

Format for ANSWERING REVIEWERS

August 25, 2014



Dear Editor,

Please find enclosed the edited manuscript in Word format (file name: lqx-review.doc).

Title: Is Hand-Sewn Comparable with Stapler in Anastomotic Leakage after Oesophagectomy? A Meta-Analysis

Author: Quan-Xing Liu, Xu-Feng Deng, Jia-Xin Min, and Ji-Gang Dai

Name of Journal: *World Journal of Gastroenterology*

ESPS Manuscript NO: 12498

The manuscript has been improved according to the suggestions of reviewers:

1 Format has been updated

2 Revision has **been made according** to the suggestions of the reviewer

- (1) **Reviewed by 00003940:** This meta-analysis addresses an important question for oesophago-gastric surgeons. There are many minor English errors in the attached manuscript.
Answer: Thank you for your kind comments for our manuscript to World Journal of Gastroenterology. We appreciate your valuable comments and suggestions to improve it. We have reedited the manuscript for proper English.
- (2) **Reviewed by 00182276:** The topic is very interesting and it is a useful comparison. Language polish is required.
Answer: Thank you for your valuable suggestion. We have reedited the manuscript for proper English. We hope that the revised manuscript will leave you with a good impression.
- (3) **Reviewed by 00182538:** The authors meta-analyzed the incidence rates of anastomotic leak and stricture after esophagectomy between mechanical and hand-sewn esophago-gastrostomy adding new results from a few randomized trials after the meta-analyses reported previously for the same purpose. However, I am concerned about the following aspects. First, the authors should not have added the results from the randomized trial literatures which were not written in English (references #14 and #20). This is because most of readers cannot understand the original contents in these two literatures and cannot judge whether the literature selection for this meta-analysis was correct. Therefore, it is better for the authors to remove these two literatures from their meta-analysis. Second, the authors divided into two groups according the published year in the subset analysis. Why did the authors use 2003 to divide the two groups? What was it based on? The authors need to describe their rationale about this division in the Methods. The authors also need to interpret the result from this subset analysis in the Discussion. Finally, the authors also need to response to the following concerns. 1) Hand-sewnu HH as in author name in the Table 1 should be Hsu HH. 2) The authors should add the reference number in the Table 1. 3) There are duplicated references in #6 and #28. 4) References of #12 and #16 show the same results but written in two different languages.
Answer: We very much appreciate your careful review, constructive comments, and suggested corrections to our manuscript. Thank you for your suggestions and comments addressing the inadequacies of the manuscript. We have revised the manuscript according to your comments

and have incorporated all of the corrections. A detailed point-by-point response to your concerns is included below. First, although two randomized trial literatures included in our analysis were not written in English, they all met our prespecified inclusion criteria. To make sure the high quality for this meta-analysis, a rigorous study protocol was prespecified and several electronic databases, references, and international conference abstracts for relevant trials, were searched without restrictions on language. Second, This really does need some explanation. We have performed in the discussion section that “Third, as the technology of the stapler has been improved during the twenty years, and considered mature during the current decade, whether the different results of the outcome come from published year of the literatures is unclear.(references #14 and #24)”. Finally, duplicated references were corrected and the false written in table1 was corrected.

- (4) **Reviewed by 02546581:** This is a nicely written manuscript and the analyses seem to be well performed. The topic of the esophagogastric anastomosis is not really new, but it is still one of the mainly important problems in esophageal surgery. I have only minor issues to mention. - The authors should describe in more detail, why they performed this meta-analysis although there are several published before. What was the special aim in contrast to the others? - The table and figure references should be given in more detail in the results part, so the reader can easily find the mentioned analyses.

Answer: Thank you for your positive comments for our manuscript to World Journal of Gastroenterology. We appreciate your valuable comments and suggestions to improve it. Although recently several meta-analyses had undertaken to compare hand-sewn and stapler anastomosis methods revealed the results that there was no significant difference in the risk of developing anastomotic leakages, and the stapler anastomosis was more frequently contribute to the development of anastomotic strictures. Obviously, in the those meta-analyses some limited factors exists as follows: (1) not included all published RCTs (2) as the clearly clinical heterogeneity between all included RCTs several subgroup meta-analyses are needed. In our meta-analysis the largest number of RCTs were included in this study to date on this topic and through subgroup analyses we also examined the contribution of the site of anastomosis, the suture layer of hand-sewn method and the published year as effect modifiers. According to your suggestion ,we have given the table and figure references in more detail in the results part in the revised manuscript.

- (5) **Reviewed by 00182188:** The manuscript has interesting information and can be published, but I have some recommendations and some questions to be done. 1. Important corrections to the language (English) need to be made. 2. From the title and the abstract, we get the impression that the primary outcome is anastomotic leakage, but in the methods section two primary outcomes are mentioned (anastomotic leakage and 30-day mortality). Usually, one primary outcome is chosen. 3. Figure 1, in the eligibility phase, 14 articles were eliminated (6 because they were retrospective cohort studies and 6 because they were review articles). What about the remaining 2 articles, why were they eliminated? 4. Avoid repetition of results in the conclusion with the following suggestion: “This study revealed that there appear to be no significant difference in the incidence of developing anastomotic leakage, between the hand-sewn and the stapler group, however the use of a stapler method contributed to reduce the anastomotic leakage rate in the latest decade and is superior to the single layer hand-sewn in preventing postoperative anastomotic leakage”.

Answer: Thank you for your conscientious work. 1. We have reedited the manuscript for proper English. We hope that the revised manuscript will leave you with a good impression. 2. That is correct. According to your suggestion we have placed the content of 30-day mortality to the Secondary Outcome Measures section. 3. This really does need some explanation. Figure 1 was not performed correctly for different word software version, and we have reedited the figure1. You can find in the figure1 that the remaining 2 articles were letter which was eliminated. 4. This is a good suggestion. in the revised manuscript we have avoided repetition of results in the conclusion.

3 References and typesetting were corrected

Thank you again for publishing our manuscript in the *World Journal of Gastroenterology*.

Sincerely yours,

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