Peer Review File

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Review comments

Comment 1: In the section of introduction, the sentence is too long and redundancy. Please described more briefly.

Answer 1: Thanks a lot for your constructive comment. We optimized the introduction and deleted some redundant descriptions to make the introduction more concise.

Changes 1:We have modified our text as advised (see Page 3-7, line 69-148)

Comment 2: Authors should add information about the presence and/or history of hepatic ascites and encephalopathy In Table 1. This information is associated with Child Pugh classification. In addition, authors should add the ALBI score.

Answer 2: It's a really thought-provoking question. Thanks a lot for your advices. Our team discussed it in detail. Firstly, ALBI score has been supplemented in Table 1.

Besides,as regards two indicators you mentioned： hepatic encephalopathy and ascites, we decide to hold reserved opinions in the text for the reasons as follows.

For the indicator of hepatic encephalopathy, according to our further follow-up in 467 patients we did not find positive patients with hepatic encephalopathy. In addition, the probability of hepatic encephalopathy in patients is relatively low due to such good liver function of those undergoing local thermal ablation/resection.

For the indicator of ascites, there is a lack of specific quantitative description of this indicator in some cases because the Child-Pugh score was evaluated when the patient was admitted to hospital by the doctor according to the patient's condition and only the score was recorded in the medical record. As a retrospective study, it's impossible to inquire concrete information of ascites degree. In addition, we read some papers in the same domain and found that ascites is not a necessary indicator in the prognostic analysis of local resection or thermal ablation. This is why we didn 't include it in the study. Indeed, we know that ascites is an important indicator in prognosis research. In

future studies, we will utilize other methodology objectively to evaluate it.
Changes 2:We have modified our text as advised (see Page 24, Table 1)

Comment 3: Figure 1 is confusing. According to figure legend, cumulative survival curve is Figure 1a and 1c. But, vertical axis in Figure 1a and 1b showed overall survival. Please check the figure 1 carefully.
Answer 3: We apologize for the confusion generated by the previous version of the manuscript and sincerely hope that our logic is now easier to follow with this new version. We have already made some changes in Figure Legend with track changes.
Changes 3:We have modified our text as advised (see Page 23, line 532-536)

Comment 4: I did not understand why authors have to analyze the propensity score matching. I believe that it is desirable to delete the Figure 3 and Table 4.
Answer 4: We appreciate reviewer for his effort to review our manuscript, and his feedback. However, we believe it is necessary to use propensity score matching in our study. In this study, based on Child-Pugh A patients, we find that a significantly larger number of patients in the ALBI-1 group exhibiting higher PLT level, lower AST level, lower GGT level and lower ALP level than those in the ALBI-2 group. The presence of an imbalance baseline characteristics between the ALBI-1 and ALBI-2 groups may lead to a biased estimate of the results. Matching each subject in the ALBI-1 group with subjects in the ALBI-2 group with comparable baseline confounders is an intuitive way to minimize confounding in this study. In this study, using a similar estimated PS creates approximate balance for all the confounders, and difference in outcomes within groups with a similar PS gives unbiased estimate of comparison in different ALBI grades.
Changes 4:

Comment 5: I think that regarding recurrence-free survival stratified Child Pugh class A and B, no significant difference might be associated with small number of patients with Child-Pugh class B. That is, this may be due to the lack of statistical power. What do you think about it? Authors should mention and discuss it.

Answer 5: Thanks for your excellent advice. You have raised an important point here. The number of patients is really important in survival analysis. The small small number of C-P B patients is discussed in the revision of the manuscript. We cite relevant studies in discussion which have similar points of view with us. What's more, we add this drawback in our limitations.

Changes 5:We have modified our text as advised (see Page 15-16, line 340-348 & Page 18, line 397-400)