Author's response to reviews

Title: Ultrasonic scalpel causes greater depth of soft tissue necrosis compared to monopolar electrocautery in a pig model

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Author's response to reviews: see over
Dear Michael Flierl,

We appreciate the extensive reviewers’ comments and thank you for the opportunity to submit a revised version of our manuscript. We carefully considered all reviewers comments and we improved the manuscript accordingly. In addition to these revisions, we reformatted the revised manuscript according to the journal guidelines as provided by the downloadable sample article.

In the following you will find a point-by-point response to the reviewer’s objections. We hope that the revised manuscript now meets the demands of BMC Surgery.

**Reviewer 1: Major Compulsory Revisions**

1. In order to minimize systematic bias, the authors developed a useful device for the excision of samples. Interestingly, the mode of excision (manual vs. automatic) does not seem to play a major role although it has been shown in a previous study that tissue damage depends on activation time and pressure. In the same study (Enam and Cuschieri, Annals of Surgery 2002, Vol. 237, 2, 186-191), which is also referenced in the manuscript, it has been demonstrated that the heat produced during activation of UC is proportional to the power setting. Therefore, it is surprising that the authors only used a single power level for each device (60W for ME or level 5, respectively, which is the highest level for UC). Thus, the conclusion that UC induces deeper necrosis as compared to ME is not supported by the data presented. Ideally, both systems, ME and UC, should be compared for histopathology by employing ‘dose-responses’ of power levels which cover the whole range of application of each system.

We agree that evaluating histopathological changes for different power level settings would have led to a more detailed assessment of both investigated devices for soft tissue dissection. However, to evaluate all possible power level settings for both devices by both methods would require a very high sample size. We therefore focused on those power level settings that are predominantly used in abdominal surgery. We specified this in our revised manuscript.
but since the used power level settings are generally accepted in abdominal surgery, we believe that this methodological focusing does not shorten the relevance of our findings for clinical routine and further improvement of soft tissue dissection devices.

**Minor Essential Revisions**

1. **Introduction:** The background (controversy about the advantages of UC over high-frequency electrosurgery with respect to safety, costs, operating time, etc.) should be pointed out in more detail. It is not entirely clear as to why this study was eventually performed and the Introduction basically lacks a hypothesis.

   We expanded the introduction and mentioned the controversy with respect to safety, costs and operating time. However, existing publications often focus on very special clinical scenarios making transfer to abdominal surgery questionable.

2. **Number of samples:** The authors describe that from each pig (n=16) 8 excisions were taken, which would add up to 128 samples in total. However, according to the Results section 100 samples were analyzed. What happened to the rest of the samples?

   As pointed out in the methods section manual excision was performed in all 16, but the automatic device was used only in 9 animals after validation experiments (data not shown). Table 1 also displays that for animal 10-16 no samples were harvested.

3. **The Results section is very ‘concise’ and might be expanded in a future version of the manuscript.**

   The results section presents all data that had been collected for the present study. Furthermore, we aimed to present our data as concise as possible.

4. **Figures/tables:** The resolution of the images is very low which makes it difficult to identify some details.

   Images were made using a high-resolution microscope camera. With the revised manuscript we uploaded a rearranged composite figure (figure 4) which previously might have been problematic due to image processing. Alternatively, we would be able to upload the images as separate figures by the editor’s discretion.

5. **Table 2 contains redundant information and should be omitted since the same data are displayed in Fig. 5.**

   We added table 2 to display the exact numbers but based on the reviewers recommendation we omitted it in the revised manuscript.
Reviewer 2: Major Compulsory Revisions

The depth of the necrosis caused by the two devices is pointed out well. However the depth of the necrosis is just one aspect in the decision making process of whether one prefers to use UC or ME. For the patients and the treating surgeons many other parameters of higher importance exist:
1. Is the local acute and chronic inflammatory response different?
2. What is the rate of wound breakdowns or infections?
3. Is there a difference in local fibrosis?

These questions should be addressed to make the paper more valuable. The conclusion that UC causes a deeper coagulation necrosis is already known and described in the current literature. The study lacks a practical guideline for the treating surgeon.

Our study was designed to only investigate the quality and depth of necrosis caused by the two different dissection devices as to the best of our knowledge there are no consistent data on this question in the literature. Unfortunately, based on our described experimental setup we were not able to investigate additional relevant but later occurring aspects of tissue alteration in particular inflammatory responses or induction of fibrosis. The harvested tissue samples were immediately paraffin-embedded and prepared for histopathological examination. We investigated only the separated tissue samples but not the healing site and therefore could not report on wound healing processes.

We believe that our data clearly show that UC cannot uncritically be recommended because in addition to well known advantages we could demonstrate a clear disadvantage and this might be relevant especially when nerve structures are close to the resection plane. However, more experimental and controlled clinical data are needed to generate valid recommendations for the clinical setting. We added this comment to the discussion in the revised manuscript.

The literature should be updated.

We added citations related to the reviewers’ comments to the references. Furthermore we re-performed an intensive literature search but could not identify new relevant publications on the depth of necrosis or other limitations of ultracision versus monopolar electrocautery.

Yours sincerely,

K. Homayounfar, MD