

Peer review file

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Reviewer A

In this submission, the authors compared patients who received U-VATS(n=161) and 3P-VATS(n=71) for PSP surgery. They found that perioperative results and recurrence rate did not differ between the 2 groups.

The following are further points for the authors to consider:

Comment 1: There was no difference in the primary outcome measures. Could this be a type 2 statistical error? Have the authors done a sample size estimation to see if this study is adequately powered to show a difference? The recurrence rate after surgery is very low anyway, and the number of patients in one study arm is 71, so is this enough to show a difference?

Reply 1: Your question is very reasonable. Therefore, the limitation was added about the statistical power.

Changes in the text: We added the possibility of low statistical power to the limitation on page 14, line 284 to 286.

Comment 2: More patients in the 3P-VATS group were followed up at 1.5-2 years according to Fig 2. Does this bias in favor of U-VATS?

Reply 2: Your comment is very reasonable. In my complete error, I didn't notice the mistake in attaching the Figure 2 file until pointed out. I exchanged to the confirmed exact file.

Changes in the text: We attached the correct Figure 2.

Comment 3: Significantly more patients in the 3P-VATS group had single bulla, yet single bulla was a significant factor for lower recurrence. Does this bias the primary outcome results?

Reply 3: Thank you for your suggestion. In this study, multivariate analysis was used to investigate the independent factor for the recurrence of pneumothorax. Therefore, the bias was minimized.

Changes in the text: No change.

Comment 4: As per usual practice in Japan, the authors do not perform any pleurodesis. This is stated in the methods and discussed in lines 260-268. However, because it is quite different from practices in most other parts of the world, this point needs to be maybe emphasized more in the Discussion. This study finds non-inferiority in terms of bullectomy. But it doesn't really show non-inferiority in terms of pneumothorax surgery including pleurodesis.

Reply 4: Your suggestion was very responsible. Therefore, the sentences about the mechanical pleurodesis performed worldwide were added in the discussion section.

Changes in the text: We have modified our text as advised (see page 13, line 268 to 276).

Comment 5: In lines 100-103, the authors made a presumption of 'superiority' of U-VATS in terms of invasiveness to declare 'usefulness' of this approach if recurrence rates were similar. This statement shows a potential bias on the part of the authors. They should remain neutral when conducting a study such as this, and not assume anything before staring. They should at least remove the bias wording, provide evidence of U-VATS 'superiority' (if this exists), and explain what they mean by 'usefulness'. ("usefulness" isn't normally a product of less invasiveness or recurrence rates) The word "useful" is again used in line 274, and maybe this isn't the right word to use in this context.

Reply 5: Thank you for your suggestion. We corrected the sentence you insisted.

Changes in the text: The sentence "more useful than" was changed to "an alternative approach of" on page 4, line 87 in the revised manuscript. Additionally, the word "useful" was changed to "alternative" on page 14, line 282 in the revised manuscript.

Comment 6: In line 110, the authors mention the use of CT. Was CT done in all patients? This is not recommended in most guidelines, and not normal practice in most regions. Are patients who meet the other criteria for surgery then not offered surgery if CT was not done, or CT showed no lesions?

Reply 6: Your comment is reasonable. In our institution, all patients with pneumothorax received CT to examine the existence of bulla or blebs because it was easy to be

performed based on the accessibility although this practice was different from other guidelines. The surgical indications were mentioned on page 5, lines 95 to 98. All patients in this study met the criteria.

Changes in the text: No change.

Comment 7: Prior to 2010, what approach was used for PSP surgery? Is it possible that even the 2010-2012 experience was also a learning curve for 3P-VATS?

Reply 7: Your question is reasonable. Three portal approach was adopted prior to 2010. Therefore, we had more experiences on 3P-VATS than uniportal VATS. This less experience of uniportal VATS might lengthen the operative time in the early period.

Changes in the text: No change.

Comment 8: What was the chest drain strategy? Size of tube? Suction or no suction?

Reply 8: Thank you for your question. A 19 Fr chest drain was placed through the apex in an inverted U shape from the anterior to posterior direction at the end of surgery. In addition, -7cmH₂O suction was applied for managing drainage until the following day of the surgery. If we found postoperative air leak on the day, -7cmH₂O suction was changed to a water seal without suction.

Change in the text: These sentences were added on page 7 lines 149 to 150, and on page 7 line 152 to 153.

Comment 9: What was the outcome of patients with recurrence? How significant were the recurrences? Did any have redo surgery? If so, what approach was used, and what was found?

Reply 9: Thank you for your question. There were seven recurrence cases. And three of them were re-treated by 3P-VATS and all of them had new bullae around the staple line of last surgery. In one patient, a drain was inserted followed by pleurodesis, while only drainage was performed in two patients. One patient was treated conservatively.

Change in the text: The outcome of postoperative recurrence and these sentences were added in Table 1 and on page 9 lines 194 to on page 10 lines 197 respectively.

Comment 10: A key issue with this study is that U-VATS PSP surgery has been well reported before. This study does not find anything new. The findings about age and

number of bullae are actually irrelevant to the main clinical objective (comparing U-VATS and 3P-VATS). Therefore, this study really does not give readers any new information about U-VATS that hasn't already been reported before. Yes, the cohort here is larger. However, see comment 1 above.

Reply 10: Your comment is reasonable. As you insisted in the comment 1, the calculation power in this study was relatively low because the occurrence rate of relapse was low. As we mentioned in Reply 1, we added the low calculation power about the recurrence in the study limitation. We hope that the readers evaluate our manuscript considering the limitations.

Change in the text: No change.

Comment 11: The insights from Fig 3 is quite intriguing. It seems even more novel than a comparison of U-VATS and 3P-VATS. However, it is uncertain whether one would recommend that the authors write a separate paper on that because the level of evidence is relatively weak based on one ROC curve alone.

Reply 11: Thank you for your suggestion. We used ROC curve to set the cut-off value of age in terms of postoperative recurrence. Finally, multivariate analysis revealed ≤ 17 years old was an independent factor about the recurrence, which can prove the validity of the ROC curve between age and recurrence.

Change in the text: We added the parameter of age in Table 2.

Comment 12: Has permission been obtained for publishing Fig 4?

Reply 12: Thank you for your suggestion. We remove the figure 4.

Change in the text: The figure 4 is removed.

Reviewer B

Comment 1: The title, although is of novelty, won't affect our future surgical practice.

Reply 1: Thank you for your suggestion. The title was changed to "Uniportal approach can be alternative of three portal approach in video-assisted thoracic surgery for primary spontaneous pneumothorax.".

Change in the text: The title was changed.

Comment 2: You stated in your text that 3P-VATS is a standard surgical practice for PSP, which is not a global consensus.

Reply 2: I agree with your opinion. Therefore, we revised the sentence.

Change in the text: The sentence “in several countries, especially Japan” was added on page 11, lines 218 in the revised manuscript.

Comment 3: Fig. 4 should be cancelled.

Reply 3: Thank you for your suggestion. We remove the figure 4.

Change in the text: The figure 4 is removed.

Comment 4: The major defect of the entire article is the language, which must be revised by native professionals before your submission next time.

Reply 4: Understood. I send the manuscript revised by a native speaker.

Change in the text: The revised sentences were highlighted in blue color.

Reviewer C

You describe an, in my opinion, an interesting patient analysis of patients with spontaneous pneumothorax after uni- and three-portal surgery. Unfortunately, you don't have any information about postoperative pain level and satisfaction with treatment strategy. Maybe you can explain, why you didn't sample this data?

Reply 1: Your comment is reasonable.

Unfortunately, the evaluation of postoperative pain and patients' satisfaction were lacked in many cases because this was a retrospective study. Therefore, we did not show these data in this study. We would like to focus on “non-inferiority” of uniportal VATS for PSP compared to three-portal VATS, which was proved by the non-inferior perioperative results and recurrence rate although showing the evaluation of postoperative pain and patients' satisfaction was more desirable.

Change in the text: We added the sentences about the lack of these data in many cases page 13 lines 277 to 278.