Author's response to reviews

Title: A critical review of the epidemiology of hepatitis E virus in Africa

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Author's response to reviews: see over
Minor Essential Revisions:

1- The authors reported some findings which are needed to be discussed in “Discussion”:

- Page 7, line 9: “The seroprevalence seems to be higher in pregnant women than in the general population in Ghana (28.7% [78] vs. 4.6% [76]) and also in Gabon (14.2% [73] vs. 0% [72])”. What could be the explanation of higher prevalence in pregnant women compared to general population? Fatality rate is expected to be higher in pregnant women but how the higher prevalence in pregnant women could be explained?

- Interesting findings has been reported in “Co-infection with other infectious diseases” section but it has not been discussed in “Discussion”.

=> We added one sentence in the discussion section to address the comments.

2- Page 8, bottom of the page: “However, assuming that hepatitis outbreaks characterized by acute jaundice and a high CFR among pregnant women were likely due to HEV, Teo identified earlier, probable HEV outbreaks in Tunisia from 1950 to 1953, Algeria from 1952 to 1956, Congo in 1958, Morocco from 1958 to 60, and Libya from 1968 to 1970 and also in 1975 [23]”. Stronger evidence is needed to support this statement. This statement has been repeated in the beginning of discussion as well.

=> We think it is reasonable to assume that early hepatitis outbreaks that have no known cause, but showed high mortality among pregnant women are "probable" HEV outbreaks. Details appear in the cited article below.


3- Figure 2 is confusing. Is this figure supported by table 5? If it is the case, some of the studies in table 5 assessed only sporadic cases (some assessed a single case) which are not representative. Is the data coming from studies assessing the distribution of different genotypes in a population with a reasonable size? If this is the case, a pie chart for each country showing the prevalence of each genotype is preferred. For example look at two following papers mapping the distribution of HCV genotypes in different countries:


Figure 2 is supported by Table 5. Although only a small number of serum or fecal samples have been genotyped, we believe that these data are the best we can get. There is only one case of genotype 3 has been reported in Egypt. However, we believe that it is safer to assume that there may be genotype 3 in Egypt than otherwise. Likewise, although one sample of genotype 1 has been reported in Central African Republic (CAR), it should be reasonable to assume that genotype 1 may be circulating in CAR because genotype 1 is prevalent in other countries. Finally, we do not believe that we have enough data to get useful insights by analyzing the genotype distribution as in the references the reviewer mentioned above.

Discretionary Revisions:

1- In terms of prevalence of HEV, The authors could consider excluding the studies with a small sample size (for example n<50) given the prevalence rate reported from a sample size has low external validity, even when there is no selection bias.

=> All included studies have sample sizes larger than 30, which we believe is acceptable.

2- The authors could consider reporting 95% CI for each prevalence reported in table 1.

=> We followed the ways in which the original articles presented their results: most of the articles do not report confidence intervals although we think that it should be possible to include the confidence intervals.