Peer Review File
Article information: https://dx.doi.org/10.21037/jtd-23-37

Reviewer A

Comment 1: I suggest the addition of a chart displaying the total population in the study and its division into ILE vs THE/ME, as well as the rates of CL-NL. This would make the comprehension of the patients in each group much easier."
Reply 1: Thank you for this suggestion. We have added Figure 1 to our paper which details the total population and its division into ILE and THE/ME.

Comment 2: It is also important to note that the manuscript only mentions the use of esophageal stenting in the management of chest leaks and does not mention endovac therapy. While stenting has been a commonly used method for managing chest leaks, endovac therapy has emerged as a promising alternative with several advantages, such as potentially reducing the need for surgical intervention which is a variable in this manuscript."
Reply 2: Thank you for this comment. We have added information on EndoVAC therapy in our discussion in addition to esophageal stenting as an intervention for intrathoracic leaks.

Comment 3: Lastly, the authors start the Results section by presenting the data collected in percentages followed by the total N number in parentheses. However, it may be useful for the reader to have the total N in every section of the manuscript.
Reply 3: We have added the total N number in addition to the percentages for the data presented in the results section.

Reviewer B

Comment 1: The manuscript does not correspond to the title. The main outcome is insufficient for the clinical question. In the data, the management after leakage was the difference between chest leak vs neck leak. This should be much better described and clarified.
Reply 1: We disagree with this comment regarding the title as we have titled our manuscript "Chest vs Neck Anastomotic Leak Post Esophagectomy for Malignancy: Rate, Predictors, and Outcomes" where we report on the rate, predictors, and outcomes of anastomotic leak post esophagectomy. Therefore, we have not made any changes to the title of our manuscript.

Comment 2: The results are difficult to follow to lead to the main outcome.
Reply 2: We have edited the results, specifically the intervention vs non-intervention section in Table 5 to help clarify the intervention results.

Comment 3: Respiratory complications were main complications after esophagectomy. However, anastomotic leak is main focus of this study. Thus, the influence of respiratory complications should be adjusted in the analysis.
Reply 3: Thank you for this comment. In Table 6, following multivariable regression, there was not a significant difference in odds between chest leak and neck leak. Therefore, we did not adjust for respiratory complications elsewhere.

Comment 4: Surgeon specialty is a significant deference between Chest and neck leak after esophagectomy. The influence of surgeon specialty should be carefully described in the discussion part.
Reply 4: Thank you for this comment, although we have demonstrated a significant difference with surgeon specialty between chest and neck leak on primary comparison in our supplemental table, surgeon specialty was not found to be an independent predictor of chest or neck leak on multivariable regression.

Reviewer C

Comment 1: The authors put emphasis on the result that chest leaks were associated with greater odds of needing an intervention than neck leaks. However, the data about interventions for leaks were not provided in the section of results in the abstract. I think it would be better if the authors show the data about interventions in the abstract.
Reply 1: Thank you for this suggestion. We have added this information to the results section of the abstract.

Comment 2: I do not think National-Surgical-Quality-Improvement-Program data have leak severity data. But the severity is very important when it comes to the difference between chest leaks and neck leaks. I think it will be interesting to only compare severe chest leaks with severe neck leaks.
Reply 2: Thank you for this comment. Unfortunately, as this comment states, leak severity is not included in the NSQIP database. We have included a sentence in our limitations to detail this.

Comment 3: According to the section of methods, the authors performed one-way ANOVA or Kruskal-Wallis test to compare the three groups. However, I do not think it is a good idea to include the total group as the third group. The authors should delete the total group from all the tables and only compare two groups (for example, chest leaks vs neck leaks). All the tables with the total group look busy.
Reply 3: Thank you for this comment. We have deleted the total group from all of our tables to make them less busy. We have also edited our methods section to demonstrate that we used Wilcoxon signed-rank test for our continuous variables.

Comment 4: I see the following sentence and do not agree with it. "The total operative time was longer in ILE when 231 chest leaks occurred compared to THE or ME when neck leaks occurred". Isn't this comparison necessary? Isn't this significant due to the difference itself in surgical approaches?
Reply 4: Thank you for this comment. We have deleted the operative time comparison from Table 5 and subsequently removed the associated text from the manuscript.

Comment 5: Regarding interventions for leaks, I propose that the authors divide the patients into two groups, not documented/non-invasive intervention and invasive intervention/reoperation. Otherwise, the authors should use the fisher's exact test or Chi squared test with corrections to identify which comparison is significant. For instance, there was a significant difference only between reoperation and non-invasive intervention when comparing chest and neck leaks.

Reply 5: Thank you for this comment. We have edited the management section of Table 5 to only include intervention vs non-intervention as reoperation is additionally mentioned in this table. We performed a fisher's exact test to analyze the updated management data in Table 5 and reported the outcomes in the text of the manuscript.

Comment 6: The authors should provide the definitions of non-invasive interventions and invasive interventions in the section of methods.
Reply 6: Thank you for this comment. We have edited the verbiage in the manuscript to represent the wording used by NSQIP – interventional vs non-interventional. Unfortunately, NSQIP does not further detail inclusion criteria for these different groups.