Peer review file

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Reviewer A

In this study, the authors present a single surgeon's experience with VATS lobectomy. They present an individual surgeon's learning curve with 189 cases, starting with multiportal VATS and then shifting to uniportal VATS. They show that as time progresses and experience is accumulated, operative time and postoperative air leaks decrease; furthermore, the introduction of uniportal VATS during this time did not negatively impact results. The authors should be commended on their excellent outcomes and their successful transition to a technically challenging but clinically rewarding approach to lobectomy. The manuscript is clear in its scope, well-written, and the methodology is sound. I do however have some concerns, outlined below:

Comment 1: I think the title of the manuscript should be reconsidered. It is grammatically inaccurate and does not fully convey the essence of the manuscript, which is a single center's learning curve with thoracoscopic surgery and successful implementation of the uniportal VATS technique.
Reply 1: Thank you for your excellent suggestion, and I agree with your opinion. Therefore, I changed the title to "The learning curve of thoracoscopic surgery in a single surgeon and successful implementation of uniportal approach".
Change in the text: The title is changed.

Comment 2: In the first sentence on page 4 line 63, I would recommend including the year of publication of Gonzalez et al's uniportal publication to give the reader better context. There is also literature dating back to 2004 by Rocco et al, describing even earlier experiences with uniportal wedge resection (PMID: 14759479).
Reply 2: Your suggestion is reasonable. I added the year of publication of Gonzalez's first report of uniportal thoracoscopic major pulmonary resection. Moreover, Rocco's report for uniportal thoracoscopic wedge pulmonary resection was also added because this report is also important in uniportal approach as you insisted.

Change in the text: They are added in lines 61-63 on page 5 of the revised manuscript.

Comment 3: How was the data collected for this study? Was it a prospectively maintained database or was it pulled from individual charts by an author of the study, or a data custodian? Some further explanation would help to explain the limited variables presented in the data.
Reply 3: Your question is very reasonable. I added the explanation of data collection.
Change in the text: The explanation of data collection was added in lines 93 on page 6 of the revised manuscript.

Comment 4: There should be a statement of IRB approval and/or waiver for informed consent due to the retrospective nature of the study.
Reply 4: I apologize confusing location of IRB approval. This was mentioned in the Footnote section. Moreover, the sentences "The study was conducted in accordance with the Declaration of Helsinki (as revised in 2013). The study was approved by institutional ethics board of Japanese Red Cross Maebashi Hospital (NO.: 2021-13) and individual consent for this retrospective analysis was waived." were added.
Change in the text: The sentences "The study was conducted in accordance with the Declaration of Helsinki (as revised in 2013). The study was approved by institutional ethics board of Japanese Red Cross Maebashi Hospital (NO.: 2021-13) and individual consent for this retrospective analysis was waived." were added in lines 87-89, on page 6 of the revised manuscript.

Comment 5: How long was the author/surgeon in practice before adopting thoracoscopic techniques? Did they exclusively perform open thoracotomy for all lobectomies prior to 2012? Were there still some cases that were performed via open thoracotomy during this time? Were there conversions from VATS to open? A general context of the surgeon's experience prior to the adoption of VATS would help to provide context for the reader.
Reply 5: Thank you for your suggestion. I added my surgical experiences about pulmonary lobectomies.
Change in the text: The sentences "Before the study period, the author (HI) adopted mini-thoracotomy approach. The author (HI) has the experience of performing about

150 pulmonary lobectomies via mini-thoracotomy approach." were added in lines 93-96 on page 6 of the revised manuscript.

Comment 6: I would recommend eliminating the frequent references to the author's self when describing the study as a single surgeon's experience.
Reply 6: Thank you for your suggestion. I totally understand your recommendation. Therefore, some of references our team previously reported were withdrawn.
Change in the text: Some of references our team previously reported were withdrawn in the revised manuscript.

Comment 7: I am not thoroughly convinced of the assertion that uniportal VATS is superior to multiportal VATS from the data that is presented. While existing literature supports this claim, and the statistical analysis does show significantly significant results, how can we determine that this is not simply a result of the single operator becoming a better surgeon over time? This is a major confounder and should be mentioned as a limitation of the paper.
Reply 7: I totally agree with your opinion. I would like to insist that introduction of uniportal approach did not deteriorate the learning curve of a single surgeon in thoracoscopic pulmonary lobectomy. Therefore, I added the sentence that this manuscript did not mean uniportal approach is not superior to multiportal approach.
Change in the text: The sentences "Moreover, we would like to insist that this manuscript described the learning curve of a single thoracic surgeon and successful implementation of uniportal approach, which was different from the superiority of uniportal approach compared to multiportal approach." were added in lines 256-259, limitations section, page 14 of the revised manuscript.

Comment 8: An issue I have with the propensity match employed by the authors is that the only patient demographic information present is age, sex, location of tumor and histology/pathology. Were the authors able to account for relevant comorbidities such as BMI, which is a known pre-operative predictor of post-operative air leak? Or other comorbidities that could lead to morbidity and mortality (diabetes, hypertension, history of stroke or heart attack, etc.). I do not think that this takes away from the overall message of the manuscript, but it is another serious limitation that should be

acknowledged, and this comparison should probably not be emphasized as a main finding.

Reply 8: Thank you for your suggestion. I think your opinion reasonable. However, our medical record was lack of data including BMI or preoperative comorbidities. Therefore, the sentences about the lack of those data were added in the study limitation.

Change in the text: The sentences “In addition, although the patients’ characteristics were matched based on the propensity score to address the imbalance in sample size and potential intrinsic differences between the two groups, the data were lack of other variables including body mass index or preoperative comorbidities which might affect the perioperative results.” were added in lines 252-255 on page 14 of the revised manuscript.

Reviewer B

Thank you for allowing me to review the manuscript. It is an interesting evaluation of the learning curve for different minimally invasive approaches to lobectomy. I have a few comments and questions below:

Comment 1: I congratulate on the well formulated and strategic phased role out plan for introduction of a new and technically difficult technique. Does this mean prior to the role-out (2012?) there was no minimally invasive thoracic surgery being performed at your institution?

Reply 1: Thank you for your question. Our team adopted mini-thoracotomy approach as minimally invasive thoracic surgery. The skin incision was 5 to 8 cm via the approach. These sentences were added in the revised manuscript.

Change in the text) These sentences were added in lines 93-96 on page 6 of the revised manuscript.

Comment 2: Can you please define a ND2a-1/2 lymphadenectomy for those who are not familiar with the terminology?

Reply 2: Thank you for your suggestion. We added a reference describing ND2a-1/2 lymphadenectomy, which was reported by Okada et al. in 2006.
Change in the text) Okada's report was added in reference No. 6.
In addition, the sentence describing ND2a-1/2 lymphadenectomy was added in lines 154-156, on page 9 of the revised manuscript.

Comment 3: Why were the additional 187 patients of the 376 total not enrolled in the study? Did they not undergo the same operation? Does this add a selection bias to the study? Please elaborate.
Reply 3: Thank you for your question, and I apologize my confusing expression. Other surgeons performed thoracoscopic lobectomies for the additional 187 of the 376 patients during the period. This manuscript demonstrated the learning curve of a single surgeon's experience. To avoid the confusion, the title was revised.
Change in the text) The title is changed.

Comment 4: Your rate of PAL following the phase 2 multi-port group seemed very high. What changes did you make to bring the rate of PAL down so significantly? Do you approach the fissure more frequently with multi-port compared to uniport? Any other technical differences that could account for this change in PAL?
Reply 4: Your question is very reasonable. I consider that the adoption of fissureless technique had the most important role in reducing the rate of PAL. Additionally, to perform fissureless technique appropriately, a highly experienced surgeon is required because the surgical steps are different from those of the conventional technique, in which the pulmonary arteries are dissected and exposed at the fissure. These sentences were mentioned in lines 219-226, on page 12-13 of the revised manuscript.
Change in the text) No change.

Comment 5: Based on your study, do you think there is an advantage to one technique over another once the learning curve has been overcome?
Reply 5: Thank you for your question. I think there are some advantages of introducing the new technique even after the learning curve has been overcome in a certain one if the new technique is less invasive for the patients.
Change in the text) No change.