Reviewer’s report

Title: Comparison of hepatitis E virus prevalenceseroprevalence between HBsAg-positive population and healthy controls in Shandong province, China

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Reviewer: Sébastien Lhomme

Reviewer's report:

The paper written by Zhang et al. interested in HEV antibody prevalence in people living in 2 different counties of China. This is an interesting work but the authors should clarify the results both in the abstract and in the text.

Background

Line 54: The word emerging is incorrect: HEV did not emerge in the past few years, HEV infections are just now better diagnosed.

Line 61-62: it is incorrect to write that « in immunosuppressed persons […] HEV might induce sever disease ». It is possible but in immunocompromised patients, the major concern is the development of a chronic infection.

Line 66: « Prevalence of hepatitis E virus IgM antbodies […]suggested a relatively higher risk of transmission of hepatitis E through transfusion in these countries ». Instead of citing papers about HEV IgM antibodies, the authors should mention papers dealing with HEV RNA prevalence in blood donations. In addition, factors influencing the transfusion transmitted HEV infections remain to be determined, especially the infectious dose.

Results

This section is hard to read and should be improved and shortened if possible.

- For instance, Chi-2 values are useless and should be removed.
- The authors presented mean± SEM but also mentioned the range. They have to choose between Mean± SEM or median [range].

Line 149, the authors could write for instance « Among HBs Ag-positive patients, the positive rate of anti HEV IgG was 9.16% in Zhangqiu and was lower in comparison with IgG prevalence in Rushan (38.06%, p<0.001) » instead of using every time respectively.
The authors mentioned in the Methods that they examined the liver by ultrasound. Did they see any correlation between IgG against HEV and a higher liver stiffness? Is the liver stiffness higher in HBsAg+ patients who were previously infected with HEV? It would be interesting to mention that in the result section.

Discussion

The authors mentioned once again the implication of blood transfusion in HEV epidemiology (line 215-217). It is unclear why they mentioned this. If HBs Ag+ patients are more frequently transfused, the authors should mention that in the introduction. The authors could discuss the main source of HEV contamination in these Province.

Line 222 : superinfection should be define here instead of line 229-231.

Conclusion

It would be interesting to expand the conclusion « the CHB patient living in HEV hyperendemic areas are at high risk of HEV HBV superinfection » what is the consequence of HEV superinfection for AgHBs carriers? This could be discussed.

Table 1: the authors should precise what the numbers are in columns. We guess it is number and (%) but this should be mentioned. X2 value are unless: the authors could mention which test they used in the legend instead.

Figure 2: Is the difference in Rushan for 60 and 70 statistically significant? Usually, anti HEV-IgG prevalence increase with age. May be the authors could try to explain this result or at least discuss it.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

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