Reviewer’s report

Title: Extended use of Extra Corporeal Membrane Oxygenation as Bridge to lung transplantation in two patients

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Reviewer: Patrick Mc Connell

Reviewer's report:

Skansebo et al. provide a very well written account of 2 patients on long term ECMO as bridge to LTx. The authors seem to want to lead the reader into the idea that we can understand the limits of ECMO. Unfortunately, the 2 patients presented, though both on longterm ECMO, are very different in almost all aspects. This makes it very hard to draw conclusions towards the authors idea of the limits reached.

They also "spring" the idea of cost into the discussion without a single economic data point in the case discussions. Furthermore, the costs for the survivor: ECMO, med flight to Sweden, days on ECMO, LTx, ECMO 2, kidney transplant far exceed the cost of the patient that died. The authors do not describe either patients pre-operative ventilatory status or whether they had tracheostomies, were they intubated etc.Nor is right heart function or the need of atrial septostomy discussed and is a concern with longterm v-v ECMO.Was V-V ECMO done with single double lumen cannula or other strategy?

The authors have provided 2 references and neither help the reader adjudicate risk in these high risk patients - ECMO is not the issue, the patients comorbid diseases are, and there is literature to guide teams through this 'fog'.

Thus, I do not have a clear sense of the take home points with the 2nd patient regarding single lung transplant after prior contralateral pneumonectomy. The authors failed strategy of median sternotomy for recipient explant does not translate to centers that would have employed peripheral bypass and approached a complicated explant via thoracotomy.The idea that median sternotomy offered options was likely misguided; as exposure and extra pleural dissection is impossible, and this is often required for such cases.

The authors don't acknowledge or consider that their surgical approach was potentially flawed. Another issue that they don't directly address in assessing risks of the patients based on the presumed functional respiratory capacity pre/post transplant. If their deceased patient was truly non-ventilatory for their extended time on ECMO and with a contralateral pneumonectomy - post transplant weaning from ventilatory support would have been prolonged, if even possible, with pneumonia a likely outcome.
These patients tend to perform like a patient with a paralyzed diaphragm, have post pneumonectomy like syndrome and without formal chest wall and diaphragm decortication, thoracic compliance is potentially a limiting issue to wean from positive pressure ventilation. The authors need to focus on the 800lbs gorilla in the room - post pneumonectomy and non ventilated patient - best case scenario is 30% mortality (F. Le Pimpec-Barthes 2014).

Obviously, it is difficult to adjudicate these factors, but readers need to appreciate why patient 1 did well is because her greatest risk was dialysis before LTx (different than new dialysis after transplant) and the second patient was a futile adventure due to surgical misadventure that was likely due, in part, to a poor surgical strategy given the history.

But, that even if transplant was successful, the patients thoracic comorbidities would have likely been unsurmountable given the experience of others - need references of prior pneumonectomy and LTx.

I certainly commend the authors for their valient and superb care of these complicated patients, but I do not believe readers are better informed regarding the limits of ECMO as bridge to LTx based on the information presented and the conclusions drawn by the authors.

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