

August 03th, 2022

Dear editor,

Thank you for your decision letter regarding our manuscript entitled “The clinical value of predictive model based on liver stiffness measurement in predicting liver reserve function of compensated chronic liver disease” (Manuscript Number ID:78451). We also thank the reviewers for the recognition of the scientific merits of our study and the valuable comments. We have revised the manuscript according to the reviewer’s suggestion. Point-to-point responses to the reviewers’ comments are listed as follows:

Reviewer #1:

Major comments:

1. The liver stiffness measurement (LSM) surely contributes to the superiority of their model. However, in the current study, they excluded those with incomplete LSM data. I consider that such patients should be classified as those who cannot be diagnosed by their models. If many patients were excluded due to the unsuccessful measurements, the real diagnostic performance for the all patients might be lower than the presented data. They should show whether their proposing models can provide good diagnostic performances, even the LSM-unmeasurable patients would be regarded as those without a correct diagnosis.

Response: Thank you for your reminding, only a small number of patients fail to complete LSM (most of the patients who do not want to be tested), and the incident happens randomly. This study has validated the good performance of the predictive model.

2. The LSM technique requires an expensive tool, and thus can be conducted in specific institutions. The limited availability may abolish the versatility/utility of their models.

Response: Thank you for your review. Due to the advantages of non-invasive detection of LSM, the LSM technique becomes more and more available, which is helpful to promote the prediction model.

3. As the authors mentioned, most of the studied patents suffered from HBV-related liver diseases. Regarding the biomarkers, diagnostic performances for the degree of liver fibrosis are different among the etiologies. If possible, kindly enlarge the number of cases without HBV-infection and check the diagnostic performances.

Response: Thanks for your comment. Most of the patients in this study were Asians with B viral hepatitis, and we talk about that in limits of the study. Therefore, the performance of the model in patients of other ethnicities and etiologies still needs further validation by enlarging the number of cases.

Minor comments:

1. They described ‘The normally distributed continuous variables were presented as mean \pm standard deviation (SD), which were further evaluated by Student’s t-test in the different groups’. Kindly mention how they determined the parametric/nonparametric variables. In addition, they should add how they analyzed the abnormally-distributed (nonparametric) variables.

Response: Thanks for your comment. We will add ‘variables showing skewed distributions were evaluated by the Mann-Whitney U test, which are presented as median (interquartile range)’ to statistical analysis.

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2. They cited BavenoVI paper. Kindly cite the BavenoVII paper (J Hepatol2022 Apr;76(4):959-974).

Response: Thanks for your review. We will cite the BavenoVII paper (J Hepatol2022 Apr;76(4):959-974).

Reviewer #2:

Thanks for your specific Comments.

We would once again like to thank you and the reviewers for evaluating of our manuscript. We believe that the quality of our manuscript has been significantly improved after revising it according to the comments and suggestions. We hope that the revised manuscript is now acceptable for publication in your prestigious journal *World Journal of Gastroenterology*. We look forward to hearing from you.

Best Regards,

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