Author’s response to reviews

Title: Double lung, unlike single lung transplantation might provide a protective effect on mortality and bronchiolitis obliterans syndrome

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Author’s response to reviews:

Dear Editor and Reviewers,

Thank you for your showed interest in reviewing our paper "Double lung, unlike single lung transplantation might provide a protective effect on mortality and bronchiolitis obliterans syndrome" and for investing your time in partaking in the manuscript.

Reviewer #1: Thank you for finding our manuscript acceptable for publication in The Journal of Cardiothoracic Surgery. We hope you found the paper fulfilling and that it will contribute to the field of lung transplantation.
Reviewer #2: Thank you for your well-thought of and constructive comments regarding how we may improve the manuscript so that it might be eligible for publication. In the text below we have tried to follow reviewer 2 recommendations and made the following changes to the manuscript:

Major:

1) We agree with reviewer 2 about adding a KM-figure regarding the survival estimate for once BOS grade > 2 has been diagnosed until follow-up/death for SLTx vs. DLTx. Such analysis has been made and added as requested. In the analysis we do see a pattern, however not significant but we believe that this could be due to insufficient number of patients. We do however still believe that the analysis improves the manuscript and we have added the analysis to the paper (Figure 5).

A table (table 2) regarding post-operative cause of death (organ rejection, infection, malignancy and "other causes") before/after 12 months for each tx-type has also been analysed and added as recommended.

2) We agree with reviewer 2 that there are several confounding factors that might play a role. Lung transplant patients are a very complex group of patients to study. Retrospective studies are also limited by the sometimes lack of clinical data needed to add several confounding factors, this is however a limitation. We have added comments to the section limitations with the possible confounding factors that reviewer 2 recommends.

Minor:

1) We agree that the present study population in Sweden differ in comparison with the ones reported to the ISHLT especially the US. We have therefore included this discussed issue under the section "discussion".
2) We agree that "HLTx" added significant text without adding results under the result-section. It has therefore been removed as recommended.

3) We agree that we may have been vague regarding the presentation of our center. Our university hospital consists of several sections spread out over the city of Malmo and Lund in the county of Skåne and is therefore called "Skåne University hospital" but belongs to Lund University. This has been clarified in the introduction as recommended by referring to our center as "Skåne University Hospital, Lund University, Sweden".

4) We acknowledge your comment on our limitation. We do however feel due to the retrospective nature of our study that it might not be suitable to adopt the current CLAD criteria and differentiate into BOS and RAS. Nonetheless this point of view has now been added under limitations so that the reader may be more aware of this dilemma.

5) We agree that more discussion on why our findings are important, clinical implications and teasing out plausible explanations would add to the discussion regarding the different disease states. We have therefore added several paragraphs with additional references when discussing CF, PF and PH regarding the clinical implications and plausible explanations of the importance of our findings for each disease state.

Concluding this response, we hope you found our revision satisfactory and that we addressed the recommendations on how to improve the manuscript.

Sincerely,