Author's response to reviews

Title: Risk of De Novo Post-Transplant Type 2 Diabetes in Patients Undergoing Liver Transplant for Non-Alcoholic Steatohepatitis

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Editorial office

Dear Editors:

We are re-submitting our manuscript entitled “Risk of De Novo Post-Transplant Type 2 Diabetes in Patients Undergoing Liver Transplant for Non-Alcoholic Steatohepatitis” for publication consideration. We have addressed the reviewers’ comment and appreciate the input.

Again, our deepest appreciations for your consideration and we look forward to hearing from you.

Sincerely yours,

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Reviewer 1

Q1. *I think there is an unintentional inclusion of HCV related transplants due to inclusion of primary liver malignancy (27.5%). It is known that a significant proportion of the HCC patients are probably due to HCV and thus the control group might not truly represent non HCV controls. This would skew the calculations and would need to be adjusted.*

A1. The reviewer is right that HCC is often driven by HCV, and we are grateful for that. We, however, believe that exclusion of all HCC cases from controls for the sake of ruling out HCV would cause even more bias because HCC is a major complication of NASH and, thus, should not be completely excluded from either study arms. So, we added negative HCV serology to the definition of controls and rerun the analysis accordingly. We must note that the association of post-transplant DM with NASH has become even more prominent (as seen from slightly greater risk ratios) suggesting that indeed some previous controls did have HCV infection and were affected by its diabetogenic effect. We, again, are extremely grateful to the reviewer for this excellent suggestion which allowed improving the study design.

Q2. *The authors themselves published recently regarding certain donor characteristics: donor age, donor h/o DM, DCD to have a significant impact on PTDM (p< 0.0001) in the recipient. The authors include DCD risk in table 2 but it would be pertinent to include the adjusted hazard ratio (aHR) of donor age and donor h/o DM for PTDM in this paper as well.*

A2. We added those parameters to the models (see updated Table 4). The association is present but only borderline significant.

Q3. Line 51, 53, 54: *Need to rephrase that the etiology is cryptogenic cirrhosis (it can be clarified later that a majority of them are most likely related to NASH).*

A3. Corrected.

Q4. Line 56: *clarify the sentence “atleast one onset of de novo diabetes”. Do the authors mean one episode of hyperglycemia?*


Q5. Line 58: *How was diabetes diagnosed? Was ADA criteria used to diagnose DM? Please use diabetes mellitus or DM or PTDM consistently through out the paper.*

A5. See A4 – it is a clinical diagnosis of DM made by a patient’s healthcare provider. We fixed the inconsistent notation.

Q6. Line 107: *simplify the sentence.*


Q7. Line 289: *epidemic*

A7. Corrected.

Q8. *Table 2: 2nd group is controls not ALD. Please correct*

A8. Corrected.