Reviewer's report

Title: The Effect of Sedation and/or Analgesia as Rescue Treatment during Noninvasive Positive Pressure Ventilation in the Patients with Interface Intolerance after Extubation

Version: 2 Date: 16 May 2017

Reviewer: Patrick Murphy

Reviewer's report:

The study is an observational cohort study examining the effect of sedation on outcomes of patients with interface intolerance who have been extubated onto NIV. The manuscript has already been reviewed and revised appropriately.

I have some minor additional comments

A main limitation of the manuscript, acknowledge by the authors is the study design. Could the authors provide the rationale for the use of sedation in some patients, was there a protocol for indications for sedation or was this clinician preference? This is important as it may provide some insight as to how the populations were separated and thus differences in other aspects that need to be considered when interpreting the results. Further information on sedation administration policy should be provided in the manuscript to add information for the reader.

The interfaces are described as oral-nasal or facial; it remains unclear exactly which masks are being discussed. The terms oro-nasal and full face (both covering nose and mouth but not extending to cover the whole face) mask are often used interchangeably but are distinct from total face (covering whole face including eyes) masks. Although this has been revised it remains less than clear and so could the authors add further clarity?

The mortality analysis was adjusted for a range of cofounding variables although age is not listed despite being different between groups. Was age not related to outcome or was it used in the logistic regression but not listed?

The term OI is used in discussion but is not defined in the manuscript (p13 L12).

Mean PaCO2 data in discussion should be moved to results section (p13 L18-20).

The authors refer to a RASS target of +2 to -2 in the discussion (p14 L8-10) however in the response to reviewers comments state that no target was used during the sedation protocol. The authors should clarify this apparent discrepancy.

The discussion references improved outcomes in patients with hypercapnia rather than hypoxia, however the data provided in this manuscript does not directly support this (p15 L1-2). The higher CO2 in the surviving population does not support the assertion that sedation improves
outcomes preferentially in hypercapnic patients as this could relate to differences in diagnosis eg hypercapnic patients may be more likely to have COPD or other factors and a comparison of outcomes based on PaCO2 would be needed to further support this.

Table 1: PaCO2 - could the authors check the units (cmH2O)?

Table 1: Consider moving outcomes such as ICU LoS, mortality NIV failure as they are not baseline characteristics to another table or into results text.

Figure 3: Consider changing y-axis name to survival.

The manuscript would benefit from final revision from a native speaker.

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.

No

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 am able to assess the statistics

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

Needs some language corrections before being published
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