Esteemed Reviewers,

Esteemed Editor,

We appreciate your valuable feedback. We have provided a point-by-point response to your comments, trying to address the most important caveats of our work.

Peer Reviewer 1:

1. **The sample size was relatively small for multivariate analysis. The sample size should be estimated prior to the study.**

   We do understand your concern with regards to the sample size of our study. Unfortunately, given the design of our research, we find it impossible to increase the number of patients without further generating bias. However, we would like to reassure the reviewer that we have made adequate pre-test estimations of our sample size by taking into account the expected prevalence of malnutrition among patients with advanced liver disease, among other decompensating events. Of course, we are aware of the important variability of malnutrition prevalence among different cohorts, with available figures ranging from 20% to more than 90%, depending on the type of measurement and the inclusion criteria (EASL Clinical Practice Guidelines on nutrition in chronic liver disease. J Hepatol 2019;70(1):172–93). Furthermore, when we designed our study, there were only a few other studies available with comparable design and using similar assessment tools, most of which have a significantly smaller sample size (**36 patients** - Tai ML, Goh KL, Mohd-Taib SH, et al. Anthropometric, biochemical and clinical assessment of malnutrition in Malaysian patients with advanced cirrhosis. Nutr J. 2010;9:27; **50 patients** - Comparison between handgrip strength, subjective global assessment, and prognostic nutritional index in assessing malnutrition and predicting clinical outcome in cirrhotic outpatients. Alvares-da-Silva MR, Reverbel da Silveira T. Nutrition. 2005; **104 patients** - Akerman PA, Jenkins RL, Bistrian BR. Preoperative nutrition assessment in liver transplantation. Nutrition. 1993;9(4):350–6. Feb; 21(2):113-7). In this light, given a statistically acceptable confidence level, the margin of error, and to avoid type I and type II errors, we calculated a required sample size of approximately 100 patients needed to fit our design. We have addressed this issue in the “limitations” section of the Discussions.

2. **The findings should be validated in another cohort.**

We agree that external validation can add value to any study design and can ultimately prove its reproducibility. However, correct this caveat is virtually impossible in a short timeframe given our design. In our efforts to address this issue, we have compared our results with similar papers published in the field in the discussion section of our initial manuscript. Our data followed the same trend as previously published reports and confirmed the validity of our findings. We have also added a phrase at the end of the discussion section addressing the lack of external validation.

Peer-reviewer 2:

1. **In general, fully compensated patients without any previous decompensation (n=22) are to be defined as the compensated group and re-compensated patients (n=15, 40.5%) should be treated as the decompensated one. Additionally, authors should better show the validation of setting the definition of "decompensation" in this study. Nevertheless, all the data analyzed in this study would be less persuasive.**

   Thank you for your pertinent remark. We have adjusted our statistical analysis accordingly. We have also clarified the definitions of decompensation in the Methods section of the manuscript. The new results follow the same trend and retain their significance across the board; however, the discrepancy between groups has slightly decreased, as expected.

2. **Furthermore, to diminish the unbalance of registered number of patients in both groups (compensated and decompensated), authors should better include more fully compensated cases.**

   We do agree that there is a significant unbalance between groups. However, for the latter part of our analysis, we tried to include approximately equal proportions of patients from all ranges of the liver disease severity spectrum (there are about one-third of the patients in each Child-Pugh class overall). Given our design, we did not want to generate a potential bias by creating equal decompensated and compensated groups, as it would have led to inaccurate assessments with regards to the overall prevalence and skewed survival analysis. As stated, comparing the two groups from a nutritional standpoint was a secondary aim of our study.

3. **The data of branched-chain amino acids to tyrosine ratio value (BTR), body mass index (BMI) and skeletal muscle index (SMI,**

https://www.nature.com/articles/s41430-017-0034-5) would be important in this study.

We agree that these metrics would have added value to our report. Unfortunately, these data were not available for retrospective analysis. We have introduced an in-depth paragraph in the Discussion section addressing this topic.

4. **Authors should better show the flowchart of patient recruitment**

We have provided the flowchart of patient recruitment

5. **The image resolution of supplementary Fig.S1 was coarse**

We apologize for the mistake in our initial version of the manuscript. The high-quality Textversion of the figure was added.

Answer reviewers for Re-review

Dear Editor, dear reviewer Thank you for your comments. Please find our response to all the comments raised by the Editor and reviewer.

1. The authors responded well to the reviewer's comments. However, there are still some concerns. Even though the authors did not have any data of BTR, it would be actually possible to indicate the scores of body mass index which is simply calculated by height and body weight in the targeted patients, instead. Concerning to the SMI, all what you need is just the images of CT scan by which you and your colleagues had diagnosed some cases of HCC in their early stages in the participants of this study. Indeed, it is unfortunate that we don't have data on BTR or CT scans in all patients. We added the data about BMI (was added in Table 2). However, we believe that in patients with ascites, it is not very relevant. Regarding the suggestion to calculate the SMI only in patients with available CT scans (those with HCC diagnosis), we believe that will be a source of selection bias. Moreover, the subgroup is small, and the extrapolation of the results to the entire population is doubtful.

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