Peer Review File
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Reviewer A

Comment 1:
The article deals with a very interesting topic. There are not many studies dealing with the problem of interventions in children, therefore this work arouses everyone's interest. The analysis was done correctly and the article is well structured.
Reply 1: Thank you very much for reviewing our manuscript and for providing your insightful comments on it.
Changes made to the manuscript: No changes were required with respect to this comment.

Reviewer B

Comment 1:
The authors present an interesting and novel method for selecting surgical approach in paediatric patients with mediastinal masses. While this technique may be interesting to readers, the small sample size and methodology may not warrant publication as a full research study, and may be better reframed as a brief report.
Reply 1: Thank you for your review of our manuscript and for your insightful suggestions on it. We agree that the small sample size is an important limitation of our study. As the reviewer has pointed out, the article may be presented as a "Brief report" instead of as an "Original article." However, many reviewers have reviewed our manuscript in its current form and have suggested revisions for it. Therefore, we will first try to revise our manuscript accordingly and resubmit it as an original article. If it cannot be accepted in this form, we will consider simplifying it and submitting it as a Brief report instead. To address your concerns at this stage, we have revised the Discussion section to emphasize the limitation of this study.
Changes in text 1: We have revised the limitations as follows: "*Our study had several limitations. First, this was a single-center retrospective study with a small sample size. Other factors, such as the location, histological characteristics, and invasion status of the tumor; availability of one-lung ventilation; and surgeon's experience may further affect the feasibility of RP-VATS. Although the baseline characteristics between the two groups were not significantly different, the patients' characteristics were varied; these differences may have affected the study findings due to its small sample size. Currently, our findings only suggest a low TTH ratio to be a predictor of the feasibility of RP-VATS; this must be confirmed in prospective multicenter studies with larger sample sizes. Therefore, the feasibility should be well determined preoperatively."* (see Page 13, Lines 238–247).

Comment 2:
The small sample size makes it impossible to draw conclusions about whether TTH is truly an effective predictor for successful RP-VATS.
Reply 2: Thank you for your comment. We agree with the reviewer that the small sample size limits drawing of concrete conclusions in this study. As the reviewer has suggested, the

feasibility of reduced-port video-assisted thoracic surgery (RP-VATS) may depend not only on the tumor-to-thoracic height ratio (TTH ratio), but also on other factors (such as the location and invasion of the tumor, patient's age and body size, availability of one-lung ventilation, and the surgeon's experience). Nevertheless, we believe that the TTH ratio can serve as a predictor of the feasibility of RP-VATS in children because it can represent the tumor's occupancy of the thoracic cavity, which is known to influence the surgical difficulty. Because no previous studies have suggested the usefulness of the TTH ratio in this aspect, we believe that advocating this concept will benefit future clinical practice. However, we do agree with the reviewer that definitive conclusions cannot be drawn in this study due to its small sample size. Therefore, we have accordingly revised the Discussion section to emphasize this as a study limitation.

Changes in text 2: We have revised the Discussion section to emphasize the study limitations as follows: *"Our study had several limitations. First, this was a single-center retrospective study with a small sample size. Other factors, such as the location, histological characteristics, and invasion status of the tumor; availability of one-lung ventilation; and surgeon's experience may further affect the feasibility of RP-VATS… Therefore, whether a low TTH ratio can be an effective predictor of the feasibility of RP-VATS should be investigated further. Currently, our findings only suggest a low TTH ratio to be a predictor of the feasibility of RP-VATS; this must be confirmed in prospective multicenter studies with larger sample sizes."* (Page 13, Lines 238–247).

Comments 3:

Further, it is not clear how patients were selected for the RP-VATS and traditional VATS approaches. If the surgeons were using TTH to select patients preoperatively then it is not surprising that TTH is statistically associated with successful RP-VATS. If all cases of mediastinal mass were initially begun as RP-VATS and then some were converted, it would make sense to use TTH as a predictive variable, but it seems as though some patients were selected for traditional VATS from the beginning.

Reply 3: Thank you for your comment. Since this was a retrospective study, the TTH ratio was not calculated preoperatively; instead, it was evaluated retrospectively. We generally considered RP-VATS as the first choice whenever possible; however, we opted for open thoracotomy or traditional VATS when the tumor seemed to infiltrate the surrounding structures or encased great vessels. Two patients in the conventional procedures group underwent traditional VATS from the beginning; one of these required conversion to thoracotomy. We agree with the reviewer that the initial selection of the procedure and whether we used the TTH ratio for the selection of the procedure should be clarified in the manuscript. We have revised the Methods and Results sections, accordingly. Furthermore, we recognize that our description of the patients who underwent a conversion was not accurate in the Results section. We described that "one of the seven patients in the RP-VATS group underwent conversion to thoracotomy due to the large size of the tumor, and one of the three patients who underwent the conventional VATS procedure underwent conversion to thoracotomy." However, the patient who underwent the conversion was not included in the RP-VATS group, but was actually a part of the conventional procedures group. Therefore, the more accurate statement would be as follows: "One patient was initially started on RP-VATS, but underwent conversion to thoracotomy due to the large size of the tumor; furthermore, one of the two patients who were

initially started on conventional VATS underwent conversion to thoracotomy." We apologize for this error. The results themselves do not require changes because the patients were accurately divided into the two groups. We have revised the Results and Discussion sections as well as Table 2 accordingly.

Changes in text 3: The Methods section has been revised as follows: *"We generally considered RP-VATS first whenever possible, regardless of the TTH ratio; however, we opted for conventional VATS or open thoracotomy when the tumor seemed to infiltrate the surrounding structures or encased great vessels. When RP-VATS was deemed difficult to perform, we converted to conventional multi-portal VATS or open thoracotomy."* (see Page 7, Lines 106–109).

Furthermore, we have revised the Results section 1) to clarify whether treatment was initiated with RP-VATS or conventional VATS and 2) to present the findings more accurately, as follows: *"One patient underwent conversion from RP-VATS to thoracotomy due to a large tumor size; furthermore, one of the two patients who were initially started on conventional VATS also underwent conversion to thoracotomy."* (see Page 9, Lines 158–160). We have also revised the Discussion section as follows: *"Only one patient who was initially started on RP-VATS required conversion to thoracotomy due to a large tumor; another patient who initially underwent conventional VATS also required conversion to open thoracotomy."* (see Page 11, Lines 208–210). In Table 2, the number of patients who underwent conversion to thoracotomy in the conventional procedures group was revised from 2 to 1. A patient who initially underwent RP-VATS required conversion to thoracotomy. Another patient who initially underwent conventional VATS also required conversion to thoracotomy; there were two patients who underwent conversion to thoracotomy in the conventional procedures group.

Comments 4:

Its unclear if any patients were converted from RP-VATS to traditional VATS.

Reply 4: Thank you for flagging this with us. We agree that clarifying the number of patients who were converted from RP-VATS to traditional VATS is very important. In this study, no patients who underwent RP-VATS from the beginning experienced a conversion to traditional VATS. We have revised the Results section to clarify this.

Changes in text 4: We have added the following sentence to the Results section: "*No patients underwent conversion from RP-VATS to conventional VATS."* (Page 9, Lines 156–157).

Comments 5:

Furthermore, there are other significant differences between the two arms, including gender differences and the inclusion of a large proportion of congenital cysts in the RP-VATS arm and none in the traditional VATS arm, and these cysts may have been easier to resect.

Reply 5: Thank you for your comment. Although the sex distribution did not differ significantly between the two arms in this study, we agree with the reviewer that the influence of differences in the sex distribution on the utility of the TTH ratio cannot be ruled out because the study's small sample size. Thus, this should be investigated further in more large-scale studies. Furthermore, we also agree with the reviewer that a large proportion of congenital cysts in the RP-VATS arm may have affected the study's findings related to the feasibility of RP-VATS. Therefore, the effect of this factor should also be investigated further. We have revised the

Discussion section to emphasize the limitations of the study accordingly.

Changes in text 5: We have added the following sentence to the Discussion section: *"Other factors, such as the location, histological characteristics, and invasion status of the tumor; availability of one-lung ventilation; and surgeon's experience may further affect the feasibility of RP-VATS."* (see Page 13, Lines 239–241).

Comments 6:

Its not obvious to me that the two cases using CO2 should have been excluded either.

Reply 6: Thank you for your comment. We understand that we should provide a detailed explanation on why patients who underwent VATS with CO_2 insufflation were excluded from the study. VATS with CO_2 insufflation requires airtight access ports. In our institute, the airtight access ports have a minimum size of 5 mm; because RP-VATS involves the use of 3 mm access ports, we deemed CO_2 insufflation to be incompatible with it. In the future, it may be beneficial if CO_2 insufflation could be adapted to RP-VATS.

Changes in text 6: We have made the following revisions to the Methods section to explain why patients who underwent CO_2 insufflation were excluded: *"Furthermore, two more patients who underwent VATS with carbon dioxide (CO_2) insufflation were also excluded. This is because the procedure requires airtight access ports, which were of a minimum size of 5 mm at our institute; thus, carbon dioxide (CO_2) insufflation was deemed incompatible with RP-VATS (which involves 3 mm access ports)*" (see Page 6, Lines 85–88). We have also added the following sentence to the Discussion section to mention the adaption of CO_2 insufflation to RP-VATS as a future research scope: *"Finally, we excluded patients who underwent CO_2 insufflation; it may be beneficial if CO_2 insufflation can be adapted to RP-VATS."* (see Page 14, Lines 255–256).

Comments 7:

Lastly, the inability to generate a cutoff threshold for TTH makes the results difficult for readers to apply to their own practice. How can we use this technique to select patients without this information?

Reply 7: Thank you for pointing this out. Unfortunately, we were unable to identify a cutoff TTH ratio due to the small sample size. In daily practice, the selection criteria of RP-VATS can be affected by not only the TTH ratio, but also by other factors (such as the tumor's location, histological characteristics, and invasion status; availability of one-lung ventilation; and experience of the surgeon). Nevertheless, our study suggests the benefit of the TTH ratio as a predictor of the feasibility of RP-VATS. We believe that the TTH ratio can be beneficial to surgeons because there are no criteria to help select the optimal surgical approach. However, we do agree with the reviewer that the lack of a cutoff TTH ratio makes it difficult for readers to apply the study results to their own practice. Therefore, we have revised the Discussion section to explain the study limitations accordingly.

Changes in text 7: We have revised the Discussion section as follows*: "Other factors, such as the location, histological characteristics, and invasion status of the tumor; availability of one-lung ventilation; and surgeon's experience may further affect the feasibility of RP-VATS…Therefore, whether a low TTH ratio can be an effective predictor of the feasibility of RP-VATS should be investigated further. Currently, our findings only suggest a low TTH ratio*

to be a predictor of the feasibility of RP-VATS; this must be confirmed in prospective multicenter studies with larger sample sizes. Therefore, the feasibility should be well determined preoperatively." (see Page 13, Lines 239–247).

Comments 8:
Clarification is also needed about the "opt out clause" used to obtain consent.
Reply 8: Thank you for your comment. We agree with the reviewer you that "opt out clause" may be confusing to some readers. This study was retrospective and observational in nature, with no interventions performed; thus, we were not necessarily required to obtain informed consent from the included patients. Information about the conduct of the study, including its purpose, was made readily available to the patients or their legal guardians on the institution's website. The opportunity to refuse participation (i.e., opt out) was guaranteed to the patients or their legal guardians. To clarify this, we have revised the Methods section accordingly.
Changes in text 8: We have revised the Methods section as follows: *"The requirement of obtaining individual informed consent was waived due to the study's retrospective nature; however, information about this study was made readily available to the legal guardians of the patients on the institutional website. Furthermore, the opportunity to opt out was guaranteed; patients who opted out were excluded from the study."* (see Page 6, Lines 79–82).

Comment 9:
I do think this technique is worthy of dissemination and may have the potential to influence practice and other studies but it does not have the statistical or methodological rigour to be presented as a research study. It would be more appropriate as a brief report.
Reply 9: Thank you again for this suggestion. As the reviewer has pointed out, this study may be published as a Brief report for the reasons that the reviewer has mentioned. However, as we have mentioned in our response to your first comment (i.e., Reply 1), many reviewers have reviewed our manuscript in its current form and provided suggestions on it. Therefore, we will first try to revise our manuscript accordingly and resubmit it as an original article. If it cannot be accepted, we will consider simplifying the article and submitting as a Brief report.
Changes in text 9: The changes relevant to this comment are listed in the responses above.

Reviewer C

Comment 1:
The paper by Shiiya et al is an observational retrospective analysis of a very peculiar series of patients undergoing surgery with curative intent for a very rare surgical indication. Despite the case load is limited this is not a major limitation considering the clinical context.
The aim of the study is interesting and original in the core goal, but the possible scientific perspective is low. The Authors retrospectively review 13 patients out of a generally expanded experience. The proposed index may have a role in the surgical planning, but to perform a UVATS and reduced the access of such kind of a sub-centimetric incision or by adding a needle-scopic incision for the camera could be out of a real surgical discussion. In regard of a straightforward procedure to perform a highly demanding technique could be useless for a correct and uneventful procedure.

Despite my reserve on the real groundbreaking scientific intuition, the paper is methodologically well designed and well written. In this regard I have several comments:

1. Measures are taken from differ imagine tools.

Reply 1: Thank you for reviewing our manuscript and providing your insightful comments on it. The operating time, blood loss, duration of drainage, length of hospital stay, and postoperative complications in the RP-VATS group in our study were acceptable and minimal. Thus, even though reduced-port video-assisted thoracic surgery (RP-VATS) may not be widely used yet, we believe that this procedure offers clinical benefits in appropriately selected patients. In our study, the thoracic height was calculated using chest radiography. However, the tumor height was calculated using chest computed tomography (coronal view), because it was difficult to measure using chest radiography due to ambiguous lesion margins in some cases. Nevertheless, in daily practice, both the thoracic height and the tumor height can be calculated using chest radiography. Although data are not shown because these results are outside the scope of the current discussion, the tumor-to-thoracic height (TTH) ratio calculated by chest radiography also differed significantly between the RP-VATS and conventional procedures groups (median [interquartile range]: 33.8 [25.6, 39.1] vs. 49.3 [45.4, 53.8], P = 0.003). Therefore, we believe that the use of different imaging tools had a minimal effect on the results of our study. However, we agree with the reviewer that the effect of using different imaging tools on the results may be of interest to the readers. Therefore, we have revised the Discussion section to discuss the potential effects of using different imaging tools.

Changes in text 1: We have added the following sentence to the Discussion section to discuss the potential effect of using different imaging tools: "*In this study, chest radiography and chest CT were used to calculate the thoracic height and tumor height accurately; however, calculating both with radiography alone may be an easier method for predicting the feasibility of RP-VATS. The optimal modality for these calculations should be investigated further.*" (see Pages 12, Lines 212–214).

Comment 2:

2. Patient allocation to different technique is not guided by the proposed index but according to surgeon preference.

Reply 2: Thank you for your comment. As the reviewer you have pointed out, patient allocation in this study was not guided by the tumor-to-thoracic height (TTH) ratio. Because this was a retrospective study, the TTH ratio was not used to allocate the patients; however, it was evaluated retrospectively instead. We generally considered RP-VATS first whenever possible, regardless of the TTH ratio; however, to guarantee safety, we opted for conventional VATS or open thoracotomy when the tumor seemed to infiltrate the surrounding structures or encased great vessels. We agree with the reviewer you that the criteria for patient allocation are very important. Therefore, we have revised the Methods section to emphasize the allocation criteria and have revised the Discussion section to emphasize the need for a prospective study to confirm our findings.

Changes in text 2: We have revised the Methods section as follows: *"We generally considered RP-VATS first whenever possible, regardless of the TTH ratio; however, we opted for conventional VATS or open thoracotomy when the tumor seemed to infiltrate the surrounding structures or encased great vessels."* (see Page 7, Lines 106–109). We have also revised the

Discussion section as follows: *"Additionally, patients were allocated to the groups at the surgeon's discretion."* (see Page 13, Line 243) and *"this must be confirmed in prospective multicenter studies with larger sample sizes."* (see Page 13, Lines 246–247).

Comment 3:
3. Patients characteristic are sparse and very different form one to another.
All this information needs to find mention in the discussion section in order to expand the future perspectives. I have been kept by originality and by the focused association between the proposed ratio and the described technical options. For these reasons I suggest major revisions, a different scientific perspective, but I am afraid that the audience could be limited to multiportal VATS performers.
Reply 3: Thank you for your comment. Although the baseline characteristics did not differ significantly between the two groups in our study, we agree with the reviewer that the patient characteristics were varied; these differences may affect the study results due to the small sample size. We further agree that this should be mentioned as a limitation in the Discussion section. As suggested, the majority of the audience may comprise multiportal VATS performers because the RP-VATS procedure is not yet a gold standard; however, we believe that our findings may help these multiportal VATS performers in their decision making.
Changes in text 3: We have added the following sentence to describe the potential differences in the baseline characteristics as follows: *"Although the baseline characteristics between the two groups were not significantly different, the patients' characteristics were varied; these differences may have affected the study findings due to its small sample size."* (see Page 13, Lines 241–243).

Reviewer D

The report presents the TTH as a factor to predict feasibility of RP-VATS for mediastinal lesions for children less than 10 years old. The message of this paper is that low TTH ratio can help to select patients for RP-VATS procedures. The manuscript is well written and interesting. The main weakness of this paper is the small number of cases.

Major remarks:
Comment 1:
Introduction section:
VATS is considered as a muscle sparing method in comparison with open thoracotomy. The advantage of RP-VATS in comparison with VATS is not considered whereas better outcomes are expected. Otherwise, what would be the interest of the present subject?
Reply 1: Thank you for reviewing our manuscript and for pointing this out. We agree with the reviewer that mentioning what advantages reduced-port video-assisted thoracic surgery (RP-VATS) offers over VATS is important. Although the advantages of RP-VATS in children should be investigated further, its theoretical advantages in adults include minimal surgical invasiveness. Due to the use of 3 mm ports, the patients experience minor surgical invasiveness because the wound does not require suturing. Further advantages include minimal scarring and minimized pain (Interact Cardiovasc Thorac Surg. 2013;17:268–72, Minim invasive Ther allied

Technol. 2012;21:168–72., Ann Thorac Surg. 2003;75:599–601.) We have mentioned these advantages in the Introduction section.

Changes in text 1: We have added the following sentence to present the advantages of RP-VATS in the Introduction section*: "The theorical advantages of RP-VATS are minimal surgical invasiveness (because the wound does not require suturing), minimal scarring, and minimized pain (9,11–13)."* (see Page 5, Lines 59–61).

Comment 2:

Methods and surgical procedures:

The authors state "an initial incision of 5-40 mm…". During the authors' experience, was it necessary to enlarge this incision in order to remove pathology specimen?

Reply 2: Thank you for raising this important point. In one patient who was initially started on uniportal VATS with a 40 mm incision in this study, we had to enlarge the incision to 45 mm for specimen extraction.

Changes in text 2: We have added the following sentence to the Results section: "*In one patient who was initially started on uniportal VATS with a 40 mm incision, the incision had to be enlarged to 45 mm for specimen extraction.* " (see Page 9, Lines 157–158).

Comment 3:

Results and groups:

The choice to add a trocar (RP-VATS vs. VATS) was made during the surgical procedure. However, it seems that group 1 and group 2 do not have the same post-operative observation period ((93 vs.1119 days). Has the practice of the authors evolved from VATS to RP-VATS with the surgical team experience? Could the authors comment about this point? Is there a relationship between RP-VATS and the surgical experience?

Reply 3: Thank you for your comments. Although the postoperative observation period seems to be different between the two groups, this is not due to the experience of the surgical team; instead, this may be attributed to differences in the need for long-term observation. In the RP-VATS group, there were three patients with bronchogenic cysts who did not require intensive follow-up. Furthermore, some patients in our institute travel from another city. Therefore, they sometimes request for postoperative follow-up to be conducted in hospitals in their city for residence. This may have further caused the differences in the follow-up periods between the two group. We agree with the reviewer that these points should be clarified.

Changes in text 3: We have added the following sentences to the Discussion section: "*Third, the postoperative follow-up period in this study varied according to the patients' diseases; furthermore, some patients travelled from another city and requested postoperative follow-up to be conducted at another hospital. This could have further led to the differences in the postoperative follow-up period in this study. Therefore, further studies are necessary to determine the recurrence rate over a long-term follow-up."* (see Page 13–14, Lines 251–255).

Comment 4:

Results and TTH:

It seems that TTH is a crucial point to predict RP-VATS feasibility. In the RP group (Table 1), children are largest, biggest but their thoracic height is smaller in comparison with conventional

group. Could the authors comment this point?

Reply 4: Thank you very much for pointing this out. The thoracic height was a crucial parameter in this study. We rechecked the Table and the study results carefully and found that the thoracic heights in Table 1 were incorrectly swapped between the two groups. The accurate median thoracic height was 179.0 [interquartile range = 152.0, 203.0] in the RP-VATS group and 151.0 [interquartile range = 110.5, 175.0] in the conventional procedures group. Therefore, the thoracic height was larger in the RP-VATS group than in the conventional procedures group. We apologize for this error and thank you again for your careful review of our manuscript. We have also checked and confirmed that the data of the other characteristics listed in Tables 2 and 3 are accurate.

Changes in text 4: We have corrected the thoracic height in Table 1 as follows: RP-VATS group, from 151.0 [110.5, 175.0] to 179.0 [152.0, 203.0]. Conventional procedures group, from 179.0 [152.0, 203.0] to 151.0 [110.5, 175.0].

Comment 5:

Results and RP-VATS:

Do the authors would advise RP-VATS if possible? What is the author's belief: are per and postoperative outcomes are better for group 1 in comparison with group 2 because the patients are different or because the surgical technique is different (number of ports)?

Reply 5: Thank you for your comments. We would advise that surgeons opt for RP-VATS whenever possible; however, we do not believe that it is necessary to insist on RP-VATS in difficult cases. Even if RP-VATS is performed, it would provide no benefits if it were to fail and the outcomes were to worsen. Therefore, we proposed the TTH ratio as a possible predictor of the feasibility of RP-VATS. In our study, the postoperative outcomes (operating time, blood loss, and duration of drainage) were better in the RP-VATS group than in the conventional procedures group. This may be attributed to both the background characteristics of the patients and the surgical technique used. RP-VATS can reduce the operation time because opening and closing of the wound requires lesser time in this procedure than in conventional VATS and open thoracotomy. Blood loss may further be lesser because the muscle incision made during RP-VATS is minimal. We hypothesized that surgical difficulty is lesser in patients with a low TTH ratio. If the surgical procedure is easier, the operating time and blood loss should be shorter and lesser, respectively. Our results suggest that RP-VATS was performed in appropriate patients, and thus, the postoperative outcomes were better.

Changes in text 5: We have added the following sentences to the Discussion section to mention the possible reasons for the better outcomes in the RP-VATS group: *"These findings may be attributed to both the surgical technique used and the patients' background characteristics. RP-VATS may reduce the operation time because the time required to open and close the wound is generally lesser than that required in open thoracotomy. Furthermore, the blood loss in RP-VATS may be lesser because the muscle incision made is minimal. Conversely, surgery is generally less difficult in patients with a low TTH ratio; therefore, the operating time may be shorter and blood loss may be lesser."* (see Page 12, Lines 220–225). In addition, we have added the following sentence as well: *"RP-VATS can be considered whenever possible; however, in difficult cases, conventional VATS or open thoracotomy should always be considered instead."* (see Page 12, Lines 231–232).

Comment 6:
Discussion and robot:
The authors could emphasize the end of the discussion by introducing robotic perspective like the following reference: Assessment of paediatric thoracic robotic surgery. Interact Cardiovasc Thorac Surg. 2015; 20(3):300-3. doi: 10.1093/icvts/ivu406.
Reply 6: Thank you for your suggestion and for introducing a great reference. We have revised as suggested by the reviewer.
Changes in text 6: We have added a paragraph on the robotic perspective as follows: *"Recently, robot-assisted thoracic surgery (RATS) has been performed for pediatric mediastinal lesions (23,24). However, a major limitation of RATS for infants and small children is the lack of dedicated instruments (23). Although only few studies on RATS for pediatric mediastinal lesions have been reported, RATS will likely become more common with advances in technology. Future studies are required to compare the outcomes of thoracotomy, conventional VATS, RP-VATS, and RATS."* (see Pages 13, Lines 233–237).

Comment 7:
Minor remark: Line 219: RP-VATS and not PR-VATS
Reply 7: Thank you for pointing this out. We appreciate your careful review of our manuscript and have corrected the typographical error.
Changes in text 7: "PR-VATS has been revised to "RP-VATS." (see Page 12, Line 230).

Reviewer E

Some points I suggest review:
Comment 1:
1- replace the height of the tumor by the largest diameter of the lesion.
Reply 1: Thank you for your suggestion. We selected tumor height because it can also be calculated using chest radiography in most cases. In this study, we used chest computed tomography (CT) to calculate the tumor height because the tumor margins are clearer on chest CT. However, we agree with the reviewer that the largest lesion diameter may be another alternative for calculating the tumor-to-thoracic height (TTH) ratio and have added a sentence to the Discussion section to address this.
Changes in text 1: We have added the following sentences to discuss another possible choice for calculating the TTH ratio: *"In addition, the maximum lesion diameter, instead of the lesion height, may be used for calculating the TTH ratio. In this study, we chose to use the lesion height because it is simpler to calculate and can be estimated using chest radiography."* (see Page 12, Lines 214–217).

Comment 2:
2- consider that larger lesions will be more complex and therefore, by themselves, they will already have a longer surgical time and the only option would be larger incisions as well.
Reply 2: Thank you for your comment. We agree that the resection of larger lesions may be more complex and increase the surgical time. In our study, RP-VATS was performed in patients

with a lower TTH ratio; therefore, their lesions were relatively small and the time required for the resection was shorter. This may explain why the surgical time was shorter in the RP-VATS group. One aspect of our study was to predict the feasibility of RP-VATS using the TTH ratio. Therefore, we believe that RP-VATS can be performed for mediastinal lesions with a low TTH ratio and achieve acceptable outcomes. In addition, we agree with the reviewer that larger lesions require larger incisions for extraction. In one patient in our study, we initially started with uniportal VATS with a 40 mm incision; however, the incision had to be enlarged to 45 mm for specimen extraction. In another patient, we had to convert from RP-VATS to thoracotomy due to a large tumor. The tumors in other patients could be extracted thorough the initial 5–40 mm incision. Because a low TTH ratio generally means that the lesion is relatively small, RP-VATS may be feasible for a lesion with a low TTH ratio. We agree with you that these discussions should be included in the manuscript; we have revised the text accordingly.

Changes in text 2: We have added the following sentence to the Discussion section to mention the effect of the lesion size on the outcomes: *"Conversely, surgery is generally less difficult in patients with a low TTH ratio; therefore, the operating time may be shorter and blood loss may be lesser."* (see Page 12, Lines 223–225). We have also added information on tumor extraction in the Results section as follows: *"In one patient who was initially started on uniportal VATS with a 40 mm incision, the incision had to be enlarged to 45 mm for specimen extraction. One patient underwent conversion from RP-VATS to thoracotomy due to a large tumor size; furthermore, one of the two patients who were initially started on conventional VATS also underwent conversion to thoracotomy."* (see Page 9, Lines 157–160).

Comment 3:

3- despite the access route and the size of the lesions, emphasize that there was no difference in the rate of complications.

Reply 3: Thank you for your suggestion. We have revised the text as suggested.

Changes in text 3: We have added the following sentence to the Discussion section to emphasize that the complication rate did not differ significantly between the two groups: *"The RP-VATS and conventional procedures groups differed in terms of the access port and TTH ratio but not in terms of the complication rate*." (see Page 12, Lines 227–228).