

Peer Review File

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Reviewer A

Comment 1: On the population considered, there is no reference to the pathology treated with this type of intervention. It is well known that the indication for decortication can be the empyema as well as the pleural mesothelioma. It must be elucidated in the text and possibly, if these indications are both present, the two population must be separated.

Reply 1: The pathology treated in this cohort includes a broad spectrum of diseases and primarily we tried to focus on the surgical aspects of this procedure and its complications. That is why we used as main outcome the 30-days mortality. We think that presenting the pathology of this cohort is out of the scope of this manuscript.

Changes in the text: N/A

Comment 2: Why the only parameter measured is the 30-day mortality? It could be more interesting a score for postoperative complications as well (air leaks, infections...)

Reply 2: Thank you for the suggestion. We added to the analysis the postoperative surgical site infection and failure to wean from ventilator. Unfortunately, there are not available data on postoperative air leaks on NSQIP database. Please find attached the added variables in table 4 and the appropriate changes in the manuscript (Lines 192-197).

Changes in the text: Table 4 and Lines 191-194

Comment 3: I think that a higher 30-day mortality can be expected in a patient with advanced cancer, dependent on the ventilator, and >80 y/o...which is the key to having a new score?

Reply 3: Conventionally, “elderly” has been defined as a chronological age of 65 years old or older in international literature [1-3].

Changes in the text: N/A

Comment 4: I did not understand properly the sentence in row 147.

Reply 4: In row 147 it says: “702 (30.3%) patients underwent thoracoscopic total pulmonary decortication”. By this phrase we mean that 702 patient underwent total pulmonary decortication by using thoracoscopic technique, not open access.

Changes in the text: N/A

Reviewer B

Comment 1: The manuscript adeptly presents the development of a decortication prognostic score, yet it lacks information on its validation. It would be beneficial to validate this score using an independent cohort to ascertain its reliability and generalizability across different patient populations.

Reply 1: We would like to thank you for your constructive comment. We are planning to proceed with a future single institution study to validate this prognostic score.

Changes in the text: N/A

Comment 2: To further enhance the study's depth, the authors might consider executing subgroup analyses by stratifying patients based on age, sex, or other pertinent clinical factors. Such an approach would facilitate the identification of specific patient populations that stand to gain the most from employing the decortication prognostic score.

Reply 2: Thank you for the constructive comment. We proceeded with gender subgroup analysis and we found that the score is more powerful in predicting mortality in male population.

Changes in the text 2: See changes in Table 4 and Line 195-197

Comment 3: A more exhaustive review of the existing literature on the topic would be beneficial, as it would afford greater context and enrich the overall presentation of the study.

Reply 3: We performed a more exhaustive review of the existing literature, and we added new references. Please find the changes in revised manuscript.

Changes in the text 3: Lines 79-82 and 233-234

Comment 4: The authors are encouraged to delve into the potential clinical implications of the decortication prognostic score, elucidating how it could be seamlessly integrated into clinical practice to inform decision-making and risk stratification.

Reply 4: We updated the conclusion section with information on how to incorporate the DPS in clinical practice.

Changes in the text: Line 265-269

Comment 5: In the Abstract, it is advisable to relocate the statement of the study's objectives from the Methods section to the Introduction section, ensuring a more coherent structure.

Reply 5: We made the suggested changes accordingly.

Changes in the text: Please see the updated abstract.

Comment 6: To adhere to the Journal's guidelines, the authors could specify the reporting checklist followed at the conclusion of the Introduction.

Reply 6: Please see the revised manuscript.

Changes in the text: Line 89-90

Comment 7: It has been noted that lines 85-88 reiterate the message conveyed in lines 72-74; thus, it would be appropriate to omit the redundant information for the sake of clarity and conciseness.

Reply 7: The lines 85-86 are empty lines and 87-88 have the titles of the section “Materials and Methods”. I am not able to understand this comment.

Changes in the text: N/A

Reviewer C

Comment 1: I would like the authors to give a clarification in terms of decorticating disseminated cancers. Are these cancers primarily from pulmonary/pleura/ other organs? It is known in the literature that one would disseminate cancer during decortication, hence it will only be acceptable to do decortication only in mesothelioma as part of decortication pleurectomy and in cases of pleural sepsis without pulmonary as a primary. (check line 32 and 146 on review version)

Reply 1: Unfortunately, the NSQIP database does not provide this information.

Changes in the text: N/A

Comment 2: On line 97 it is indicated the mesothelioma was excluded, therefore it is important to know which disseminated cancers were decorticated as we know that decortication malignancy is associated with poor outcomes.

Reply 2: Unfortunately, the NSQIP database does not provide this information.

Changes in the text: N/A

Comment 3: Can the authors also clarify if the decortications undertaken were primary or secondary as we know that secondary decortication is associated with poor outcome.

It is also important to indicate which phases/stages of empyema thoracic were decorticated via thoracoscopy vs. open because outcomes will differ.

Reply 3: All the cases in NSQIP database are referred as the index operation.

Changes in the text: N/A

Reviewer D

Comment 1: The number of cases considered empowers your conclusions.

The limits you discussed are certainly true particularly referring to the lack of information about the extension of decortication but again I believe that the important number of pts analyzed makes this study absolutely remarkable.

Reply 1: We would like to thank reviewer D for the constructive criticism of our manuscript.

Changes in the text: N/A

Reviewer E

Comment 1: What were the indications for decortication? Was there a difference in outcome based on pre-operative diagnosis? Were any of these decortications done after another thoracic surgery? If so, how did you account for the effect of the index operation on mortality?

Reply 1: The pathology treated in this cohort includes a broad spectrum of diseases and primarily we tried to focus on the surgical aspects of this procedure and its complications. That is why we used as main outcome the 30-days mortality. We think that presenting the pathology of this cohort is out of the scope of this manuscript.

Changes in the text: N/A

Comment 2: Does the risk score work differently based on surgical approach? While there was no difference in mortality between open and minimally invasive approaches, that does not guarantee that your score performs the same for both populations. It would be nice to see how the risk score performed in both groups.

Reply 2:

Odds of having postoperative mortality after decortication

Score	uOR	(95% CI)	p-value	Total Number of each Group/ number of patients with postoperative mortality (%)
In Open Cases				
Score 0	Reference			120 / 1 (0.8)
Score 1	0.87	0.08-9.64	0.865	277 / 2 (0.7)
Score 2-3	9.13	1.22-68.1	0.031	351 / 25 (7.1)
Score ≥4	47.6	6.29-360.4	<0.001	84 / 24 (28.6)

In Thoracoscopic Cases

Score 0	Reference			237 / 1 (0.4)
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Score 1	3.17	0.39-25.9	0.282	529 / 7 (1.3)
Score 2-3	15.67	2.14-115.1	0.007	562 / 35 (6.2)
Score \geq 4	82.99	11.18-616.3	<0.001	123 / 32 (26.0)

Changes in the text: N/A

Comment 3: Why did you choose to make each variable within your risk score have the same weight when some of the variables have a significantly greater impact on mortality than others based on odds ratio? It seems you can improve the accuracy of the risk model by using the values provided in your logistic regression rather than giving equal weight to all variables.

Reply 3: We used the same weight for each variable for easier use of the score in clinical practice.

Changes in the text: N/A

Comment 4: None of the risk factors that are identified are modifiable. How do you propose we use this information? With no good non-operative alternative for patients who are high risk, what do you propose their management be?

Reply 4: Individualized assessment of patients by DPS components may assist in the decision to pursue surgical decortication or an alternative treatment option.

Changes in the text: Lines 266-269

1. Orimo, H., *[Reviewing the definition of elderly]*. Nihon Ronen Igakkai Zasshi, 2006. **43**(1): p. 27-34.
2. *Elderly population*, O.f. Economic and C.-o.a. Development, Editors.
3. Singh, S. and B. Bajorek, *Defining 'elderly' in clinical practice guidelines for pharmacotherapy*. Pharm Pract (Granada), 2014. **12**(4): p. 489.