Reviewer's report:

Title: The effect of tracheotomy on drug consumption in patients with acute aneurysmal subarachnoid hemorrhage.

Version: 2 Date: 14 October 2014

Reviewer: Craig Jabaley

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The authors have conducted an observational study of patients with subarachnoid hemorrhage who underwent percutaneous tracheostomy with regard to their pre- and post-tracheostomy sedation requirements. The authors observed a statistically significant reduction in total daily sedative and analgesic requirements following tracheostomy. They have further asserted that this difference is clinically significant as patients subsequently required less vasoactive medication and transitioned more readily to spontaneous modes of ventilation.

Major Compulsory Revisions:

1) The authors’ discussion serves as a good review of relevant topics but should be altered as outlined below:

1.1) Prior publications have investigated the impact of tracheostomy on sedative requirements. (For example, see Nieszkowska 2005 and Veelo 2006.) Including discussion of prior publications would help to frame the findings presented in the submitted manuscript. Furthermore, how would the authors suggest that the reader reconcile relevant conflicting prior investigations with their results?

1.2) Similarly, I encourage further discussion of the observed patterns in modes of ventilation following tracheostomy framed against previously-published findings (as mentioned briefly). Also, could reduced sedation requirements be secondary to improved ventilator synchrony?

1.3) The authors correctly acknowledge that their observational data cannot provide definitive conclusions regarding long-term patient outcome. However, other definitive conclusions are offered elsewhere (page 7, line 193 and page 9, line 250). I would encourage a similar level of restraint in those elements of the discussion as well, especially given point #3 below.

1.4) The authors present only a brief discussion in their “limitations of the study” section, especially given the observational nature of the data presented.

2) The submitted manuscript requires further grammatical editing and careful proofreading prior to publication to enhance readability and clarity. In lieu of a listing here, please see the attached PDF with respectfully suggested revisions, which could serve as a starting point.

3) Interpretation of sedation and vasopressor requirements is severely limited by the absence of reported clinical assessments of patient sedation and agitation.
While unlikely, could patients have been more agitated and thus required fewer vasopressors? Did reduced pharmacologic sedation achieve similar levels of efficacy post-procedure? These data, if available, would provide valuable insight into the presented findings and enhance the manuscript greatly. While I do not feel that the absence of these data should preclude publication, this represents a major shortcoming that should be formally acknowledged in the manuscript.

Discretionary Revisions:
4) The authors’ assignment of numerical descriptors to modes of mechanical ventilation is understood from a statistical standpoint but somewhat cumbersome in the body of their manuscript and on the x-axis of Figure 4.
5) Data regarding CPP trends extending across a wider interval of time would be valuable if available.

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests.