

Dear Editor

Please find herewith our response to reviewer comments. Our responses are in red bold and italics

1. In the introduction section of the manuscript the authors try to make the case that transient elastography overestimates liver fibrosis in patients with AIH citing “recent studies have indicated that TE overestimates fibrosis in AIH.” The particular citation actually refers not to a “study”, but actually is a case series of 2 patients. Please make this clear for the reader.- ***We have supported our statement with two new references and updated the same paragraph.***
2. What was the time from initial diagnosis and treatment to liver biopsy and MRE in these patients?- ***this information is now included in the revised manuscript***
3. Were there changes in technique and interpretation methods of MRE over time?- there was no change in the technique during this period. ***All studies were performed with the same 2D- GRE technique***
4. If analysis is limited to untreated patients, is there a difference in diagnostic performance between MRE and serum-based tests?- ***we performed additional statistical analysis. In the untreated group of 17 patients, MRE had the best diagnostic performance when compared to serum based tests for advanced fibrosis (0.93 vs. 0.51-0.86) and cirrhosis (0.95 vs. 0.57-0.95). similarly MRE also had best diagnostic performance in 19 treated patients for both advanced fibrosis (0.98 vs. 0.59-0.87) and cirrhosis (1.0 vs. 0.64-0.89). We have added this information to the results section.***
5. In their discussion, authors’ propose “inflammation” as a potential cause of the difference in liver stiffness the observed between the treated and untreated patients. In non-cirrhotic patients there was increased liver stiffness among the untreated AIH patients, however this was reversed in cirrhotics. How can they reconcile these differences?-# ***The exact reason is not known, however it is possible that the fibrosis content in treated patients is likely to be more as the duration of disease was longer in these patients. This needs to be confirmed in studies with larger number of participants.***

6. The authors recognize the limitations of their retrospective study. They enumerate these limitations, but actually skipped the “fifth” limitation. They go from “Fourth, our study group...” to “Sixth, patients were assessed...”.- *the numbering of the limitations has been corrected in the revised version.*
7. Also on the point of limitations of the study, the authors seem to overplay the rarity of AIH as a reason the sample size is small. However, I don’t think this is justified, especially when considering Mayo Clinic is a world-renowned institution and a known referral center for liver disease.- *we have deleted the phrase that AIH is uncommon. We think that eligible patients in our group were smaller due to our strict inclusion criteria.*
8. It is unfortunate there was no direct comparison between MRE and other methods of assessing liver stiffness, such as transient elastography or acoustic radiation force impulse imaging. There was a recent publication on ARFI in AIH in *Hepatogastroenterology* 2015;62:670-672 suggesting ARFI is also a good method of assessing fibrosis in AIH. I recommend this citation be added to the manuscript. *We added another paragraph comparing the recently published articles on TE and ARFI*
9. Figure 2 and its figure legend do not coincide. The figure is a box plot of liver stiffness by MRE across the different histologic stages of fibrosis while the figure legend makes reference to an MRE of a patient with AIH. – *We apologise for the error. The correct figure is now included.*

I would ask all these points be addressed by the authors prior to considering the manuscript for acceptance to the *World Journal of Gastroenterology*.