

Response to reviewers

Reviewer #1:

1. **Comment:** In the cover letter, “We believe, that our work will have a significant impact on clinical practice and a peer-reviewed high quality journal, like The Journal of Gastorintestinal Surgery is the perfect platform to present our results.”. “Journal of Gastorintestinal Surgery” is wrong.

Response: Thank you for drawing our attention to it!

Action: It has been rewritten for World Journal of Gastroenterology.

2. **Comment:** What was the rationale behind only include articles until 2019?

Response: This work was conducted in 2020 but got delayed due to the COVID-19 pandemic. Nevertheless, it truly carries a limitation due to this fact.

Action: We drew the attention to the time-frame of our search in the limitation section of discussion once again.

3. **Comment:** Some content fonts have inconsistent formats and should be changed.

Response: Thank you for noticing!

Action: We revised the manuscript accordingly.

Reviewer #2:

1. **Comment:** It would have been probably of interest to include few more studies in the analysis since those already included have a number of weak points.

2. **Response:** Thank you for your comment. At the time of the analysis, we included all studies, which met our inclusion criteria (randomized controlled trials, reporting on any outcome of interest). We made this decision to reach the highest achievable evidence concerning this question. Hopefully, more randomized clinical trials will be conducted to achieve a higher number of patients in the future!

Reviewer #3:

The authors conducted a network meta-analysis to assess the superiority of minimally invasive esophagectomy over transthoracic, transhiatal, hybrid or robot-assisted surgery. They found that minimally invasive technique significantly reduced the incidence of pulmonary infection, whereas non-open surgery (thoracoscopic, hybrid, and robot-assisted) required longer operative time. They did not find any significant difference in survival, total adverse events, cardiac adverse events, anastomotic leakage, atrial fibrillation, wound

infection, total pulmonary adverse events, vocal cord paralysis, or length of hospital stay and blood loss. They concluded that minimally invasive esophagectomy is only beneficial considering the decreased rate of pulmonary infection, and thus more trials and systematic analyses are needed for further assessment. This paper is well written, along with the sophisticated methodology using a novel network meta-analysis and well visualized figures. The manuscript is also well structured with sufficient presentations of the results. The strengths and weaknesses are adequately described in the discussion. The authors may consider the followings for minor points:

1. **Comment:** “Survival” means cancer-specific or overall?? Please define clearly in the manuscript.

Response: Thank you for your valuable comment! It is overall survival, which we will indicate in our revision.

Action: The manuscript has been revised accordingly

2. **Comment:** Title “superiority of thoracolumbaroscopy”: to be more consistent with the manuscript contents, “superiority of minimally invasive technique (or surgery)” may be considered as an alternative title.

Response: Thank you for your comment!

Action: The title has been rewritten to “The superiority of minimally invasive surgery”.