Major Kenneth F. Burns, VC  
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Dear Major Burns:

I regret to have delayed so long in replying to your letter of 16 February 1950, which was accompanied by a copy of your manuscript entitled, "Immunological Relationship between the Viruses of Japanese B Encephalitis and Japanese B Equine Encephalomyelitis." I have been very busy in the laboratory and have had almost no time for correspondence recently.

Referring to the galley proof of the papers which have recently appeared in the AMERICAN JOURNAL OF HYGIENE, you say, "May I hasten to advise you that the paragraph reading 'It should be pointed out here that no placental transfer of antibody occurs among pigs ...' is questionable as to its scientific basis in fact." I am sure that if you had had an opportunity to consult certain standard textbooks on immunology, you would not have made this statement. (You undoubtedly miss a good library in Tokyo.) It has been abundantly proved that there is no placental transfer of antibody in pigs, that whatever immunity is transferred from pigs to the offspring is through the colostrum, and that the duration of such immunity is of relatively short duration, being almost entirely gone by the third month. Although considerable work has been done along these lines with reference to various infections, the two most clean-cut papers dealing with this subject are perhaps those by John B. Nelson dealing with an experimental study of the transmission of vaccinal immunity in swine. For your information, the two papers are (1) The Maternal Transmission of Vaccinal Immunity in Swine, JOURNAL OF EXPERIMENTAL MEDICINE, 1932, 56, 835 and (2) The Duration of Active Immunity in the Sow and of Passive Immunity in the Young, JOURNAL OF EXPERIMENTAL MEDICINE, 1934, 60, 287. It is a long established fact in immunology that depending on the histological structure of the placenta, antibody is transmitted through the placenta to the young in some species and not in others. In hog cholera, the evidence also indicates that the transfer of immunity is not placental but by colostrum. This question is of importance in Japanese B encephalitis, since the presence of high titer antibody in pigs of six months of age would, therefore, indicate that that antibody was there as a result of acquired infection rather than of passively transmitted antibody through the colostrum. You confused in your mind the difference between placental transfer and colostrum transfer.
With regard to the second paragraph in your letter, I should like to point out that the presence of complement-fixing antibodies in a certain proportion of horses and of swine at any one time has also been demonstrated during non-epidemic and non-epizootic years in Japan and Korea, and it is already well established that the virus can be disseminated among animals at a time when it is apparently not disseminated among human beings. Therefore, the demonstration of complement-fixing antibodies in a certain proportion of horses and swine in 1949 is no different from that which occurred in 1946 or other times, and in itself, is no evidence that Japanese B encephalitis was being disseminated among human beings in 1949.

In this morning's mail, I received a report from Dr. I. S. Yun, who is both Professor of Pathology and Dean of the Graduate School, of the National University in Seoul, Korea, regarding the work carried out on the 1949 epidemic. I expect to circulate copies of that letter and my comments thereon in the very near future. However, there are certain things in there which make me even more dubious of the status of the work in Korea during that epidemic and especially his report that the Korean investigators have isolated the virus from the cerebrospinal fluid and heart blood. I should like to caution you to handle the Korean material with great care.

I am returning herewith the copy of the paper you were good enough to send me. While it represents a good deal of routine work, in my opinion it contains no new information and is merely a record in great detail of what many of us have done before in the routine identification of a recovered strain of virus. If this paper were submitted to me as an editor, I would not accept it for publication in its present form. I think a brief note, not more than one or two paragraphs, indicating that several additional strains of virus recovered from horses were identified as Japanese B, might be in order as a short note in a journal such as the PROCEEDINGS OF THE SOCIETY FOR EXPERIMENTAL BIOLOGY AND MEDICINE, or one of the veterinary journals.

I should like you to know that I have great admiration for the work you have done, for your industry and ability, and that you have all of my good wishes for continued success.

Sincerely yours,

Albert B. Sabin, M. D.