Reviewer’s report

Title: Is tracheostomy a better choice than translaryngeal intubation for critically ill patients requiring mechanical ventilation for more than 14 days? A comparison of short-term outcomes

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Reviewer: Craig Jabaley

Reviewer’s report:

1) While the authors are to be commended on the overall quality of the language in the manuscript, it would benefit further from proofreading by a native English speaker to enhance readability.

2) To the authors' point in the introduction regarding previous literature comparing outcomes between prolonged translaryngeal intubation vs. tracheostomy - I would clarify that this study is unique in that patients did not undergo tracheostomy until an average of 18 days after intubation. As such, the authors may want to specify either in the abstract or discussion that the study essentially compares the outcomes of patients who *first* underwent prolonged mechanical ventilation and *then* either subsequent delayed tracheostomy or continued translaryngeal intubation. This distinction is outlined more clearly in the discussion.

3) What was the primary hypothesis? Although inferred, it should be more clearly stated.

4) In the methods regarding the waiver of consent, I would clarify that a waiver of consent was obtained as the retrospective study design posed minimal risks to patients.

5) Was noninvasive positive pressure ventilation provided consistently after extubation, or only on an as-needed basis?

6) The authors mention in the conclusions that the prolonged duration of mechanical ventilation in this patient cohort was due in part to previous failed extubations. Is data about the incidence of this complication available? If so, it may be an important latent variable and source of hidden bias. If not, discussion of this limitation should be included in the discussion.

7) With regard to limitations of the study, I worry that there is a high likelihood of susceptibility bias. Patients that were translaryngeally intubated had a worse prognosis (more ill, DNR, neurologic disorders) and thus might have been less likely to be offered a tracheostomy. As such, many findings related to the route of intubation may be due to indication bias and not true treatment effect. Although efforts have been made to correct for this through sophisticated statistical modeling, limited overlap between groups may limit the utility of propensity score matching in this context. This is mentioned by the
authors as a limitation, but I would strongly recommend an expanded discussion as this is a major limitation.

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

Yes

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

Yes

**Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?**
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I recommend additional statistical review

**Quality of written English**
Please indicate the quality of language in the manuscript:

Needs some language corrections before being published

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