

Peer Review File

Article information: <https://dx.doi.org/10.21037/aoe-21-49>

**Reviewer A**

Thank you for the opportunity to revise our manuscript. Please find below responses to reviewer comments. We also would like to point out that the academic affiliation of Ammara Watkins is now the same the remaining authors and is reflected in the amended manuscript with track changes in the title page.

**Comment 1:** Lines 84, 85,86 and 87 appear to describe the potential mechanisms for para-conduit herniation; these need to be cited appropriately.

**Reply 1:** Thank you for bringing this to our attention. We have added references and for the hypotheses that do not have any data or citations, we have clarified the language.

**Changes in the text:** (Now line 96-99), text now reads: “Division of the crus during the initial esophagectomy may also increase the risk of para-conduit herniation (5, 7, 8). Another suspected but poorly studied hypothesis is that incomplete division of the gastrocolic ligament may effectively drag the colon up into the mediastinum with the conduit.

**Comment 2:** What is the surgeon's level of experience robotic surgery and his estimated operative volume?

**Reply 2:** The authors are from a high volume robotic foregut center which is a certified mentor site and center of excellence for robotic thoracic and foregut surgery.

**Changes in the text:** We have added line 145-147 which now reads: “All operations were performed at a high-volume robotic foregut center which is a certified mentor site and center of excellence for robotic thoracic and foregut surgery.”

**Comment 3:** Was a cost analysis (comparative) done as these repairs can also be safely and effectively performed via the laparoscopic approach.

**Reply 3:** No and this is beyond the scope of the manuscript.

**Changes in the text:** none.

**Comment 4:** Did the patients in this report fare better in terms of duration of surgery, length of hospital stay and post-operative complications (early & late) compared to patients in other studies that received the traditional laparoscopic repair?

**Reply 4:** Given the rarity of this complication, it would be difficult to study these outcomes in a comparative manner and likely require a multi-institutional study design. Our anecdotal experience is that the patients have equivalent outcomes to the laparoscopic repair and perhaps, the need for laparotomy and thoracotomy may be decreased given the superior optics and ease of high transhiatal dissection with the robot.

**Changes in the text:** none

**Comment 5:** Your conclusion does not drive a convincing message as to why the robotic technique is a superior technique over the conventional laparoscopic approach. This section of the paper needs to be re-written to stress the authors' take-home message.

**Reply 5:** Thank you for your critique. We have augmented the conclusion. The purpose of this manuscript was not to directly compare the robotic approach to the laparoscopic approach. This would not be appropriate in this relatively small series. Rather, the purpose is to highlight potential utility of the robot for these complex cases and this manuscript may provide a foundation for future comparative studies.

**Changes in the text:** Line 192-194 now modified to read: "The robot-assisted approach is a safe and effective minimally-invasive option for this complex problem."

## Reviewer B

I thank the authors for shedding light on a difficult clinical situation on which the literature is sparse. The manuscript is well written. I recommend accept for publication.

**Reply:** Thank you very much for your comments.

**Changes in text:** None.

## Reviewer C

This is a nice report of a clinical entity that can be quite problematic when encountered. It is particularly vexing in the early post-operative phase. The authors do a fine job of delineating the two types with appropriate clear illustrations. Two points that I believe should be made in the paper are:

1. Intra-operative evaluation with Indocyanine Green (ICG) is also available with commercially available non-robotic video towers.

2. Biologic or absorbable mesh graft should not be used as a bridge. Though this was not done in this case, it is worth while mentioning.

**Reply:** Thank you very much for your points. We have expanded on the possibility of intraoperative evaluation with ICG using non-robotic towers. We agree that biologic or absorbable mesh graft should not be used as a bridge and have expanded on the text to make this more clear.

**Changes in text:** Line 174-176 now read: “Notably infrared imaging is available for non-robotic video towers; however, this feature is typically not a standard feature and may require add-on capital expenditures.”

Line 158-162 now read: “As expected, absorbable mesh has high recurrence rates and should not be used as a bridge. For these reasons, if mesh is required, we prefer not to place mesh directly on the gastric conduit; rather, we perform a diaphragmatic relaxing incision lateral to the crus and cover the incision with non-absorbable mesh reinforcement.