

Peer Review File

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Reviewer A

Comment 1: You propose a new index for better detection of the asymmetrical funnel chest. Important preliminary work by Marcelo Martinez Ferro on this topic is not mentioned.

Reply 1: As you pointed out, Marcelo Martinez-Ferro et al.'s work on sternal torsion is very intriguing and will deepen the Discussion of this study on asymmetric pectus excavatum. We have described a new paragraph in the key findings.

Changes in the text: We have added the text as advised (see Page 10, line 166-171).

Comment 2: You propose a sternocostal resection to correct an asymmetric funnel chest. This is significantly more invasive than the pure Nuss technique. The advantage of this invasive technique compared to the Nuss technique is not clear.

Reply 2: As you mentioned, SCE is a more invasive procedure. However, we believe that the morbidity rate is a crucial factor that should be considered when providing surgical invasion. We have never experienced residual pain or reoperation in patients who underwent SCE, and they were able to return to daily life quickly after discharge and engage in all activities, including contact sports, 3 months postoperatively.

Changes in the text: We have added the text (see Page 9, line 138-145, Page 11, line 177-180, and Page 12, line 197-199).

Comment 3: You perform a CT scan on all patients (mean age 15 years). This does not comply with the international guidelines for minimising radiation exposure in children and adolescents. An MRI would be more recommendable.

Reply 3: Although reducing radiation exposure is ideal in young patients, performing MRI in children is challenging. Therefore, we performed low-dose helical CT preoperatively and 1 month postoperatively.

Changes in the text: We have added the text (see Page 8, line 120-122).

Comment 4: Line 66 should read Haller indices instead of Heller indices.

Reply 4: As you have mentioned, it was a typographical error.

Changes in the text: We have modified the text (see Page 7, line 112).

Comment 5: In the key findings, you begin with references. However, the most important results of your study should be mentioned here and then compared with the literature.

Reply 5: As you have mentioned, the composition of the key findings was not appropriate. I have modified.

Changes in the text: We have modified and added the text (see Page9, line 150-165).

Comment 6: The discussion appears to be very brief. A discussion of your own results with those of the literature would be desirable. As mentioned above, the preliminary work by Prof Martinez Ferro on the problem of asymmetric funnel chest detection is missing here.

Reply 6: We have modified and added some content in the Discussion with reference to the comments. Marcelo Martinez-Ferro et al.'s works were helpful; we have added two more references.

Changes in the text: We have modified and added the text (see Page 10, line 166-171, Page 13, line 212-215).

Reviewer B

A retrospective study on the usefulness of sternocostal elevation for asymmetric pectus excavatum.

I have enjoyed reading this paper. I like this paper as it was written and I have no suggestion to address to the authors.

Reply: None

Reviewer C

Comment 1: If the aim is to describe the feasibility, many parameters are lacking like post-operative pain, follow-up duration, particularly the time where the Haller Index was measured.

Reply 1: Postoperative CT was performed 1 month postoperatively, and we measured the postoperative Haller Index. Regarding postoperative pain, NSAIDs were administered to all patients during their hospital stay, and epidural anesthesia was administered to adults. However, none of the patients required regular oral medication for >2 weeks after discharge. We have provided a response about follow-up duration in Reply 2.

Changes in the text: We have added the text (see Page 7, line106, Page 9, line 138-141, and Page 11, line 185-188).

Comment 2: The surgical technique seems to be similar to Ravitch technique without a metallic bar. Ravitch himself implemented his technique with a bar as he noted that cosmetic results were not as good as expected after a long-time follow-up.

Reply 2: The lack of long-term follow-up data is an important limitation of this study; one factor is the several patients lived a great distance away from our hospital. Although contact via e-mail is possible, we did not receive any communication of recurrence or complaints.

Changes in the text: We have added the text (see Page11, line 185-188).

Comment 3: Functional data before and after the surgery are lacking.

Reply 3: Because cardiac function was within normal limits preoperatively, routine echocardiography was not performed postoperatively in all patients. Pulmonary function tests were stopped in the hospital for a long time because of infection prevention measures during coronavirus disease 2019. Because there were studies on cardiac function and respiratory function in SCE, they were added to the Discussion.

Changes in the text: We have added our text (see Page 13-14, line 222-234).

Comment 4: Satisfaction score before and after the surgery are lacking.

Reply 4: We were unable to assess the satisfaction score pre and postoperatively, and this is an issue that should be considered. However, no patients were dissatisfied with their postoperative morphology, and this study only showed that fact.

Changes in the text: We have referred to the results and added the text in the Discussion (see Page 9, line141-142 and Page 11, line 187-188).

Comment 5: Demonstrative cross-sectional imaging before and after the surgery are lacking to convince the readers.

Reply 5: As you have mentioned, it would be more convincing for the readers to have a typical image pre and postoperatively.

Changes in the text: We have added Figure 5 (see Page 9, line 152-153, and Figure 5).

Reviewer D

Comment 1: I think the results of treatment should be described in more detail to introduce the effectiveness of a new and unfamiliar techniques. This is because it is possible to indirectly compare the results of existing treatments. These include patient outcomes (improvement of preoperative symptoms, postoperative pain, satisfaction, PFT, cardiac

function, etc.), perioperative outcomes (time required for surgery, blood loss, length of hospital stay, etc.), and radiologic outcomes (Haller index, correction index, etc.). For example, the median value of the postoperative Haller index presented in the paper is 3.75. As you know, values of 3.25 or higher are evaluated as "severe degree", and are considered one of the indications for surgery. If evaluating only with that value, it would be difficult to say that correction by the sternocostal elevation has been sufficient.

Reply 1: Regarding the patient outcomes pointed out, those that could be obtained in this study were added, and those that could not be obtained were cited in the past literature. Furthermore, we have added perioperative outcomes. Moreover, we consider the Haller index to be an indicator for evaluating preoperative severity, and no previous literature specified an appropriate postoperative value. Therefore, in this study, we judged it valuable that the postoperative value improved compared with the preoperative value. We recognized the correction index as a well-known index, however, it is evaluated on the left or right in cases of asymmetric pectus excavatum, whichever is higher; therefore, we did not use it because we thought it was not appropriate as an indicator of well-balanced correction.

Changes in the text: We have added the text (see Page 8-9, line 134-141, Page 9, line 153-158, Page 12, line 205-207, and Page 13-14, line 222-234).

Comment 2: Reconstructing between costal end and sternum has the advantage of preventing chest wall instability. However, there is concern that functional improvement may be hindered by decreasing the thoracic cavity volume compared to before surgery.

Reply 2: Although we were unable to investigate respiratory function in this study, improvement of pulmonary function after surgery can only be expected only in cases of severe reduction of lung function in patients with pectus excavatum. Normal chest morphology is more important for respiratory function than rib cage volume.

Changes in the text: We have added the text (see Page 13-14, line 225-229).

Comment 3: Because the cost cartilage is weak, it will not be easy to fix it by suturing. It is expected that there will be complications including dehiscence or cartilage fracture, but it was reported that there was no complication. Is there a special suture technique to prevent this?

Reply 3: There is no special suture technique; however, we used #1 or #3 braided polyester sutures with a round needle because it can withstand strong tension and is less likely to cause damage to the ribs and sternum, such as cartilage cutting.

Changes in the text: We have added our text (see Page6, line 93-95).

Comment 4: You presented two new indexes, and I want to know data to support their validity or effectiveness.

Reply 4: Unfortunately, no data can support our new index. Other studies have also devised new indexes; therefore, it is expected that various geometric indicators will be proposed for the three-dimensional evaluation of thoracic deformity in the future.

Changes in the text: We have added the text (see Page13, line 212-215).

Comment 5: I think it would be better to delete the Figure 1 "Cobb angle" because it is not important to the subject of the study.

Reply 5: As you mentioned, the explanation of the Cobb angle was not important for this study.

Changes in the text: We have removed Figure 1 "Cobb angle".

Comment 6: I think 'Heller' in line 66 is a typo.

Reply 6: As you have mentioned, it was a typographical error.

Changes in the text: We have modified the text (see Page 7, line 112).

Reviewer E

Comment 1: There is a basic problem in the work: sternocostal elevation is not clearly conceptualized; Is it a surgical technique? Or is it a maneuver like Crane? and what is it for, exactly? Without this issue being clarified, the reading of the manuscript is impaired.

Reply 1: As you have correctly mentioned, we had not described the definition of SCE. SCE is the name of our surgical technique.

Changes in the text: We have added the text (see Page4, line63-64).

Comment 2: Title

The term "usefulness" is in the Title, but it is necessary to clarify what this "usefulness" is for: to elevate the sternum? to resect the sternum?

It is also important to say in the title that the manuscript refers to open surgery and not MIRPE.

Reply 2: We meant to report the usefulness of the surgical technique named SCE. We have modified the Title.

Changes in the text: We have added the word "open surgical technique" in the Title (see Page 1, line1).

Comment 3: Abstract

It is noted that there were 51 patients, but that those with scoliosis were excluded, suggesting that fewer than 51 were evaluated...it deserves correction.

Reply 3: As you have mentioned, the notation was inappropriate.

Changes in the text: We have modified the text (see Page 2, line 26-29).

Comment 4: Highlight Box What is known

The authors write that there are no good indicators and suggest the new index.

But is this index a predictor to be used in the pre op or a tool to be used in the post op as a result evaluator?

Reply 4: We used this indicator as a postoperative result evaluator.

Changes in the text: We have modified the text (see Page 3, line 44).

Comment 5: Highlight Box What is the implication

It does not seem to me that the literature supports the claim that pectus repair remains controversial. The vast majority of publications place MIRPE as the surgery of choice for pectus.

Reply 5: As you have mentioned, MIPRE is the mainstream surgery of choice for pectus, so I have corrected the notation.

Changes in the text: We have modified the text (see Page 3, line 45).

Comment 6: Line 35 - if the author believes it to be controversial, it is appropriate to cite a recent reference confirming this opinion. This is important because most of the literature does not support this view.

Reply 6: As you have mentioned, MIPRE is the mainstream surgery of choice for pectus, so I have corrected the notation.

Changes in the text: We have modified the text (see Page 4, line 58-60).

Comment 7: Line 37 - summary description of the SCE, followed by the citation of three bibliographical references. I had to research the works cited to understand what the technique was. Mainly, why the name SCE. My suggestion is that the author includes a more detailed description of the technique in the Methods, so that any reader understands which surgical technique is being used.

At this point, I would like to make a personal digression to the author, without this having any impact on the review of the work. Although the name suggests "elevation", the SCE

technique does not appear to actually elevate the sternum. Just look at Fig. 4 in reference 4. It is a post-operative photograph of SCE where it can be seen that the upper sternum remains sunken. I do not consider this to be classified as a good result after MIRPE.

Reply 7: As you have mentioned, the detailed description of SCE has been shifted from Discussion to Methods. In this procedure, the sternum is pulled laterally by shortened and resutured costal cartilages, and the resultant force raises the sternum ventrally. Furthermore, the sternum pulls the ribs and simultaneously corrects their inclination and the protrusion of the costal arches. Furthermore, the postoperative photo you have mentioned was taken 13 years ago, and the procedure has evolved more now than it was then. I added the representative chest photo before and after SCE in this paper.

Changes in the text: We have modified the text and added Figure 4 (see Page 6, line 88-102, and Figure 4).

Comment 8: Line 40 - the author writes that SCE "determines" the extent of resection...Is this correct? Is "Determines" the term? Please explain this point better.

Reply 8: As you mentioned, the notation was inappropriate.

Changes in the text: We have modified the text (see Page 4-5, line 65-66).

Comment 9: Objective

Line 44 - I believe the Objective begins with "This study...". The previous sentence "We consider..." should be part of the Introduction, not the Objective.

Reply 9: As you mentioned, the notation was inappropriate.

Changes in the text: We have modified the text (see Page 5, line 71-74).

Comment 10: Materials and methods

Line 53 - it was not clear to me what the criteria for asymmetry was. Would there be a difference greater than 10 mm between the hemi thoraxes? This sentence should be improved.

Is this criterion established in the literature? Please include a reference or clarify that it is the author's own discretion.

Reply 10: We meant that the criteria for asymmetry is a difference >10 mm between the hemi-thoraxes, and this criterion is as per our own discretion.

Changes in the text: We have modified the text (see Page 5, line 81-83).

Comment 11: Line 53 - Were patients with scoliosis excluded before and 51 patients resulted? That was what happened? This sentence must be improved.

Reply 11: As you mentioned, the notation was inappropriate.

Changes in the text: We have modified the text (see Page 5-6, line 80-85).

Comment 12: Line 54 to 56 - the explanation of the importance of the Cobb angle is not part of the Methods. This should go to Introduction.

Reply 12: The detailed description of the Cobb angle and Figure 1 have been removed based on the comments of other reviewers; they indicated that these were not important for this study.

Changes in the text: We have removed Figure 1.

Comment 13: Line 60 - "several indices"? It seems to me that there are only two: Haller and asymmetric index. Sternal torsion is not an index, it is just a measurement. It is better to quote the names of the two indices in full.

Reply 13: As you have mentioned, the notation was inappropriate.

Changes in the text: We have modified the text (see Page 2, line 28 and Page 7, line 104-106).

Comment 14: Results

It makes no sense to repeat the data that are part of Table 1: either the Table is published or the data is described in the text.

Reply 14: As you have mentioned, it is not favorable to repeat the data; therefore, we modified the sentence simply.

Changes in the text: We have modified the sentence (see Page 8, line 130-133).

Comment 15: Discussion

Line 107 to 115 - describes surgical technique. This should go to Methods.

Reply 15: As you have mentioned, the sentences regarding surgical technique have been shifted to Methods.

Changes in the text: We have modified the text (see Page 6-7, line 88-102).

Comment 16: Line 124 - do the authors have any hypothesis as to why the technique is not used in other Services? If there is some short explanation, this could enrich the manuscript. At least why they prefer SCE to MIRPE as mentioned in line 128?

Reply 16: The reason why SCE has been rarely performed outside our institution is that fine-tuning of chest wall reconstruction in SCE has a strong art aspect and requires a high level of proficiency to achieve a certain level of patient satisfaction. Furthermore, we consider that the examination of the learning curve of SCE is an issue for future research.

However, the morbidity rate, including residual pain or reoperation, which prevents the patients' quality of life, is an important factor to consider in providing surgical invasion. That's why we prefer SCE over MIPRE.

Changes in the text: We have added the text (see Page11-12, line 177-180, 182-184, and 197-199).