Author’s response to reviews

Title: Systematic review on the needle and suture types for uterine compression sutures: a literature review.

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Author’s response to reviews:

October 24, 2019
Hayley Henderson
Editor-in-Chief
BMC Surgery

Dear Editor:
Thank you for reviewing our manuscript titled “Systematic review on the needle and suture types for uterine compression sutures: a literature review.” The editor and reviewer’s comments were very helpful. We have revised our manuscript based on these comments. Our point-by-point responses to the reviewer’s comments are provided below.
We would like to take this opportunity to express our sincere gratitude to the reviewers who identified areas of the manuscript that needed corrections or modifications. We would also like to thank you for giving us the opportunity to resubmit a revised copy of the manuscript.

We hope that the revised manuscript will be suitable for publication as an Original Article in BMC Surgery. We look forward to hearing from you at your earliest convenience.

Sincerely,
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Editor’s Comments:
Please include a cover letter with a point-by-point response to the comments, describing any additional experiments that were carried out and including a detailed rebuttal of any criticisms or requested revisions that you disagreed with. Please also ensure that all changes to the manuscript are indicated in the text by highlighting or using track changes.

Thank you for your comments. As per your suggestion, in addition to revising the manuscript, we have included our point-by-point responses to the reviewer’s comments. We sincerely hope that the revised manuscript will satisfy the reviewers.

Reviewer 1’s comments:
Sahin Onur Güralp (Reviewer 1): This is an interesting and clinically relevant review about uterine compression sutures, the materials used and the thickness of the sutures.

Reviewer 1, Point 1
The conclusion of the review is overestimated. There is no significant difference between the No 1 and 2 groups considering all suture materials. However the authors conclude at the end of the text "UCSs with size 2 sutures seem to achieve better uterine preservation than with size 1 sutures." The authors must emphasise that the significant results belong only to 2 suture materials, Polyglactin 910 and catgut.

Thank you for your insightful comments. We completely agree with your assessment that the heterogenous nature of this study prevented a comparison of the efficacy of UCS between No.1 and No.2 sutures. In the revised manuscript, we have revised the comments and conclusion (lines 41, 45, 149 and 236).

Reviewer 1, Point 2
There are many kind of compression sutures and their modifications, such as B-Lynch, Cho, modified B-Lynch etc. How do the authors standardise the success rates of those different methods?
Thank you for your valuable comments. As you correctly pointed out, it is impossible to standardize the success rates of these methods. Therefore, we have assumed that UCS techniques are equally effective and show the same uterine preservation rate. We acknowledge that this can introduce a strong bias in our analysis; therefore, we have expressed this as a limitation of our study in the revised manuscript (line 207).

Reviewer 1, Point 3
How do the authors interpret the success rates, if some of the patients got also additional arterial ligation?

Thank you for your helpful comments. As you correctly pointed out, it is difficult to determine or exclude the effects of adjunctive procedures. Although we have listed the rates of adjunctive hemostatic procedures (without hysterectomy) in Table S1, we have also included this issue as a limitation of this study (line 220).

Reviewer 1, Point 4
86.9% and 93.5% are quite near to each other. Considering that many different physicians with various experience levels were included in this review, do the authors find these close rates clinically significant, just not thinking about statistics for a second?

Thank you for your comments. As you correctly pointed out, there was a lack of clinical significance between these data. Therefore, in the revised manuscript, we have deemphasized these findings and have modified the conclusion (lines 45, 186 and 236).

Reviewer 1, Point 5
It is not an excuse for me "not being able to differentiate the indications due to low number of patients" if the authors make such strong conclusions, which seem to include all types of indications. One may not evaluate every single indication but the authors may at least make a subgroup for uterine fundus atony and give the success rates. Otherwise, comparing the accreta cases with fundus atony does not seem to be appropriate.

We completely agree with your opinion and have accordingly revised the conclusion (line 236). A sub-analysis of cases of uterine atony and No.1 and No.2 Polyglactin 910 and catgut has been included (line 153). We did not observe significant difference in the rates uterine preservation. These results are depicted in revised Table 3 and Table S3 have been included the description in the main text.

Reviewer 1, Point 6
The difference between the success rates of Polyglactin 910 (which is used very often in Europe) No 1 and 2 is very striking (56.7% vs 90.2%). I think the authors may emphasise this difference, for sure, better by giving the success rates in the uterus atony subgroups.

Thank you for your valuable comments. The values (56.7% vs 90.2%) actually reflect the transfusion rate; therefore, in the revised manuscript, we have included a discussion regarding
the transfusion rate. (lines 165 to 178). Because we noticed errors when performing this analysis, it has been accordingly revised [Tables 2 and 3, transfusion rate (62.4% vs 79.1%)]. The P value remains unchanged. Furthermore, uterine atony cases have been included in Table 3. We have also revised Table 3 and included additional data in Table S3 and in the main text (line 153).

Reviewer 2’s comments:
Shigeki Matsubara (Reviewer 2): The theme is very important but, as you described, has been neglected. This study shed light on this important issue. There are much heterogeneity among patients (cases), techniques used, and also needles that were used. The authors partly solved this issue by focusing on the cases (reports) that they set, which is reasonable study design. Considering the heterogeneity mentioned above, no "definite" conclusion can be made: authors well understand this point and their conclusion is modest, just based on the present review.

Thank you for your positive comments on our manuscript. Although investigating the efficacy of different types of needles and sutures is required, the lack of randomized control and case-control studies with large number of patients is a significant limitation. A randomized control study investigating the efficacy of difference of needles and sutures is expected in the near future. Therefore, we have made several assumptions, resulting in biases and heterogeneity of the findings; these have been acknowledged as limitations in the revised manuscript. Despite these limitations, we believe that our study conveys useful information.

Reviewer 3’s comments:
Reviewer 3, Point 1
Well done methodology for a very difficult area to analyze. the number of reports and patients are not very extensive given the common nature of the problem. Furthermore details of management tend to be sparse.
The authors have determined about as much as possible given the limitations listed above. It is difficult to separate a number of related factors such as suture size and type, or surgeon/center and suture size and type and finally thresholds for transfusions and surgery including hysterectomy.

Thank you for your insightful comments.Although our manuscript has a lot of bias, we would appreciate your understanding of our study design. We have emphasized on the bias generated in this study and have presented it as a limitation (lines 207 to 225). Despite these issues, we believe that our study conveys useful information.

Reviewer 3, point 2:
The manuscript would be strengthened if the authors pointed out the weaknesses of this type of analysis, particularly sources of bias. For example there was a much higher transfusion rate ass'd with No 2 vs No 1 suture. Was this all pre suture? if so, then sicker patients, however if post suture, maybe the surgeon/centers that used No 2 were more aggressive in preserving the uterus. Maybe the use of hysterectomy is related to setting- lower resource vs higher resource?
Thank you for your helpful comments. As you correctly pointed out, the timing of transfusion is crucial. However, we could not estimate the timing of transfusion from previous studies due to the lack of description, which may be attributed to the lack of consensus of indication for UCSs. UCSs have been reported as life-saving procedures instead of hysterectomy at first; however, these procedures are now used universally and sometimes even for prophylactic use. We have added this discussion (lines 165-178).

A previous study that included the largest number of UCSs using No.1 sutures had performed “early” intervention for the indication of UCSs and had observed that the transfusion rate was only 9.3%. We speculate that performing UCSs in the less severe cases can lead to lower transfusion rates. In the revised manuscript, we have discussed the unclear timing of transfusion as a limitation and have also included data on transfusion rates (lines 131 and 222).

Reviewer 3, point 3
There are obviously many sources of bias that should be addressed. While the systematic review suggests the findings indicated, better studies—case controls, cohort, maybe even randomized studies provide better evidence.

Thank you for your helpful comments. According to your suggestion, we have included the description as follows (line 228): “In order to resolve these biases, better study designs, such as case-control and cohort studies, and perhaps even more robust studies, such as randomized control trials, are expected to provide better evidence regarding the needles and sutures used for UCSs.”