Dear Doctor:

The attached manuscript by: Gordon C. Brown and John A. Napier
entitled: **SEROLOGIC STATUS OF CHILDREN FOUR YEARS AFTER POLIOVIRALIS
FIELD TRIAL.**

consists of 14 pages of text and references, 3 tables and 0 figures.

Would you be good enough to review it and return your comments in DUPLICATE as an anonymous memorandum to be transmitted to the author. (The third copy may be retained for your own files). Please use an additional sheet if necessary.

If you cannot referee the manuscript in the next 10-14 days, will you return it for referral to someone else?

Sincerely yours,

JOHN Y. SUGG
Editor-in-Chief

COMMENT: Referee #

The most significant portion of the data in this communication in part indicates and in part suggests the very extensive amount of subclinical infection with polioviruses that has occurred in the study group over a period of 4 years. As the authors point out, this fact makes it impossible properly to evaluate the persistence of vaccine-induced antibody - and yet there are many statements about the effects of vaccination in the body of the paper and especially in the "Summary and Conclusions" which disregard this recognized difficulty. The order and manner in which the data are presented made it difficult for me to obtain a clear idea of what conclusions could justifiably be drawn and I had to read the paper several times and rearrange the data myself before I was able to find the real meaning of this communication.

In my opinion, the data in this communication should be on record but I would advise revision of the manuscript as a condition of publication. I believe that the following clarifications and revisions would be very helpful:

1) Under "Methods" - the meaning of antibody titers should be clarified by indicating the volume of serum employed, whether the titer has reference to dilution of serum before or after addition of virus and cells, the time and temperature of incubation of the serum-virus mixtures. It is also important to know the kind of neutralization test that was used in establishing the triple-negative status of the
children before the field trial and shortly thereafter (Table 3). The
type of test used is also important in evaluating the significance of
the discussion on page 10 relative to rise or fall of antibody titers,
unless of course the authors were able to obtain the original 1954
sera and test them simultaneously with those of 1958 - some state-
ment about this is indicated.

2) Under "Results" - it would be most helpful to present and analyze first
the data in Table 3 which give some indication of the extent of spontaneous
natural immunization during the 4-year period. This should be accom-
panied by tables in which the antibody titers of individual children, given
only in the discussion on page 10, are presented and analyzed. This
would be helpful even if it turns out that the sera were not actually
compared in a simultaneous test, and even if they were tested by one
method in 1954 and by another method in 1958. After this may follow
the data, now in Tables 1 and 2. The geometric mean titers, seem to
me, to have very little significance, particularly in view of the exten-
sive subclinical immunization, and could be deleted without loss. There
is a "typing" error now in Table 1 under group No. 19 relative to number
of vaccine injections.

3) The present "Summary and Conclusions" needs to be rewritten to give
some idea of the actual data (giving figures, etc.) especially on the
extent of subclinical immunization.

4) I apologize for the pencilled comments and queries on the manuscript,
but I hope that the authors may take them in consideration in revision.