PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Multi-centre Evaluation of Renal Impairment in Thoracic Surgery (MERITS): a retrospective cohort study
AUTHORS	Naruka, Vinci; McKie, Mikel; Ahmadi, Navid; Pama, Claudia; Coonar, Aman; Collaborators, MERITS

VERSION 1 – REVIEW

REVIEWER	Ricardo Navarro Sanatorio Allende , Thoracic Surgery
REVIEW RETURNED	01-Dec-2021

GENERAL COMMENTS	It is an interesting and well written paper.

REVIEWER	Nuria M Novoa University Hospital of Salamanca, Thoracic Surgery
REVIEW RETURNED	11-Dec-2021

GENERAL COMMENTS	Dear authors, I have enjoyed reading your manuscript because this is a topic not extensively reviewed and we have a lack of knowledge about it making the paper very interesting. However, you have been too ambitious because your data do not support an important part of your results. Comments for improving: Introduction: I suggest deleting sentence in lines 35-36 as it gives information related to the results or discussion section not to the introduction. Methods: - Why was age categorized in three levels? Why was it not considered as a continuous variable? - Why endoscopic procedures included in the population of study? According to the degree of aggressiveness and the inflammatory consequences those are completely different procedures compared to pulmonary or chest wall resections for instance. My suggestion is to delete this group of collected patients and redo the analysis. -Why type of procedure is not considered in the analysis? I think it is crucial to know the type of resection not only the approach. -What about the comorbidity of the patient? Is there any possibility of adding comorbidity to the analysis? I see this is not possible Results: - Table 1: I think it is more interesting adding the “file” percentage than the “column” percentage at it is now. In my opinion it gives more information because it gives you the number of patients than having that common characteristic suffer or not AKI. Although in the tables I suggest changing the percentage, I suggest keeping it -for complete information- as it is in the text.

	- As expected, the mortality ranking is not the same as the AKI ranking. I would modify the sentence in the results section (lines 38-39, page 8). Aki acts as one of multiple factors producing mortality. - I am surprised about the provided mortality data. It is really high and needs an explanation. Discussion and references: - I am surprised seing that authors dedicate most of the discussion to the relationship between AKI data and mortality when there is a lack of supporting data for it. As authors comments somewhere, data not collected have the clue for the large amount of variation that is not explained by the model. I see this as a major limitation which stops me to recommend the paper for publication. Actually, my recommendation is redo the manuscript just to present the observational data of AKI in the different units but I would delete the rest of the analysis because of lack of data to conclude anything. This means that i would simplify the current manuscritp to present only the results of the analysis of the first, main objective and do not attempt, remove the other objectives. - Remember that MITS approach normally means less surgical resection and open approaches are related to more difficult and larger resections or are related to technical complications therefore cases are not comparable as postoperative complications are not comparable either.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1 – no comments

Reviewer 2- see the following points:

- ***Introduction****: I suggest deleting sentence in lines 35-36 as it gives information related to the results or discussion section not to the introduction.*

  We have addressed this comment and deleted the sentence in lines 35-36

- ***Methods:***
  - *Why was age categorized in three levels? Why was it not considered as a continuous variable?*

    We have previously published a pilot study (Naruka et al., 2019) in which we also considered age as a categorical variable, which is an approach used in several published research studies. By allocating into age groups we were able to increase the numbers in each group which allowed some comparisons to be made. In future work we will definitely consider the use of a continuous variable as helpfully suggested by reviewer 2.

  - *Why endoscopic procedures included in the population of study? According to the degree of aggressiveness and the inflammatory consequences those are completely different procedures compared to pulmonary or chest wall resections for instance. My suggestion is to delete this group of collected patients and redo the analysis.*

    Thank you, but we prefer not to exclude this group. We used the SCTS thoracic surgery database which includes bronchoscopic procedures as a category. The intention was to include <u>all</u> procedures recorded in the database to accurately reflect the unit outcomes. If we excluded, then that would also be open to different criticisms. Our study is not designed to explore causation and we never make this claim. We do agree with the reviewer that generally there may be less inflammation with lesser procedures but also view that this is variable – for example an aggressive interventional airway case which may include stenting could be more "aggressive" and longer than a simple VATS chest wall resection.

  - *Why type of procedure is not considered in the analysis? I think it is crucial to know the type of resection not only the approach.*

    Our study is limited to the procedure codes recorded in the SCTS thoracic surgery database which does not include more granular information. We agree it could be important and it is a point that we could explore in future work.

  - *What about the comorbidity of the patient? Is there any possibility of adding comorbidity to the analysis? I see this is not possible*

    This data is not collected in the SCTS database, but it is a topic that we could explore in future work.

- ***Results:***
  - *Table 1: I think it is more interesting adding the "file" percentage than the "column" percentage at it is now. In my opinion it gives more information because it gives you the number of patients than having that common characteristic suffer or not AKI. Although in the tables I suggest changing the percentage, I suggest keeping it -for complete information- as it is in the text.*

    We have adjusted this table as requested.

  - *As expected, the mortality ranking is not the same as the AKI ranking. I would modify the sentence in the results section (lines 38-39, page 8). Aki acts as one of multiple factors producing mortality.*

    We have changed this section which we hope better explains the multi-factorial nature of the outcome

  - *I am surprised about the provided mortality data. It is really high and needs an explanation.*

    The data collected do not allow us to answer this question at this stage. Mortality at 1 year will be variable due to multiple factors. We suspect that this is mortality due to underlying comorbidity and units undertaking a greater or lesser number of diagnostic or palliative procedures. It is something that should be examined in the future.

- ***Discussion and references:***
  - *I am surprised seeing that authors dedicate most of the discussion to the relationship between AKI data and mortality when there is a lack of supporting data for it. As authors comments somewhere, data not collected have the clue for the large amount of variation that is not explained by the model. I see this as a major limitation which stops me to recommend the paper for publication. Actually, my recommendation is redo the manuscript just to present the observational data of AKI in the different units but I would delete the rest of the analysis because of lack of data to conclude anything. This means that I would simplify the current manuscript to present only the*

results of the analysis of the first, main objective and do not attempt, remove the other objectives.

The purpose of this paper it to explore the unit variation in AKI and to see at a high level how it differs from mortality, which is the outcome measure currently recorded in the SCTS database. Other outcome measures such as length of stay, readmission etc are not outcome or quality measures in the same sense since a longer stay or higher readmission rate may reflect "good care" as opposed to suboptimal care. On the other hand, AKI for the reasons explained in the paper has physiological sense to consider as an outcome measure and also potentially correlates with later morbidity and mortality. This is why there is some discussion about this.

WE thank the reviewer for suggesting that we change focus but this is not the intention of this paper, and we would request the reviewer focusses on the stated intention of the study rather that to extend its goals at this stage.

- *Remember that MITS approach normally means less surgical resection and open approaches are related to more difficult and larger resections or are related to technical complications therefore cases are not comparable as postoperative complications are not comparable either.*

  We agree with the reviewer but again point out that this is not the intention of this paper.