

Dear Dr Wang,

Thank you very much for the rigorous peer review and evaluation by the editorial office of our invited review (ESPS Manuscript NO: 19583) entitled "Liver surgery in cirrhosis and portal hypertension".

We would like to thank the reviewers for their efforts in carefully reviewing this work.

We have responded to all points raised by all three reviewers and have carefully revised our manuscript accordingly.

We appreciate your time and consideration in handling and re-reviewing our manuscript and are looking forward to receiving your final decision.

Best regards,

Christina Hackl

In detail, the manuscript has been revised as follows:

Reviewer 1:

Dear Author, thank you for submitting this highly interesting manuscript to WJG. In my opinion it can be accepted for publication after minor revision. Please try to include results of these highly selected patient Groups more in detail. Although this is not a systematic review, please try to include e.g. Kaplan-Meier Survival Graphs to illustrate the problems of the topic (e.g. results of Liver resection of Child A vs. B vs C?). At all the paper is of outstanding quality and includes all important topics of these challenging patients.

We thank the reviewer for this judgement. Actually, we currently submitted an original paper analysing Kaplan Meier Survival graphs after hepatic resections in cirrhotic patients performed at our institution during the past 13 years (Renner et al,

manuscript in revision). Therefore, we cannot include the same graphs in the present review. However, we have included the following publications quoting Kaplan Meier analyses into our discussion:

Torzilli et al., Ann Surg (2013); 257 (5):929-937;
Zhong et al. Ann Surg (2014); 260 (2):329-340;
Ishizawa et al., Gastroenterology (2008); 134 (7):1908-1916;
Cucchetti A, et al.; Ann Surg (2008); 250 (6):922-928.
Liu et al; J Surg Oncol (2014); 111 (4):404-409.
Vitale et al; Journal of hepatology (2015); 62 (3):617-624.
Yin et al.; Journal of hepatology (2014); 61 (1):82-88.
Chang et al; Surgery(2012); 152 (5):809-820.
Kluger et al; Journal of hepatology (2015) 62 (5):1131-1140.

Reviewer 2:

The authors describe in their review the possibilities of liver surgery in patient suffering from liver cirrhosis and possibly also from portal hypertension.

Comments:

- The paper is well written, and only some minor linguistic inaccuracies as hepatic cirrhosis may be seen. It seems like the paper is comprised of three different parts written by different authors. By far the best part is between the subtitle Preoperative risk evaluation and Postoperative morbidity. This part describing the actual operative risks and surgical techniques discloses the message of the article.

We thank this reviewer for his review. We have carefully revised our manuscript, including revision by a English native speaker colleague.

- Table 1. The list of various cirrhoses is rather diffuse. The cirrhoses could be grouped on the ground of the background of the disease. Polycystic degeneration may not be regarded as cirrhosis. Infection as a cause of cirrhosis? May be omitted.

We omitted polycystic degeneration and infection as causes of cirrhosis.

- Table 2. VOD is nowadays called SOS.

VOD was replaced by SOS throughout the manuscript

- Postoperative Morbidity. Table 4 is fine, but small chapters dealing with postoperative complications are rather uninformative. They should be put together, and only a mention of wound healing and postoperative hemorrhage is enough.

The manuscript was revised accordingly

- Results after liver resection... Some repetition to the introduction of the paper is noted.

The results part was revised and redundances were deleted.

- Surgical therapy of portal hypertension. This part does not belong to the focus of the study. It is fair enough to mention that TIPS may be used before some major abdominal surgery to alleviate portal hypertension, but shunt surgery is absolutely beyond of the scope of the study. Thus Table 5 should be omitted.

Since shunt surgery is a complex surgical procedure which has to be performed in specialized centers we agree that it should not be a main focus of a review on hepatic resections in cirrhotic patients. However, especially since it can significantly reduce morbidity and mortality in selected patients needing surgery in underlying cirrhosis,

we prefer mentioning it in our subchapter. However, we leave the decision to the editors and are willing to delete this subchapter if desired.

- I would expect that the team would have much more interesting figures than Fig 1 and 2.

Since the other reviewers agreed with the figures and since we consider both figures as characteristic examples of patients asking for liver resections in underlying cirrhosis we did not modify the figures.

Reviewer 3:

Major Comment: This manuscript provides an overview on an important and delicate topic. Some passages remain vague, but this is not the fault of the authors.

Minor Comments:

For “Philipp Renner” no author contribution is stated.

Philipp Renner’s author contribution was added.

Page 4, first line: “encephatopathy” -> encephalopathy

Page 5, end of first paragraph: “thrompocytopenia” -> thrombocytopenia; “<100000/nl”-> did you mean <100/nl?

Table 4: “bilirubine” -> bilirubin.

We have carefully revised our manuscript, including revision by an English native speaker colleague.

Tables 3 and 5: Please provide references for the numbers and percentages (even if some of them are mentioned elsewhere within the text).

The desired references were added

Language polishing is required (e.g., “...a significantly increase in perioperative morbidity and mortality has been shown...”; “...expertise in shunt surgery is not widespread anymore and therefore should be only performed in specialized centres”; etc.)

We have carefully revised our manuscript, including revision by a English native speaker colleague.