

Dear Editor,

Thank you for the reviewer's comments on our article. We have revised the manuscript according to the recommendations. Please see our answers below.

Reviewer #1:

Scientific Quality: Grade C (Good)

Language Quality: Grade B (Minor language polishing)

Conclusion: Minor revision

Specific Comments to Authors: This is an interesting manuscript describing the prevalence of antibodies to HEV in different populations in Croatia. It is generally well-written.

There is 1 particularly misleading claim on page 5 - HEV is not the leading cause of viral hepatitis (Hep B is first, then Hep C). This needs to be re-written.

Page 5, lines 4-5: this statement has been rewritten.

The discussion is very long and goes into a lot of detail about other studies. I think this should be revised and there should be more discussion about the implications for Croatia.

Discussion has been shortened. Implications for Croatia have been added (page 17, Conclusions, lines 2-7).

The authors describe a significant difference in shellfish consumption - this looks like it shellfish consumption is inversely proportional to seroprevalence (Table 4). Can you please comment on this?

Comments regarding the shellfish consumption have been added (page 17, lines 5-9).

Reviewer #2:

Scientific Quality: Grade C (Good)

Language Quality: Grade C (A great deal of language polishing)

Conclusion: Minor revision

Specific Comments to Authors: This study investigated the seroprevalence rates of HEV IgG in different populations. Their results are comprehensive and have great importance.

In this study, the authors used HEV IgG positivity to represent the epidemiology in Croatia.

Can the results of HEV IgM positivity and HEV RNA test results be obtained and added?

The results of HEV IgM have been added (page 8, Results, line 8). None of the participants were IgM positive and reported no symptoms of acute hepatitis or a recent febrile disease (page 6, Materials and methods, lines 8-9). It was not expected to detect acute viral hepatitis, therefore, HEV RNA testing was not performed.

In the MATERIALS AND METHODS section, the authors claimed that they use a commercial enzyme-linked immunosorbent assay based on recombinant antigens of HEV genotypes 1 and 3 to detect HEV IgG antibody. Does this assay have the ability to detect HEV IgG antibody induced by other HEV genotypes? As we all know, HEV4 are also prevalent in many regions worldwide. Could this affect the results of the study?

ELISA test used to detect HEV IgG antibodies is based on recombinant antigens of HEV genotypes 1 and 3. A second IB test used for results confirmation used recombinant antigens of HEV genotypes 1-4 (page 7, lines 8-11). Since only HEV genotype 3 was detected in Croatia so far (Jemeršić *et al.*, Genetic diversity of hepatitis E virus (HEV) strains derived from humans, swine and wild boars in Croatia from 2010 to 2017. BMC Infect Dis 2019; 19(1):269. doi: 10.1186/s12879-019-3906-6.), we believe that serology tests used in this study do not affect the results.

Grammar mistakes can be found in this manuscript. Please make revisions.

English has been revised.

We hope that the revised manuscript will be suitable for publication in World Journal of Gastroenterology.

Best regards,

Anna Mrzljak, corresponding author