PEER-REVIEW REPORT

Name of journal: *World Journal of Hepatology*

Manuscript NO: 74876

Title: Prevalence of nonalcoholic fatty liver disease and its association with age in patients with type 2 diabetes mellitus

Provenance and peer review: Invited manuscript; externally peer reviewed

Peer-review model: Single blind

Reviewer's code: 02536349

Position: Editorial Board

Academic degree: MD

Professional title: Doctor, Professor

Reviewer's Country/Territory: Turkey

Author's Country/Territory: Japan

Manuscript submission date: 2022-01-10

Reviewer chosen by: AI Technique

Reviewer accepted review: 2022-01-10 14:55

Reviewer performed review: 2022-01-10 18:46

Review time: 3 Hours

Scientific quality	[ ] Grade A: Excellent [ ] Grade B: Very good [Y] Grade C: Good [ ] Grade D: Fair [ ] Grade E: Do not publish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

Peer-reviewer statements	Peer-Review: [Y] Anonymous [ ] Onymous
	Conflicts-of-Interest: [ ] Yes [Y] No

SPECIFIC COMMENTS TO AUTHORS

1- Limitations section of the study is absent. 2- The indications of CT are not expressed, thus the study group consists of diabetic patients with problems requiring imaging. So the group may not represent all cases with diabetes.   3- Histology is the gold standard of steatosis and based on CT alone is not sufficient to determine NAFLD to give a prevalance. The iron level in liver highly resembles steatosis with CT liver-spleen 4- The reported specificity is 100% while sensitivity is 80%. (J Hepatol. 2010 Apr; 52(4):579-85.) Thus 20% of cases may be dismissed. 5- Statistics: Unnecessary long decimals of p-values could be limited to three numbers as p<0.001 is the most significant.   Also very low p-values like p<0.0001 will be rarely encountered, because it would mean that the trial was overpowered and should have had a smaller sample size. It would seem appropriate, therefore, to require investigators to explain such results and to consider rejecting the research involved. (DOI: 10.1515/CCLM.2004.054) In most fields, a value of 0.0001 would be statistically significant. In a few it would not- e.g. in high-energy physics, where the experiments generate vast amounts of data, the thresholds are set much higher. In medical research were generating this amount of data is impossible, a value like this would be more than enough (indeed many journals will just report a value   of   ">   0.001"   as   such) ((https://www.quora.com/Is-a-P-value-of-0-0001-statistically-significant).   6-   Minor typo in Table 2. "dyalysis > dialysis 7- Overall, the prevalance in this study have to be reevaluated.

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Reviewer's code: 05040445

Position: Editorial Board

Academic degree: MD

Professional title: Associate Professor, Chief Physician, Professor

Reviewer's Country/Territory: China

Author's Country/Territory: Japan

Manuscript submission date: 2022-01-10

Reviewer chosen by: AI Technique

Reviewer accepted review: 2022-01-16 08:55

Reviewer performed review: 2022-01-16 16:12

Review time: 7 Hours

Scientific quality	[ ] Grade A: Excellent [ ] Grade B: Very good [ ] Grade C: Good [Y] Grade D: Fair [ ] Grade E: Do not publish
Language quality	[ ] Grade A: Priority publishing [Y] Grade B: Minor language polishing [ ] Grade C: A great deal of language polishing [ ] Grade D: Rejection
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SPECIFIC COMMENTS TO AUTHORS

The authors aim to determine the prevalence and clinical correlated of NAFLD in a cohort of T2DM. My major questions are as follows: 1. The title is very big and with little informations in the study. 2. The author only described the patients are diabetic and no other liver diseases. However, what about those diabetic patients conditions? Are they newly diagnosed or with certain years of diabetes? Or are those patients with or without complications? Or what treatments are they have at the time of the study? Are they under oral anti diabetic treatments or with insulin injection or GLP-1R agonist?   3. There are too many influence factors in diabetic patients that will affect NAFLD, such as TZD, GLP-1R agonist, metformin, duration of diabetes, so the authors should investigate such in a certain condition of diabetic patients, for example newly diagnosed diabetic patients. 4. If the patients included have a big variety condition then the results will be wired, like the author said that "Fatty liver was significantly associated with greater height ($p < 0.0001$) " , and there is sex differences in NAFLD, however, the authors should considerd the menopausal state of women.

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Conclusion	[ ] Accept (High priority) [ ] Accept (General priority) [ ] Minor revision [Y] Major revision [ ] Rejection
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	Conflicts-of-Interest: [ ] Yes [Y] No

SPECIFIC COMMENTS TO AUTHORS

Authors conducted a retrospective study to investigate NAFLD prevalence in a cohort of adult patients with T2D. Although there is previous evidence in this field, the study might expand knowledge on this interesting topic. However, in my opinion there are several major issues that prevent publication of the manuscript in this form. -Abstract: in the methods section the age of patients should be added - core tip: this section should be attractive for the readers and not just a summary of the study - Introduction: a brief overview on NAFLD and its relevant cardiometabolic burden   since childhood should be added (e.g.PMIDs 28686220 34629802). More, the intriguing link between NAFLD and T2D needs to be clearer discussed (e.g. PMID: 32165250). - methods: statistics needs to be clarified (e.g. for categorical variables we used....) - results: this section needs to be reorganized. As a suggestion, it should be useful to show the results by subdividing the associations according to " blood count parameters", "kidney function", and "metabolic status   (lipids)" - discussion: it needs to be revised. At the beginning, main findings of the study should be mentioned. As cardiovascular risk markers, the relationship with lipids should be considered in the discussion about the association of NAFLD with CVD. - English language needs a substantial revision.

RE-REVIEW REPORT OF REVISED MANUSCRIPT

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Position: Editorial Board

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Professional title: Doctor, Professor

Reviewer's Country/Territory: Turkey

Author's Country/Territory: Japan

Manuscript submission date: 2022-01-10

Reviewer chosen by: Ji-Hong Liu

Reviewer accepted review: 2022-04-13 19:17

Reviewer performed review: 2022-04-13 19:40

Review time: 1 Hour

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) [Y] Accept (General priority) [ ] Minor revision [ ] Major revision [ ] Rejection
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statements	Conflicts-of-Interest: [ ] Yes [Y] No

SPECIFIC COMMENTS TO AUTHORS

The revisions are acceptable and the manuscript is sufficient to be published.