

Dear Editors and Reviewers:

Thank you for your letter and for the reviewer's comments concerning our manuscript entitled "digestive tract reconstruction options after laparoscopic gastrectomy for gastric cancer: a systematic review" (Manuscript Number: 47218). Those comments are all valuable for revising and improving our manuscript. We have studied comments carefully and made according revisions one by one which we hope meet with approval. Revised portion are marked in the paper. The main corrections in the paper and the responds to the reviewer's comments are as flowing:

Responds to the reviewer's comments:

Reviewer #1:

SPECIFIC COMMENTS TO AUTHORS:

Currently, no consensus exists regarding the best reconstructive procedure after laparoscopic gastrectomy (LG). However, the type of reconstructive procedure determines the quality of life (QoL) for patients with gastric cancer (GC). This paper systematically analyzed the reconstruction methods that may be used and included them into three categories according to the type of resection (laparoscopic distal, proximal and total gastrectomy). The authors highlighted the technical tips of every reconstruction procedure, and assessed also their surgical outcomes and postoperative complications. Therefore, this comprehensive review helps the gastrointestinal surgeons to understand the benefits and drawbacks of all the procedures. The randomized control studies included in this review were analyzed in order to bring more evidence regarding laparoscopic gastrectomy in the treatment of advanced gastric cancer, which is expected to encounter a period rapid development. The near future will probably clarify by means of large trials all the controversies regarding the reconstruction methods and the optimal choice of the reconstruction procedure and anastomotic device in every case of laparoscopic gastrectomy for gastric cancer. The authors conclude that the premise should always be radical gastrectomy and lymphadenectomy, and the reconstruction procedure should be selected to improve the QoL postoperatively by considering the safety, efficiency, minimal invasion, stability, and QoL. Finally, this systematic review is clinically helpful for surgeons, revealing the

merits and demerits of every reconstruction method, based both on literature and personal experience.

Response:

Thank for your positive comments on our manuscript.

Reviewer #2:

SPECIFIC COMMENTS TO AUTHORS:

I must emphasize that I am not an expert in the field of laparoscopic GC surgery/reconstruction, but have only a general insight. In my opinion - this is a valuable work, clearly presented, that is likely to be very informative for the readers. There are only 2 minor methodological points that need to be revised: The work is declared as a "systematic review of the literature" - therefore, the Methods section should contain the list of databases searched and the Results section should include the PRIMSA flowchart (hits identified; exclusion of duplicates; number checked through abstracts/number excluded; number retrieved in full text/number excluded - number of included reports) - these two points are standard elements in reporting systematic literature reviews. I have no other comments.

Response:

We appreciate the reviewers' positive constructive comments and suggestions. This manuscript was prepared for publication as a **Review** format.

Reviewer #3:

SPECIFIC COMMENTS TO AUTHORS:

The conclusion of this study was an indication that this study did not contribute to the literature. Abbreviations in subheadings should not be used alone. It must be written clearly before abbreviation. The working period is not specified. 2015 ...? . This is an important problem. The dates of the study should be clearly stated. The authors should explain why they did not include less than 10 patients in this study. So this choice was

made at the request of the authors? or is it based on the result from a statistical analysis? Reconstruction models should be compared with the control groups to determine which is better. No such comparison was made in this study. Therefore, the authors merely presented their own ideas in these studies. Therefore, this article should not be published as a systematic review. If it is to be published, it must be published in Review format. The authors did not include the articles published before 2015 and attributed this to high complication and learning curves. How did they calculate who completed the learning curve? For example, a surgeon who has been performing laparoscopic surgery for years is likely to complete the learning curve before 2015. Some writers I know belong to this group. In summary, this study may be published by other journals of the WJG series under review. Finally, the authors used the phrase "Acknowledgments" as follows: This work was supported by the Jiangsu Province Young Key Talents Program (QNRC2016673). Acknowledgment to Dr. Hui Zhang for every professional advice in statistics. First, I did not understand how the authors could have received support from the organization called Jiang Jiangsu Province's Key Provincial Young Talents Program için. Second, no statistical analysis method was used in this study. Therefore, I suggest that these statements be removed from the study

Q1. The authors did not include the articles published before 2015 and attributed this to high complication and learning curves. How did they calculate who completed the learning curve? The authors should explain why they did not include in this study. This article should not be published as a systematic review. If it is to be published, it must be published in Review format.

Response:

Thank you for your strict attitude and attentive work. This manuscript was prepared for publication as a **Review** and we revised the title of the manuscript accordingly in the revision.

Q2. Abbreviations in subheadings should not be used alone. It must be written clearly before abbreviation.

Response:

We carefully checked all the abbreviations in the manuscript and made modifications accordingly in the revision.

Q3. This work was supported by the Jiangsu Province Young Key Talents Program (QNRC2016673). Acknowledgment to Dr. Hui Zhang for every professional advice in statistics. First, I did not understand how the authors could have received support from the organization called Jiang Jiangsu Province's Key Provincial Young Talents Program için. Second, no statistical analysis method was used in this study. Therefore, I suggest that these statements be removed from the study

Response:

We have removed the corresponding statements in the revised version according to the reviewer's suggestion.

Special thanks to you for your good comments.

We tried our best to improve the manuscript and made according revisions in the manuscript. We appreciate for Editors/Reviewers' warm work earnestly, and hope that the correction will meet with approval.

Once again, thank you very much for your comments and suggestions.

Yours sincerely,

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