Peer review file
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Reviewer A

Comment 1: Is there a particular reason for the cut off dates from January 2017 to May 2020?

Reply 1: This study aimed to assess contemporary practices in surgical lung resection at our institution and we filtered through our institutional database based on the inclusion criteria of the manuscript. The patient data that was available started in January 2017 and the end date of May 2020 was based on when our data collection was completed to prepare the abstract/manuscript for the study.

Comment 2: In the conclusion, there was a trend towards respiratory complications that was not statistically significant. Was the study statistically underpowered to achieve its aim?

Reply 2: We thank the reviewer for bringing up this important point. Yes, as the reviewer states, there was a trend towards respiratory complications that was not statistically significant since the study was not powered to identify this difference. We have briefly mentioned this in line 224-226 on page 11. We also made the following addendum in the manuscript:

Line 193-200: Future studies with a larger patient population or a multicenter study may help elucidate these differences

Line 283-290: The small sample limited the ability to propensity match between the marginal lung function and normal lung function group to assess composite outcome and survival. Although propensity match was not performed due to small sample size, a logistic regression model was used to adjust for important confounders.

Comment 3: Do you have further data available for the prolonged air leak/? What percentage was treated conservatively vs requiring pleurodesis / reoperation?

Reply 3: We thank the reviewer for this important point. The majority of patients with air leak did not require further intervention/pleurodesis and were sent home with a portable chest tube drainage system. Intervention was defined as mechanical or chemical pleurodesis, endobronchial valve placement, or surgical resection. 1/16 of marginal lung function patients underwent intervention for prolonged air leak and 2/25 patients underwent intervention in the normal lung function group.with no statistical difference between the two groups. We have made an addendum to include this in our manuscript.

Line 68-69: Intervention for prolonged air leak was defined as mechanical or chemical pleurodesis, endobronchial valve placement, or operative resection.

Line 112-117: The majority of patients that had prolonged air leak underwent conservative management with continuation of a chest tube. 1/16 (6.3%) of marginal lung function patients with an air leak underwent intervention with chemical pleurodesis and 2/25 (8.0%) patients with normal lung function underwent intervention ($p>0.999$), one patient with chemical pleurodesis and one patient with endobronchial valve placement.

In addition, the following addendum was made to Table 2:

Table 2:

	Marginal lung function (n=88)	Normal lung function (n=212)	p-value
Prolonged air leak	16 (18.2%)	25 (11.8%)	0.142
Intervention for prolonged air leak	1/16 (6.3%)	2/25 (8.0%)	0.999
Pneumothorax	4 (10.5%)	9 (12.7%)	0.741
Pneumonia	2 (5.3%)	5 (7%)	0.718
Pleural effusion	0 (0%)	1 (1.4%)	0.462
Tracheostomy	0 (0%)	2 (2.8%)	0.296
Pulmonary embolus	1 (2.6%)	1 (1.4%)	0.955
Ventilator support >48 hours	0 (0%)	1 (1.4%)	0.462
Respiratory failure	2 (5.3%)	1 (1.4%)	0.241
Other pulmonary events	1 (2.6%)	1 (1.4%)	0.650

Table 2: 30-day respiratory complications

Comment 4: Regarding the Kaplan - Meier survival curve, do you have additional data on the follow up of the patients? i.e. average length of follow up, how many patients lost, as the drop off rate seems quite high.

Reply 4: We thank the reviewer for this important observation. **The median follow-up was 699 days. The percent follow-up at 30 days, 90 days, 365 days, and 3 years was 99.2%, 95.3%, 81.3%, and 15% respectively. Since the collection dates were between January 2017 and May 2020, there is still limited data in the long-term follow-up, which will need to be addressed in future studies. We have made the following addendum to our manuscript to reflect this important point.

Line 129: median follow-up was 699 days

Line 200-202: Our data is a contemporary study that used the most recent data available at our institution, thus we had limited long-term follow-up data for the more recent surgical patients

Comment 5: The weaknesses of the paper have been well highlighted by the authors. Retrospective cohort data has inherited bias as those selected to surgery must pass the surgeon's "eye ball test". Further elaboration on how VATS is performed in your center would be beneficial: How many surgeons were involved in the study? Was the VATS technique (i.e. 3-port VATS, 2 port VATS, 1 port VATS/non-intubated) for consistent enough to draw the conclusions in the study?

Reply 5: We thank the reviewer for this comment. Two surgeons were primarily involved in the study in the timeframe of data collection for this study. One surgeon primarily performed resection via VATS, whereas the other surgeon primarily performed resection via RATS. VATS was generally performed with 4 ports for lobectomies and segmentectomies and 3 ports for wedge resections. Since there were only two primary surgeons for all patients in this study, there were very little deviation in surgical technique and post-operative care amongst the patients. We have made the following addendum to the manuscript.

Line 53-58: Surgical resection was performed by two surgeons and the technique for VATS and RATS was consistent throughout the study. VATS was performed with 4 ports for anatomic resections and 3 ports for wedge resections. RATS was performed using 4 robotic ports or a 5th port for a robotic stapler for anatomic resections and 4 robotic ports for wedge resections.

Reviewer B

Comment 1: The article has a heterogeneous group of procedures. Definition of marginal lung function of < 60% would not be really marginal for wedge resections (or even for most simple segmentectomies). I do see the range does go down to 18% for DLCO and 28% for FEV1, those would be best to avoid surgery, and perhaps receive SBRT or ablation instead. The range of lung function in the poor lung function group is big making interpretation of results / data difficult.

Reply 1: We thank the reviewer for this comment. As the reviewer astutely points out, marginal lung function as defined as FEV1 or DLCO <60% were based on guidelines published by ACCP, ERS/ESTS and BTS in the early 2000's that were mainly based on open surgical resections. We continued to use this definition as the inclusion criteria to stress the difference in outcomes between open thoracotomy and minimally invasive thoracic surgery. The significant difference in the type of surgical resection performed between the marginal and normal lung function group demonstrates the importance of surgeon judgement in the type of surgical resection that is offered to patients with low pulmonary function. Figure 1 in our manuscript demonstrates the increasing trend of adjusted composite outcome by deciles of preoperative FEV1 and DLCO. In order to further delineate a "cutoff" value of DLCO and FEV1 in patients undergoing minimally invasive thoracic surgery, studies that include larger number of patients with marginal lung function in different ranges (ie: 20-30%, 30-40%, 40-50% etc) will be needed and can be an important aim for future studies. The following addendum has been added to our manuscript.

Line 239-243: In addition, there have been limited studies assessing the appropriate "cutoff" value in FEV1 or DLCO for minimally invasive lung resection and the appropriate surgical procedure for each marginal or "high risk" patient based on pre-operative lung function values. Larger studies to power for each range of FEV1 and DLCO can help delineate these differences.

Comment 2: The mortality of 4-5 % at 90 days for VATS resection that encompasses wedge, segmentectomy and lobectomy (excluding pneumonectomy) seems high. Since the cases seem to have a diagnosis of cancer, I would assume these cases were mostly / all elective procedures.

Reply 2: We thank the reviewer for this observation. Our institution has a high volume of complex patients wherein approximately 1/3 of all of our patients who underwent minimally invasive thoracic surgery had marginal lung function with associated high baseline co-morbid conditions. Thus, the reported 90-day mortality may be slightly higher than expected. A 2015 SEER database analysis that compared 90-day mortality between SBRT and surgical resection (lobar or sublobar resection) reported a 6.1% mortality rate for the surgical group (reference 12). Moreover, many similar studies did not report 90-day mortality; however, 30-day mortality reported from previous studies that include patients with marginal lung function has been similar to our 30-day mortality of 1% and included in our manuscript in line 194-195. The following addendum was made to the manuscript.

Line 178-181: Our 90-day mortality of 5.7% and 4.2% for marginal lung and normal lung function, respectively, are similar to 90-day mortality of 6.1% following lung resection from the SEER

database (12). The 90-day mortality is not frequently reported in literature but we included it in our study to report the trend.

Comment 3: I would expect some kind of propensity matching would be required between the groups.

Reply 3: We thank the reviewer for bringing up this point. In this study, we did not propensity match between the two groups based on baseline characteristics due to inherently higher co-morbid conditions of patients with marginal lung function. Inclusion of high risk patients in this study shed light into plausibility of surgical resection of this group of patients in the setting of minimally invasive surgery and we would like to emphasize the importance of shared decision making in this patient population. In addition, propensity match was not performed due to our small sample size. We addended the manuscript to address this point.

Line 283-290: The small sample limited the ability to propensity match between the marginal lung function and normal lung function group to assess composite outcome and survival. Although propensity match was not performed due to small sample size, a logistic regression model was used to adjust for important confounders.

Comment 4: When survival is discussed, again the groups need to be matched, LN status , tumour subtype, adjuvant therapy data etc … would be required.

Reply 4: We thank the reviewer for this comment. Propensity match was not performed due to the limited sample size. Logistic regression model was used to adjust for important confounders. We addended the manuscript to address this point.

Line 283-290: The small sample limited the ability to propensity match between the marginal lung function and normal lung function group to assess composite outcome and survival. Although propensity match was not performed due to small sample size, a logistic regression model was used to adjust for important confounders.

Comment 5: By grouping together VATS and RATS, it again makes the message less clear and more difficult to interpret. Were same number of ports used ? Operation duration same etc ….?

Reply 5: We thank the reviewer for this important comment. Two surgeons were primarily involved in the study in the timeframe of data collected for this study. One surgeon primarily performed resection via VATS, whereas the other surgeon primarily performed resection via RATS. Since there were only two primary surgeons for all patients in this study, there were very little deviation in surgical technique amongst the patients with same number of ports used for cases. Please see our response to Reviewer A, response 5 and the addendum that was made regarding this point.

Reviewer C

I would like to congratulate the authors of an interesting article on lung resection in patients with normal and limited lung function.
The topic raised by the authors is currently of great importance in qualifying patients for various types of oncological treatment and is important and interesting for thoracic surgeons.
The article is written in very good quality English, in full compliance with the Journal's guidelines.
The only weakness of the article is the difference in the extent of surgery between groups of patients, which can affect both early and long-term outcomes. However, I think that this does not negate the

main message of the article, i.e. that patients with low lung function should also be considered for surgery. This should be discussed in a bit more detail in the limitations of the study.
Besides, I have no significant comments and I would like to congratulate the authors of this interesting study once again.

I can recommend the paper for publication in the JTD after minor revision.

Reply: We thank the reviewer for their kind review of the manuscript. In line 203, we addressed the significantly lower rate of anatomic lung resection in the MLF cohort but with no associated difference in mortality or composite outcomes. However, no concrete guidelines exist to determine which type of resection is associated with best outcomes for the MLF patient population. We have included the following addendum in the limitations section to address the issue.

Line 277-281: the type of surgical resection that was performed, namely lobectomy, segmentectomy, or wedge resection, were determined by surgeon judgement based on patient baseline parameters and preoperative lung function. More objective guidelines for determining the surgical approach based on patient lung function is a target for further studies.