**PEER-REVIEW REPORT**

**Name of journal:** World Journal of Gastroenterology

**Manuscript NO:** 39293

**Title:** Favorable Clinical Outcome of Non-Alcoholic Liver Cirrhosis Patients with Coronary Artery Disease: A Population-Based Study

**Reviewer’s code:** 02520738

**Reviewer’s country:** Italy

**Science editor:** Ze-Mao Gong

**Date sent for review:** 2018-04-19

**Date reviewed:** 2018-04-23

**Review time:** 4 Days

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| **SCIENTIFIC QUALITY** | **LANGUAGE QUALITY** | **CONCLUSION** | **PEER-REVIEWER STATEMENTS** |
| [ ] Grade A: Excellent  [ ] Grade B: Very good  [ Y] Grade C: Good  [ ] Grade D: Fair  [ ] Grade E: Do not  publish | [ ] Grade A: Priority publishing  [ Y] Grade B: Minor language  polishing  [ ] Grade C: A great deal of  language polishing  [ ] Grade D: Rejection | [ ] Accept  (High priority)  [ ] Accept  (General priority)  [ ] Minor revision  [ Y] Major revision  [ ] Rejection | Peer-Review:  [ Y] Anonymous  [ ] Onymous  Peer-reviewer’s expertise on the topic of the manuscript:  [ Y] Advanced  [ ] General  [ ] No expertise  Conflicts-of-Interest:  [ ] Yes  [ Y] No |

**SPECIFIC COMMENTS TO AUTHORS**

To: Editorial Board World Journal of Gastroenterology Title: “Favorable Clinical Outcome of Non-Alcoholic Liver Cirrhosis Patients with Coronary Artery Disease: A Population-Based Study” Dear Editor, I read this manuscript and I think that: - More numerical data should be added to the results section of the abstract. Please provide. - Inclusion and exclusion criteria should be better described. Please provide. - A flow chart of the study should be added. - The use of ICD9 could also be considered as a limitation of the study design. - NALC can influence endothelial function and increase cardiovascular risk of individuals. Please discuss such a point in relation to the paper from Ciccone MM et al. J Cardiovasc Med (Hagerstown). 2015 Jan;16(1):11-21.

**INITIAL REVIEW OF THE MANUSCRIPT**

***Google Search:***

[ ] The same title

[ ] Duplicate publication

[ ] Plagiarism

[ Y] No

***BPG Search:***

[ ] The same title

[ ] Duplicate publication

[ ] Plagiarism

[ Y] No

**PEER-REVIEW REPORT**

**Name of journal:** World Journal of Gastroenterology

**Manuscript NO:** 39293

**Title:** Favorable Clinical Outcome of Non-Alcoholic Liver Cirrhosis Patients with Coronary Artery Disease: A Population-Based Study

**Reviewer’s code:** 00060494

**Reviewer’s country:** Taiwan

**Science editor:** Ze-Mao Gong

**Date sent for review:** 2018-05-23

**Date reviewed:** 2018-05-27

**Review time:** 4 Days

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| **SCIENTIFIC QUALITY** | **LANGUAGE QUALITY** | **CONCLUSION** | **PEER-REVIEWER STATEMENTS** |
| [ ] Grade A: Excellent  [ Y] Grade B: Very good  [ ] Grade C: Good  [ ] Grade D: Fair  [ ] Grade E: Do not  publish | [ ] Grade A: Priority publishing  [ Y] Grade B: Minor language  polishing  [ ] Grade C: A great deal of  language polishing  [ ] Grade D: Rejection | [ ] Accept  (High priority)  [ Y] Accept  (General priority)  [ ] Minor revision  [ ] Major revision  [ ] Rejection | Peer-Review:  [ Y] Anonymous  [ ] Onymous  Peer-reviewer’s expertise on the topic of the manuscript:  [ Y] Advanced  [ ] General  [ ] No expertise  Conflicts-of-Interest:  [ ] Yes  [ Y] No |

**SPECIFIC COMMENTS TO AUTHORS**

A nationwide retrospective longitudinal Cohort population-based cohort study based on the Taiwanese National Health Insurance research database (NHIRD). 1. In your study, the co-morbidities are different between the study and control group. Therefore, a propensity score based grouping may be considered. 2. In study group, there are more CAD risk factors (eg, DM, HTN, CKD) but there is less probability to CAD occurrence. Please give an explanation to this issue 3 . Please supply the data of mean F/U years of these patients. I think that relative less F/U period of such a disease (CAD) may be impact on the Hazard ratios of mortality in non-alcoholic LC patients. It may be a limitation of this study.

**INITIAL REVIEW OF THE MANUSCRIPT**

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[ ] The same title

[ ] Duplicate publication

[ ] Plagiarism

[ Y] No

***BPG Search:***

[ ] The same title

[ ] Duplicate publication

[ ] Plagiarism

[ Y] No

**PEER-REVIEW REPORT**

**Name of journal:** World Journal of Gastroenterology

**Manuscript NO:** 39293

**Title:** Favorable Clinical Outcome of Non-Alcoholic Liver Cirrhosis Patients with Coronary Artery Disease: A Population-Based Study

**Reviewer’s code:** 03832930

**Reviewer’s country:** Reviewer\_Country

**Science editor:** Ze-Mao Gong

**Date sent for review:** 2018-05-23

**Date reviewed:** 2018-05-31

**Review time:** 8 Days

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| **SCIENTIFIC QUALITY** | **LANGUAGE QUALITY** | **CONCLUSION** | **PEER-REVIEWER STATEMENTS** |
| [ ] Grade A: Excellent  [ ] Grade B: Very good  [ Y] Grade C: Good  [ ] Grade D: Fair  [ ] Grade E: Do not  publish | [ ] Grade A: Priority publishing  [ Y] Grade B: Minor language  polishing  [ ] Grade C: A great deal of  language polishing  [ ] Grade D: Rejection | [ ] Accept  (High priority)  [ ] Accept  (General priority)  [ ] Minor revision  [ Y] Major revision  [ ] Rejection | Peer-Review:  [ Y] Anonymous  [ ] Onymous  Peer-reviewer’s expertise on the topic of the manuscript:  [ ] Advanced  [ Y] General  [ ] No expertise  Conflicts-of-Interest:  [ ] Yes  [ Y] No |

**SPECIFIC COMMENTS TO AUTHORS**

I read with interest the original research by Tsai MC et al. The study is of interest since it examines the controversial role of coronary artery disease in the favorable outcome of non-alcoholic liver cirrhosis patients, in a population-based study. The manuscript is well written in a comprehensive way. However, in my opinion, there still are several things need to be clarified. 1. In the abstract, it is not clear if the inclusion period of the newly diagnosed non-alcoholic LC patient was during 2006 or from 2006 until 2011. They did not mention the mean ± SD or median [IC 95% range] of the follow-up period. They should clarify this in the abstract and also give this information in the materials and methods section. The should clearly specified the inclusion date of the first and last subjects included in the analysis 2. The authors mentioned that CAD and hyperlipidemia were less prevalent in the control group, however they did not give the prevalence and p values in the abstract. Also, the data regarding CAD prevalence is missing in table 1. The authors should include this information in the abstract and in table 1. 3. Considering that Taiwan is and endemic area of viral hepatitis, the authors mentioned that this minimized the confounding effects for alcohol consumption in this non-alcoholic liver cirrhosis cohort. However, they do no give any information regarding the treatment for viral infection (hepatitis B and C); was the proportion of treated patient similar in both groups of patients this information should be given in table 2, in the results section and should be discuss the possible influence of the treatment on the results. Moreover, the statistical analyses should be adjusted for the vital treatment. 4. In materials and methods, instead of given de ICD-9-CM codes for the comorbidities (this information could be given in supplemental material) the authors should clearly describe the criteria of diagnosis of how each comorbidity was defined, with special emphasis on the definition of CAD, which is the end point measurement. 5. Did the authors calculate the sample size to be able to observed a significant association between non-alcoholic LC with CAD? Considering the conflicting results so far published, it is desirable to calculate the statistical power of the cox regression model. 6. Family history of CAD is a very important coronary risk factor, was this information recorded in the 3236 patients and in the 16180 controls? Could this information have any influence in the prevalence of CAD in each group? This issue should be discussed in the discussion section. 7. The authors should briefly explain what does hyperlipidemia means, is TC>200mg/dl, LDL-C, >160 mg/dL, TG>150mg/dl a combination of two or more??? 8. On page 11, at the end of the second paragraph the authors should include the data of the adjusted model instead of the unadjusted model, and if so, the hepatic encephalopathy is no longer the comorbidity with the highest HR, this should be corrected. 9. On page 11, in the third paragraph the authors should include the word respectively, after the 95%CI values are given 10. The authors recognized that the severity of CAD was not recorder. The authors should discuss the potential impact of this important information on the results they have shown.

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[ ] Plagiarism

[ Y] No