

## Peer Review File

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### Reviewer A

The authors reported to reveal the effectiveness of rehabilitation as pain management for patients underwent VATS for pneumothorax. This study focused the comparative outcomes on pain scales according to rehabilitation protocol. Moreover, participants were confined to young adults requiring VATS for pneumothorax. Reviewing this article, I think that several concerns must be discussed, listed as follows:

1. In methods of the abstract, it will be more informative to describe the ranges and median value of included patients, just not by under 40 years. Additionally, specified protocol other than respiratory exercise can be explained, instead of repetitive phrase of “in patients ~ rehabilitation program”.
2. In results of abstract, detailed descriptions of comparison data are required for numerical values of pain scale and rescue medications with statistics instead of subjective outcomes.
3. In line 60, not all pneumothorax patients require urgent treatment in general.
4. In initiating rehabilitation protocol, did the protocol not contained benefits of physiotherapy for respiratory recovery? In usual rehabilitation program for pulmonary resection, airway clearance and lung expansion (as described in line 198) are crucial, not omitting respiratory muscle training, even for pneumothorax. Do the authors expect the role of pain reduction by rehabilitation protocol without aggravating pain after more exercise?
5. Rehabilitation after thoracic surgery is relatively more required in older patients to decrease morbidities. When if the patients over 40 including secondary pneumothorax are included in this study, any different outcomes can be happened? What about the author’s opinion?
6. In line 126-7, sentences on more pain with ref (14) seems not required in the surgical procedure section.
7. The authors reviewed that VATS demonstrated less painful with less-invasiveness, and 3 port VATS show more pain than single port. On the contrary, they commented that

chronic pain after VATS is relatively high with several references. Does pain outcomes can be positively expected on the long-term effects of suggested rehabilitation programs, regardless of port numbers?

8. In lines 83-91, some corrections might be required due to incomprehensible meanings with references.

9. In line 111~113, admission day seems not required to study design.

10. Descriptions on the author's hypothesis might be helpful on pain perception, pain, memory after prior procedures with references on pain control mechanism.

11. Medical records by different, many observers on the pain scale and medications comprises several biases with weakness in this retrospective study.

12. In the results section, descriptions of important data and statistical values are suggested, not only showing Table and figure legends.

13. The relationship between the smoking history, sex factor and pain are not well explained without showing significances. In line 213-218, complex descriptions about sex factors can be deleted for clear interpretation.

14. In line 180, conclusion section may be replaced with discussions.

15. More exercise may be achieved due to decreasing trends of pain scale, or more pain may be experienced by exercise contrary. What exact factors were affected on POD#2, #3, which showing no significances for pain relief between two groups, compared to the data on POD#1. It also may be helpful to discuss the correlation between chest tube removal and rehabilitation tolerance because the cause of pain is most affected during the chest tube indwelling time.

### **Response:**

Dear reviewer A

Thank you for revising our manuscript and for much informative advice.

1 I added the mean age and range. And added the special protocol of rehabilitation.

2 I added the detailed data of the numeral values of the pain scale and rescue medications.

3 I agree with your advice. I deleted the word "urgent".

4 Thank you for your opinion. It is a retrospective study, so we did not expect this result. But some patients may relieve their pain by soft aerobic exercise, and some patients may relieve their pain by talking with the various staff. This result means both causes may relieve pain, I presume.

5 Thank you for your opinion. Patients over 40 may have a tendency of less pain. Our

other research shows less pain represented in the over 40-year age patient and there is no significant difference between the two groups because of the number of cases.

6 Thank you for your opinion. We delete this paragraph and reference.

7 Two different reports have existed and show different results. But our report shows the soft aerobic exercise will reduce postoperative pain.

8 Thank you for your opinion. I revised the paragraph.

9 Thank you for your opinion. I delete the paragraph. the number of the patient who did not underwent rehabilitation was markedly less. we don't know because but one reason is the day of the hospitalization. Some outpatient doctors did not order rehabilitation on admission. I tried to explain why the two groups were not a similar number.

10 Thank you for your opinion.

11 Yes. Our study has limitations.

12 Thank you for your advice. I added the data.

13 I revise the paragraph.

14 I changed “conclusion” to “discussion”. Thank you.

15 Thank you for your opinion. The relationship between pain and drain should be discussed. The patient who underwent thoracotomy for lung cancer led to the reverse conclusion maybe the longer period of drainage. But this problem should be discussed in other studies.

## **Reviewer B**

Was this a retrospective study or prospective? My understanding from the manuscript is that patients were divided and data collected in a prospective matter. If the decision to put patients in either the rehab cohort or no-rehab cohort was made back in 2012 and the data was collected thereafter, wouldn't this be a prospective study?

If pain complaints were registered by nurses, and the patient's in the rehab session were gone for 60-80 minutes per day, and 4 days were measured, meaning 240-320 minutes in total were spent without nurses, then inherently shouldn't they have fewer pain complaints and medications used as they spend less time with nursing to register those pain complaints or be provided medication?

The rehabilitation program lacks specificity. As a reader, I am unable to determine what treatment was done or how to generalize these results. Moreover, it is unclear if every patient received the same therapeutic prescription given this lack of specificity.

Statistically, this data most likely will produce significantly overlapping confidence intervals, without further data however I am unable to say this with certainty. Confidence intervals, rather than p-values, would be much preferred for this type of study to assist us in determining the value of your outcomes.

Further, with regards to the statistical analysis, the patient's in the rehab program started with less pain and ended with less pain. The patients in the non-rehab program started with more pain and ended with more pain. However, their decline is fairly similar and likely represents a selection bias with regards to who was in which group. Moreover, none of the reductions at any day post-op amount to a minimal clinically important difference.

I see no mention of patients who were crossed over from the rehab program to the non-rehab program. I am curious that there were no patients who either had reduced programs due to pain tolerance or were unable to tolerate the program post-VATS. If none truly crossed over or were unable to tolerate the program, please include that finding.

**Response:**

Dear reviewer B

Thank you for revising our manuscript and for much with informative advice.

1 Was this a retrospective study or prospective? My understanding from the manuscript is that patients were divided and data collected in a prospective matter. If the decision to put patients in either the rehab cohort or no-rehab cohort was made back in 2012 and the data was collected thereafter, wouldn't this be a prospective study?

Thank you for your opinion. It was a retrospective study. because we did not decide the patient's rehabilitation randomly and double-blinded. We decided on the patient's rehabilitation based on the day of patient admission as a result, not on the protocol, So the time of the rehabilitation or some of the data was not decided.

2 If pain complaints were registered by nurses, and the patient's in the rehab session were

gone for 60-80 minutes per day, and 4 days were measured, meaning 240-320 minutes in total were spent without nurses, then inherently shouldn't they have fewer pain complaints and medications used as they spend less time with nursing to register those pain complaints or be provided medication?

Thank you for your opinion. Maybe you are right. We should have decided on the protocol about the complaints and administration of the painkiller during the rehabilitation. It is a retrospective study, and all we can do was check the result. But the rehabilitation room is situated beside our ward, so if the patient complained, PT could easily tell the nurse about their complaint and administer the painkiller soon and the nursing staff would record the event on the chart, I presume.

3 The rehabilitation program lacks specificity. As a reader, I am unable to determine what treatment was done or how to generalize these results. Moreover, it is unclear if every patient received the same therapeutic prescription given this lack of specificity.

Thank you for your opinion. It is a retrospective study, so we cannot know about the detail of the rehabilitation. But we ordered only aerobic exercise to avoid the collapse of the lung because of the hard-anaerobic exercise. And in our hospital, the post-operative rehabilitation was done by two PT staff.

Hence patients have received almost the same therapeutic prescription.

4 Statistically, this data most likely will produce significantly overlapping confidence intervals, without further data however I am unable to say this with certainty. Confidence intervals, rather than p-values, would be much preferred for this type of study to assist us in determining the value of your outcomes.

Further, with regards to the statistical analysis, the patient's in the rehab program started with less pain and ended with less pain. The patients in the non-rehab program started with more pain and ended with more pain. However, their decline is fairly similar and likely represents a selection bias with regards to who was in which group. Moreover, none of the reductions at any day post-op amount to a minimal clinically important difference.

Thank you for your opinion. It is a limitation of our study.

5 I see no mention of patients who were crossed over from the rehab program to the non-rehab program. I am curious that there were no patients who either had reduced programs due to pain tolerance or were unable to tolerate the program post-VATS. If none truly

crossed over or were unable to tolerate the program, please include that finding.

Thank you for your opinion. It is unclear that some patients reduced the program or not. According to the chart, no one was drop out or reduced the programs. I don't know why, but we did not order the rehabilitation programs so hard.

### **Reviewer C**

Congratulations to the authors on your brilliant clinical success after the implementation of rehabilitation for patients with spontaneous pneumothorax following VATS treatment. However, many information relating to the design of the rehabilitation as well as how it would make an impact on postoperative pain in this patient cohort is unclear. The following are my comments.

- (1) The definition of patient population was not clearly stratified. Pneumothorax? Spontaneous pneumothorax? Or primary spontaneous pneumothorax?
- (2) The surgical treatment for spontaneous pneumothorax was not mentioned properly, did you perform mechanical pleurodesis? How many patients receive chest tube insertion before their surgery? The above-mentioned factors did influence on the evaluation of pain scale.
- (3) How and when did you measure your pain scale for the patients? Measurement was performed when the patient was resting or mobilizing? Obtain the NRS scores three time a day and calculating the mean value? At the routine time point every day? Measured by nursing staff or clinical physician? All these factors would interfere the effect of pain scale and the further analysis.
- (4) According to your background knowledge searched from the literature, it seems that no other study has been conducted to explore the impact of rehabilitation on postoperative pain following VATS treatment, it would be too early to jump to this conclusion, as your case numbers was still too small. I would also recommend you to perform a propensity score matching to eliminate the bias.

### **Response:**

Dear reviewer C

Thank you for revising our manuscript and for much with informative advice.

I deeply appreciate your advice and revised the manuscript according to your advice.

- (1) I unified words to spontaneous pneumothorax.
- (2) All the patients underwent only VATS treatment without pleurodesis before surgery and 42 patients underwent preoperative drainage before surgery. We tried to investigate the effect of the postoperative pain for preoperative drainage, but it does not make a significant difference.
- (3) In our hospital, the nursing staff asked the NRS scale 4 times on the POD 0, 3 times on the POD1 to 4. We chose the highest numbers. I added the comments on *the assessment of postoperative pain* section.
- (4) Thank you for your comment. Unfortunately, we have changed the protocol about VATS operation. Now we choose single port VATS procedure for pneumothorax and mean hospitalization times shortened. So, we cannot add the number of the cases and propensity score matching.