

Response to reviewers:

Thank you all for taking the time to provide us with your constructive criticism of the manuscript. We find the comments helpful and believe they will strengthen the paper. We have addressed each reviewers comments below and applied changes to the manuscript, if applicable. We hope the revisions are to your satisfaction.

Reviewer #1:

COMMENTS TO AUTHORS

Authors should give some more information about liver the cirrhosis cause....i.e., this case of immunosuppression (1), and that of one was unknown origin should be classified as cryptogenic. In table and Figures C-P scores should be properly expressed. What does C-P progression mean? It should be opportunely clarified. The C-P classification used by authors should be referenced, referring to a modern one, as presented in....What are the implications of the spontaneous spleno-renal shunts in liver cirrhosis? BMC Gastroenterol. 2009 Nov 24;9:89

Response:

- The one case of immunosuppression was following a kidney transplant, which has been added to the results section. The unknown origin was changed to cryptogenic as you stated most correctly. This is highlighted in bold.
- Thank you for pointing out “CP Progression” on the figures. We understand how that can be misleading. It is referring to the portion of patients who retained Child Pugh A status instead of advancing to Child Pugh B. We changed this on figures 2 and 3.
- We added a definition of CP Progression as advancing from Child Pugh A to Child Pugh B in the “outcome assessments” section of METHODS. This is in bold. We also cited the reference you so kindly provided after the definition, per your suggestion. Thank you.

Reviewer #2:

COMMENTS TO AUTHORS

Hasan et al reported a retrospective study of the Stereotactic Body Radiotherapy (SBRT) for patients who had an early stage hepatocellular carcinoma (HCC). Although indication for SBRT rather than the other options of the therapy was unclear, it might contain some relevant information for future therapy. I recommend the authors should revise figs and text including refs carefully. You should follow the instruction of the proper journal. You should not submit text with previous reviewer's comments. Furthermore, you did not change the previous text at all. Table 1; Patient characteristics should present more precisely. You could present it with cox proportional hazard models. Was there any additional treatment after SBRT? It was unclear how many patients received additional treatment and what was it? What was tumor response after SBRT? Was there any change regarding vascularity? In addition, you should present TMN classification as well. I disagree with your conclusion. Because deterioration of CP score was seen in almost half patients after one year. It was too fast to deteriorate liver function compared to other therapy in general. It must be radiation, did it? This information is interesting. It seems to be negative impression, but it is an important to avoid further clinical trial. Overall the study was poor due to lack of clinical information and statistical analysis. Please revise it all including previous reviewers comments.

Response:

- Thank you for pointing out the formatting inconsistencies; they have been changed to conform with the journal's instruction.
- Previous reviewer comments have been removed, and current reviewer comments have been addressed throughout the revised manuscript.
- Thank you for the suggestion on table 1. Our apologies that it was not clear. However, the patient characteristics are not meant to represent potential risk factors for a certain outcome, but rather a description of the patients included in this observational study. Therefore, a cox proportional hazard model is not warranted. However, for such information please refer to the final sentences of the "survival" and "hepatic function and toxicity" subsections under results for cox proportional hazard models, which as noted in the paper were only significant for gross tumor volume.
- Please refer to third and 5th sentences of the "control" paragraph under the results section to note additional treatments patients received after SBRT. This is detailed in bold.

- The local response is mentioned in the first sentence under the “control” subsection of the Results section, where it states that 48 of 49 lesions were controlled locally at last follow up.
- Per your recommendation, AJCC stages have been added to the “Patient Characteristics” subsection under Results. It is located in the 6th sentence. This is in bold.
- Sorry for the confusion regarding Child Pugh progression. Over 70% of patients did *not* have progressive cirrhosis (ie 70% retained CPA status at 1 year). You wrote that you thought nearly half the patients deteriorated after 1 year, which is not the case and in fact the median time to progression was never reached because at no point did half the patients progress. We now realize how this may not be clear based on Figures 2 and 3, and your fellow reviewers pointed this out as well. Therefore, we added a new title and key for Figures 2 and 3 in bold, labeled “Freedom from CP Progression.”
- Thank you again for bringing these issues to our attention and hopefully our revision has made the interpretation of our data more clear.

COMMENTS TO AUTHORS

The authors have presented an interesting and well constructed paper looking at safety, tolerability & outcome in patients with early liver disease treated with SBRT. The paper has a reasonable number of patients given the wide range of presentation and treatment options available. The authors are also realistic in their conclusions about the reliability of this type of data. My only real concern is the 8 patients who went on to have a transplant as this will inevitably skew the data considerably and I would suggest that they should be treated entirely differently in terms of outcome. Otherwise the paper is worthy of publication.

Response:

- Thank you for your kind review. We appreciate your approval for publication.
- We agree with the potential skew in outcome based on liver transplant. In this particular data set however, 3 of the 8 transplant patients died of perioperative complications so in fact there was no difference in survival. Of course this should be discussed, to your point, and that is now reflected in the 2nd and 3rd sentences of the “survival” section under results. This is highlighted in bold.

COMMENTS TO AUTHORS

Comments for ESPS Manuscript NO 36618

1. General comments The paper tackles an interesting topic. However, I have some comments regarding the paper.
2. Specific comments

(1)Major comments: ①The authors should submit a revised manuscript rather than one previous reviewer's comments. ②The references used in this paper are rather confusing, thus the authors should give the proper level of attention to citations and reference sections. (2)Minor comments: The format of this manuscript should be revised according to the journal's requirement

Response:

- Thank you for bringing all these issues to our attention. Previous reviewer comments have been removed and current reviewer comments have all been addressed in the manuscript.
- The paper and bibliography has also been re-formatted to be consistent with the journal guidelines.
- We appreciate your recommendation for publication pending the aforementioned changes.