus "was hardly hindered by the naturally acquired immunity." I would like to refer you to my 1957 J. A. M. A. paper (your reference 25) in which I point out, on page 1216, that 50% of young adults with naturally acquired high avidity antibody were reinfected and that this was much higher than was observed with the other types.

In your SUMMARY on page 27 and page 28 the impression is conveyed that the excretion rate was the same or about the same when the 3 types were fed as a trivalent vaccine as when they were fed separately. While this is indeed the results that you obtained it should, nevertheless, be pointed out that these results cannot be interpreted as meaning the trivalent vaccine is as effective in triple-negatives or double-negatives as monovalent vaccine fed separately. Your data are mass data on a group with probably very few triple or double-negatives, and the single limited excretion counts statistically as much as prolonged excretion over a period of weeks. The data are, therefore, not designed to indicate the capacity for multiplication of the individual types in various categories of individuals when the 3 types of vaccine are fed together or separately.
When you have made the necessary clarifying statements I think that you might send the paper directly to the Editor of the British Medical Journal. I am sending you herewith a letter that I wrote to the Editor that you may enclose with your manuscript. However if, for any reason, you would like for me to send the altered manuscript to the B. M. J., return it to me and I shall be glad to do so.

With all good wishes and kindest personal regards.

Sincerely yours,

Albert B. Sabin, M. D.

ABS:meh

P. S. I am also sending you a copy of the chapter on the "Reproductive Capacity of Polioviruses of Diverse Origins at Various Temperatures" which is being published this month in Volume II of "Perspectives in Virology."

Enclosures -
Copy of Cincinnati report.
MS - "Reproductive Capacity, etc.".
Reprint of 1957 J. A. M. A. paper.
" " 1959 " "
" " 1960 " "
Letter to Dr. H. A. Clegg, The Editor, British Medical Journal.