

## Peer Review File

Article information: <https://dx.doi.org/10.21037/jtd-23-689>

### Reviewer A

In this study 69 patients undergoing open Ivor-Lewis esophagectomy were retrospectively compared: 43 had paravertebral catheters and 49 had thoracic epidural analgesia. Primary outcome was AUC pain scores in the first 48 hours.

The overall number of patients in this study appears to be low and therefore it is hard to draw solid conclusion based on this retrospective study. There are a few issues that need to be addressed.

1. Mention the number of patients and the primary outcome in the abstract.

Thank you for your comment. The primary outcome has been added as the final line to the background section of the abstract. The number of patients in the study has been added to the first line of the results section in the abstract.

2. This study is about open surgery. Isn't open surgery being replaced by minimally invasive surgery? Doesn't this limit the generalizability of this study?

The authors mention a RCT should be performed for open surgery, although a RCT is undertaken in minimally invasive surgery. Please comment on this.

The authors agree that advances in minimally invasive surgery have made minimally invasive esophagectomy (MIE) more feasible. That said, the MIE is a technically challenging operation. Many centers do not have the technical expertise and/or available equipment to consistently perform a MIE. Conversion to open or limited use of the MIE for technical reasons render the results of this study useful. The results of this study should give the surgeon and anesthesia team confidence that is a conversion to open is required post-operative analgesia will be adequate avoiding an epidural at the start of the operation. A comment to this effect has been added to the final paragraph in the discussion section of the paper.

A RCT for PVC versus epidural for MIE is being conducted as noted in our conclusion. We feel a similar study design would answer the same question for centers still performing open esophagectomy.

3. How were patients selected for either epidural or paravertebral catheters? Please comment on this

Thank you for your question. There were no specific patient selection criteria. Selection of the pain modality was at the discretion of the attending anesthesiologist and surgeon the day of the surgery, based on patient and surgical factors. Based our demographics, the two populations are comparable. 4 of the patients in the PVC group were conversions from MIS to open techniques. A comment to this effect has been added to the final paragraph in the discussion section of the paper.

4. Mention p-values in the text of the results section.

In the results section p-values have been added.

5. The overall numbers seem to be small. If there isn't any difference, this doesn't mean there isn't a difference. This needs to be addressed in the conclusions.

Thank you for your comment. The limitations of the study based on the small sample size and retrospective nature have been emphasized in the discussion and a line has been added to the final paragraph in the conclusion emphasizing this point.

6. Based on the above, the conclusion of the study sounds to firm and should be adapter. Also, the phrase "We believe paravertebral catheters should be strongly considered" sounds baseless.

Thank you for your comment. The line has been changed to "We believe paravertebral catheters should be considered"

## **Reviewer B**

Wang et al, present a well written study, comparing epidural and paravertebral catheter analgesia after open ivor Lewis esophagectomy. The study is well designed with a clearly described statistical plan. The results find that paravertebral catheters are non-inferior to epidural catheters in regards to pain scores, and are in fact, superior in regard to total opioid consumption. The limitations of this retrospective study are appropriately acknowledged in the manuscript as well. In this current era of enhanced recovery protocols after surgery, this paper is, quite timely and impactful.

I have only one minor comment, do the authors have any hypothesis as to why

hypotension rates are higher in the paravertebral catheter group? One would think that the opposite finding would be true. I think the manuscript would benefit from further discussion of this finding in the discussion.

Thank you for your comments and question. Ultimately, we do not know why more patients had hypotension in the PVC group compared to the epidural group. Similarly, to yourself, we hypothesized the opposite. Both groups of patients underwent similar post-operative monitoring. It is possible that with the advent of more judicious use of fluid intraoperatively that hypotension was a consequence of this practice change rather than the mode of regional anesthesia. A comment to his effect has been added to the discussion section of the paper.

Aside from this, I have no additional comments or suggestions.

### **Reviewer C**

The study is well conducted and thoroughly addressed. Congratulations to the authors, and I hope to be pleased to read the manuscript as soon as possible in this journal.

Thank you for reviewing our manuscript.