The data reported in this communication should be on record because of the emerging importance of VEE as a disease of human beings. The frequency of typographical errors suggests that the authors did not pay sufficient attention to the final manuscript, and they should be asked to go over it again to correct other possible errors.

The poor results obtained in neutralization tests in BHK cells, throws some doubt on the significance of the negative results reported by them. The neutralization results in Table 2 need elucidation: a) actual number of TCD_{50} in tests with KB cells as measured in KB cells, b) the test used in mice was not described in materials and methods - was it intracerebral or intraabdominal - what does "log neutralization index" mean? - is it log 10 TCD_{50} of virus neutralized by heated undiluted serum or is it log 10 of the 50% serum dilution endpoint versus 100 LD_{50} of virus? The fact that only 9 of 29 sera of human beings with a clinical diagnosis of VEE during an epidemic period had neutralizing antibodies and only 4/29 had HI antibodies (Table 6) may be as much a reflection on the technique of the tests as on the clinical diagnosis. It deserves more comment than the authors have given.

Table 8 is supposed to list all the previously reported isolations of VEE from mosquitoes but it makes no mention of Aedes taeniorhynchus, the mosquito most frequently found to be infected in the present study. Yet, the authors state that only their single isolate from Aedes scapularis constitutes the only previously unreported isolation from mosquitoes. How about Anophiles aquasalis and Psorophora connis, which yielded virus, and are not mentioned in Table 8? Moreover, the mere isolation of virus from a certain species of mosquitoes does not constitute evidence that it is a vector!

Recommend publication after the authors have made the appropriate corrections in the manuscript.