# PEER-REVIEW REPORT

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|---|---|
| **Scientific quality** | [  ] Grade A: Excellent   [  ] Grade B: Very good   [ Y] Grade C: Good [  ] Grade D: Fair   [  ] Grade E: Do not publish |
| **Language quality** | [ Y] Grade A: Priority publishing   [  ] Grade B: Minor language polishing [  ] Grade C: A great deal of language polishing   [  ] Grade D: Rejection |
| **Conclusion** | [  ] Accept (High priority)   [  ] Accept (General priority) [ Y] Minor revision   [  ] Major revision   [  ] Rejection |
| **Re-review** | [  ] Yes   [ Y] No |
| **Peer-reviewer** | Peer-Review: [ Y] Anonymous   [  ] Onymous |

| statements | Conflicts-of-Interest: [   ] Yes   [ Y ] No |
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## SPECIFIC COMMENTS TO AUTHORS

A brief comment to the manuscript by Madian et al, on the predictive role of liver enzymes (namely AST) in COVID-19 infection. The authors underline the scarce attention given to the possible presence of underlying metabolic factors (NAFLD) in the pathogenesis of severe disease, driven by pre-existing metabolic dysfunction. The comment is pertinent, but very difficult to test pre-existing disease in an epidemiological retrospective analysis. • The authors suggest that elevated AST may be the result of COVID-dependent muscle injury, but AST did bot correlate with CPK levels. This should be considered in the letter.

# PEER-REVIEW REPORT

**Name of journal:** *World Journal of Hepatology*

**Manuscript NO:** 71625

**Title:** COVID-19 and liver disease: Are we missing something?

**Provenance and peer review**: Invited Manuscript; Externally peer reviewed

**Peer-review model:** Single blind

**Reviewer's code:** 05911905

**Position:** Peer Reviewer

**Academic degree:** MD

**Professional title:** Doctor

**Reviewer's Country/Territory:** China

**Author's Country/Territory:** India

**Manuscript submission date:** 2021-09-15

**Reviewer chosen by:** AI Technique

**Reviewer accepted review:** 2021-09-19 00:30

**Reviewer performed review:** 2021-09-19 01:18

**Review time:** 1 Hour

| Scientific quality | [  ] Grade A: Excellent    [  ] Grade B: Very good    [  ] Grade C: Good<br>[ Y] Grade D: Fair    [  ] Grade E: Do not publish |
|---|---|
| Language quality | [  ] Grade A: Priority publishing    [  ] Grade B: Minor language polishing<br>[ Y] Grade C: A great deal of language polishing    [  ] Grade D: Rejection |
| Conclusion | [  ] Accept (High priority)    [  ] Accept (General priority)<br>[  ] Minor revision    [  ] Major revision    [ Y] Rejection |
| Re-review | [  ] Yes    [ Y] No |
| Peer-reviewer | Peer-Review: [ Y] Anonymous    [  ] Onymous |

| statements | Conflicts-of-Interest: [   ] Yes   [ Y] No |
|---|---|

## SPECIFIC COMMENTS TO AUTHORS

In the letter commented on the paper published on World J Hepatol 2021; 13(8): 939-948, Dr. Tarana pointed out the main concern that the study did not evaluate patient's liver function/ condition prior to COVID-19 disease. The comment is relevant, as strong evidence have shown that NAFLD is a predictor of the severity COVID-19 disease.  In the letter, Dr,Tarana also argued that it may be too early to use AST levels to predict the severity and outcome of COVID-19 illness, since author also mentioned muscle injury in COVID-19 disease also contribute to the increase AST. However,   in the original paper, both AST/ALT did not corrected with serum CK level, while strongly associated with inflammatory markers, such as CRP, implicating that liver   damage sounds the main source of elevated AST/ALT.   Another concern is the way how the paper was organized.   It looks not well-structured and hard to follow.   For example, in the first paragraph,   author   stated" Only a few studies could highlight liver function tests in patients with COVID-19 in non-cirrhotic patients". After the statement, author suddenly switched the topic and began to discuss the ACE2 expression. Again, the pattern of liver injury was placed as 3rd paragraph.