

Reviewer A

This is an interesting study regarding the important problem how to perform esophagectomy. The disadvantages of the study are - its retrospective character and relatively small number of patients.

I have several questions:

1. what were the criteria for selection patients to RAE, VAMIE or OE?
2. What was the distance from the tumors margins to the resection margins - both proximal and distal?
3. Was the intraoperative frozen section analysis of the resection margins performed?
4. What were the numbers of positive resection margins?
5. The more detailed technique of the anastomosis in the chest and the neck is necessary (linear or circular staplers? how the circular stapler was introduced to the chest in case of Ivor Lewis technique?)

We are very grateful to Reviewer for reviewing the paper so carefully. We have carefully considered the suggestion of Reviewer and have tried our best to improve and made some changes in the manuscript. Below, we will reply to your comments one by one.

Comment 1: what were the criteria for selection patients to RAE, VAMIE or OE?

Reply 1: We agree with your suggestion and have added corresponding descriptions in the paragraphs mentioned below.

Changes in the text: page 5, line141-143

Comment 2: What was the distance from the tumors margins to the resection margins - both proximal and distal?

Reply 2: We agree with your suggestion and have added corresponding descriptions in the paragraphs mentioned below.

Changes in the text: page 5, line143-146

Comment 3: Was the intraoperative frozen section analysis of the resection margins performed?

Reply 3: The point you mentioned is very important, and we have provided an answer in the text.

Changes in the text: page 5, line146-147

Comment 4: What were the numbers of positive resection margins?

Reply 4: The point you mentioned is very important, and we have mentioned the ratio of R0 resection in the table 3 (page 22) in the original manuscript.

Changes in the text: No changes.

Comment 5: The more detailed technique of the anastomosis in the chest and the neck is necessary (linear or circular staplers? how the circular stapler was introduced to the chest in

case of Ivor Lewis technique?)

Reply 5: We agree with your suggestion and have added corresponding descriptions in the paragraphs mentioned below.

Changes in the text: page 7, line204-206

Reviewer B

The authors have reported their analysis of postoperative outcomes in patients undergoing robotic versus laparoscopic/thoracoscopic versus open esophagectomy.

1. How were patients selected for a robotic versus laparoscopic/thoracoscopic versus open approach? There is a significant possibility of selection bias with a retrospective study such as this study.

2. Were there any conversions from minimally invasive (robotic or laparoscopic/thoracoscopic) to open? If so, were these patients analyzed as part of their intent to treat group or their as-treated group?

3. Can the authors categorize the costs into intraoperative versus postoperative costs?

4. It seems that there have been numerous studies addressing minimally invasive esophagectomy outcomes compared to open esophagectomy. How do the authors feel this study differs?

We are very grateful to Reviewer for reviewing the paper so carefully. We have carefully considered the suggestion of Reviewer and have tried our best to improve and made some changes in the manuscript. Below, we will reply to your comments one by one.

Comment 1: How were patients selected for a robotic versus laparoscopic/thoracoscopic versus open approach? There is a significant possibility of selection bias with a retrospective study such as this study.

Reply 1: We agree with your suggestion and have added corresponding descriptions in the paragraphs mentioned below.

Changes in the text: page 5, line141-143

Comment 2: Were there any conversions from minimally invasive (robotic or laparoscopic/thoracoscopic) to open? If so, were these patients analyzed as part of their intent to treat group or their as-treated group?

Reply 2: The point you mentioned is very important, and we have explained it in the corresponding position in the text.

Changes in the text: page 5, line138-141

Comment 3: Can the authors categorize the costs into intraoperative versus postoperative costs?

Reply 3: The point you mentioned is very meaningful, but unfortunately, as the original data only recorded the overall cost, we are unable to provide more detailed information on the usage of the cost. Based on the longer hospitalization time of the OE group and the higher total cost of the robot group, we have reason to believe that the high cost of the robot group is not due to patient bias.

Comment 4: It seems that there have been numerous studies addressing minimally invasive esophagectomy outcomes compared to open esophagectomy. How do the authors feel this study differs?

Reply 4: Your question is very valuable. As you mentioned, there have been many reports comparing two surgical methods before, but there is rare data comparing three surgical methods. It is worth mentioning that as a recent real-world study, our data includes some new adjuvant therapy cases that have been treated with immunotherapy combined with chemotherapy in recent years, which is lacking in the previously reported cases.

Reviewer C

In this study, the authors investigated the short-term outcomes between robot-assisted esophagectomy, video-assisted minimally invasive esophagectomy, and open esophagectomy for resectable esophageal cancer after neoadjuvant treatment. Comparison of the three types of surgery is important, but the number of cases in this study is small and this study is retrospective, so the selection criteria of surgical method is unclear, this has a large impact on the results. Since several randomized controlled trials of robotic surgery have reported, I think that this study has little significance.

Reply: We are very grateful to Reviewer for reviewing the paper so carefully. We have carefully considered the suggestion of Reviewer and have tried our best to improve and made some changes in the manuscript. As you mentioned, although several randomized controlled trials are currently underway or have achieved preliminary results, we also believe that this real-world data can provide a new perspective and have certain significance.

Reviewer D

Dear author, thank you very much for your manuscript. I read it with a great interest. Although interesting and very detailed, the data you provide as well as the limitation you pose, are not adding to the current body of literature. The previous studies you cited are basically very similar and already showed the differences and similarities in RAMIE, MIE, and OE. In addition, the rate of 3% of patients who received chemoradiotherapy making the data questionable as the gold standard is CROSS, FLOTT and not immunotherapy as majority of patients got.

Reply: We are very grateful to Reviewer for reviewing the paper so carefully. We have carefully

considered the suggestion of Reviewer and have tried our best to improve and made some changes in the manuscript. As you mentioned, in recent years, with the approval of immunotherapy drugs for first-line and neoadjuvant treatment of esophageal cancer, our report includes a lot of data on immunotherapy combined with chemotherapy. As a real-world study, we believe this can provide a new perspective.

Reviewer E

In this study, the authors compared short-term outcomes after robot-assisted esophagectomy (RAE) with those after video-assisted minimally invasive esophagectomy (VAMIE) and open esophagectomy (OE). They concluded that OE resulted in a longer hospital stay and the rate of successful right RLN node removal was higher with RAE. This manuscript is well written; however, this reviewer is concerned about the following points.

Major comments

1. What are the selection criteria to choose RAE, VAMIE, or OE for each patient? Historical change or the surgeon's preference? It needs to be described in the Methods.
2. It should be indicated in the Methods how to calculate the achievement rate of the RLN lymph node. The achievement of the RLN lymph node should be also defined in the Methods.
3. Since the authors compared the short-term outcomes after three surgical approaches, the patient's pathological data (Table 2) seem to be irrelevant and redundant. This reviewer recommends the authors to remove the data from this study. It should be shown in the next study comparing the long-term outcomes.
4. The patient characteristics (Table 1) needs the number of patients who underwent cervical LN dissection.
5. The operator's enough experience for RAE and VAMIE is essential for the stable surgical outcomes, which is known as "learning curve". The authors need to show the operator's experience for RAE and VAMIE before this study started.

Minor comments

1. Total cost should be indicated as in US dollar which can be easily understood worldwide.

Reply: We are very grateful to Reviewer for reviewing the paper so carefully. We have carefully considered the suggestion of Reviewer and have tried our best to improve and made some changes in the manuscript. Below, we will reply to your comments one by one.

Comment 1: What are the selection criteria to choose RAE, VAMIE, or OE for each patient? Historical change or the surgeon's preference? It needs to be described in the Methods.

Reply 1: We agree with your suggestion and have added corresponding descriptions in the paragraphs mentioned below.

Changes in the text: page 5, line141-143

Comment 2: It should be indicated in the Methods how to calculate the achievement rate of the RLN lymph node. The achievement of the RLN lymph node should be also defined in the Methods.

Reply 2: The point you mentioned is very important, and we have explained it in the corresponding position in the text.

Changes in the text: page 7, line226-228

Comment 3: Since the authors compared the short-term outcomes after three surgical approaches, the patient's pathological data (Table 2) seem to be irrelevant and redundant. This reviewer recommends the authors to remove the data from this study. It should be shown in the next study comparing the long-term outcomes.

Reply 3: The point you mentioned is very important, and we understand the reason for your point, however, as a study after neoadjuvant therapy, we believe that Table 2 can provide more comprehensive data. Therefore, after considering your opinions, we have decided to retain the data in this table.

Comment 4: The patient characteristics (Table 1) needs the number of patients who underwent cervical LN dissection.

Reply 4: We understand the reason for your suggestion, and we have carefully investigated the original data. A total of 12 patients (OE:6 VAMIE:2 RAE:4) underwent cervical lymph node dissection, and there was no statistical difference between the three groups, which is consistent with our preoperative staging. Due to the fact that the data for lymph node dissection is already reflected in Table 3, and the data in Table 1 can fully reflect the comparability among the three groups, we did not add this data to the table.

Comment 5: The operator's enough experience for RAE and VAMIE is essential for the stable surgical outcomes, which is known as "learning curve". The authors need to show the operator's experience for RAE and VAMIE before this study started.

Reply 5: The point you mentioned is very important, and we have explained it in the corresponding position in the text.

Changes in the text: page5, line136-138

Comment: Minor comments

Total cost should be indicated as in US dollar which can be easily understood worldwide.

Reply 6: We understand the reason for your suggestion, however, due to the continuous changes in exchange rates and the fact that this is a real-world study from China, after comprehensive consideration, we believe that using the Chinese yuan can most accurately reflect the cost situation.

Reviewer F

I read with interest the manuscript: Comparisons of short-term outcomes between robot-assisted, video-assisted, and open esophagectomy for resectable esophageal cancer after neoadjuvant treatment: a retrospective study.

The results are clear and the message is interesting and provocative.

I have following remarks:

Surgery:

Can the authors specify

how many patients had received a total MIE, how many VATS plus laparotomy

how many patients had received a total RAE, how many RATS plus laparotomy

The authors point out they have a limited experience with RAE. But in spite of this there was no significant difference in operating time; This is unusual because it is well known that RAE has a steep learning curve requiring at least 50 RAE to overcome the learning curve difficulties.

The authors have to, explain in the manuscript how the learning curve was overcome.

How many patients they had operated with RAER before entering patients in this study.

How was the learning process organized. First on simulators. Then how many cases with a certified proctor etc. This information is crucial.

Reply: We are very grateful to Reviewer for reviewing the paper so carefully. We have carefully considered the suggestion of Reviewer and have tried our best to improve and made some changes in the manuscript. For the points you mentioned, we have added a paragraph on the fifth page of the manuscript to elaborate on the relevant situation. We would like to emphasize that, prior to conducting this study, the surgical team had completed a large number of esophageal cancer resection surgeries, including OE, VAMIE, and RAE, with over 100 cases of RAE completed. In the VAMIE group, we only included the cases that successfully underwent combined thoracoscopic and laparoscopic esophagectomy. In the RAE group, any cases with conversions from minimally invasive to open were not included.

Reviewer G

Currently, many previous reports have demonstrated that MIS esophagectomy has some advantages compared to OE. In addition, comparison between VAMIE and RAE is currently ongoing prospectively. This report could not demonstrate any significances except for medical cost. The comparison OE is not required in current trends in esophageal surgery.

Reply: We are very grateful to Reviewer for reviewing the paper so carefully. We have carefully considered the suggestion of Reviewer and have tried our best to improve and made

some changes in the manuscript. With the approval of immunotherapy drugs for first-line and neoadjuvant treatment of esophageal cancer, our report includes a lot of data on immunotherapy combined with chemotherapy. As a real-world study, we believe this can provide a new perspective.

Reviewer H

This is a retrospective study that explores short-term outcomes among Robot-assisted esophagectomy(RAE) , video-assisted minimally invasive esophagectomy(VAMIE), and open esophagectomy(OE) for resectable EC after neoadjuvant treatment.

The achievement rate ($P=0.01$) and total cost ($P<0.001$) of right recurrent laryngeal nerve (RLN) lymph node resection were higher in the RAE group. Postoperative hospital stay was longer in OE than in the other two groups ($P<0.05$). However, there were no differences in other items including complications.

Based on these results, it is unreasonable to conclude that no clear benefit exists for either RAE or VAMIE in the treatment of resectable EC after neoadjuvant therapy. This is because, for the same reason, it cannot be said that OE also has a benefit in overall.

Reply: We are very grateful to Reviewer for reviewing the paper so carefully. We have carefully considered the suggestion of Reviewer and have tried our best to improve and made some changes in the manuscript. Based on your suggestion, we have described our conclusion in more detail: *Compared to VAMIE, no clear benefit exists for RAE in the treatment of resectable EC after neoadjuvant therapy. OE resulted in a longer hospital stay.* We hope this change can make our conclusion more accurate.

Changes in the text: page 2line57-60; page12 line396-397

Reviewer I

The authors of the study compared short-term outcomes between VAMIE, RAMIE and open esophagectomy after neoadjuvant therapy. They performed a retrospective study including 98 patients (31 RAMIE, 31 VAMIE and 36 OE) from a single-institution. They concluded that there are no clear benefits for either RAMIE or VAMIE and that total costs are significantly higher with the use of RAMIE.

This is indeed a relevant study. However, I believe the authors arrive to a strong conclusion with a study that has important limitations. For instance, the approach was defined intraoperatively which is associated with a strong selection bias of patients. In addition, there is a small number of patients in each group. Considering that costs is a main outcome of the

study, I recommend including in the methods section how costs were calculated (were admission days, pain medications, days off from work, etc. also considered in the analysis?).

Reply: We are very grateful to Reviewer for reviewing the paper so carefully. We have carefully considered the suggestion of Reviewer and have tried our best to improve and made some changes in the manuscript. Based on your suggestion, we have described our conclusion in more detail: *Compared to VAMIE, no clear benefit exists for RAE in the treatment of resectable EC after neoadjuvant therapy. OE resulted in a longer hospital stay.* As the original data only recorded the overall cost, we are unable to provide more detailed information on the usage of the cost. Based on the longer hospitalization time of the OE group and the higher total cost of the robot group, we have reason to believe that the high cost of the robot group is not due to patient bias.