Reviewer's report

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

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Reviewer: Tiago Machuca

Reviewer's report:

The paper entitled "Double lung, unlike single lung transplantation might provide a protective effect on mortality and bronchiolitis obliterans syndrome" was reviewed.

The authors present their 24-y experience with 278 lung transplants in Sweden. The manuscript is centered on the finding of double lung transplants yielding significantly higher survivals despite similar BOS rates compared to single lung transplants.

This reviewer has two major concerns and several minor suggestions.

Major:

1) Since the hypothesis is that a double lung transplant can be protective for death once the patient develops BOS, I propose the authors to plot a KM survival estimate curve for single vs double lung transplant once BOS > 2 is diagnosed. This would provide further evidence, otherwise the "protective" effect we observe here could be merely a temporal coincidence. Even though the main cause of mortality after a lung transplant is related to BOS, there is still a considerable number of deaths due to infection, cardiac issues and malignancy. A table including the causes of death in each group would be helpful;

2) Even though the number of patients may be a limitation, there are several confounding factors that could be playing a role here. Variables such a recipient age, donor age, recipient kidney function, D/R pTLC, recipient O2 requirement, total ischemic time, have all been linked to long-term survival after lung transplantation. How would be authors propose to address this?

Minor:

1) The authors should be commended for their survivals but should also acknowledge that their patient population is different than the one reported for the US (which is older and a
significantly higher incidence of ILD vs COPD/CF) and also different than what is reported by the ISHLT (similarly more ILD 30% vs 13.6% Sweden);

2) I would consider removing the HLTxs from this analysis - it adds significant text without adding results;

3) On Introduction P2 - the authors mention there are only 2 centers performing lung tx in Sweden, with one being Skane University Hospital. In the next sentence they mention they will be reporting the outcomes of Lund University. This is confusing. Are these the same centers? Does this report covers all transplants performed in Sweden?;

4) I understand the limitation but strongly suggest the authors to adopt the current CLAD criteria and differentiate into BOS and RAS. I don't think nowadays we can accept the definition of FEV1 < 80% from best baseline for BOS. Phenotyping into BOS vs RAS would add significantly to the paper.

5) The mere description of their findings with regards to specific disease groups is repetitive and adds little to the paper. I would suggest adding more data (major point 2) and removing that if needed. If not, at least some discussion on why these findings are important, clinical implications for different disease states and may be teasing out plausible explanations would add to the Discussion.

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