Reviewer’s report

Title: Comparison of surgical field visibility during propofol or desflurane anesthesia for middle ear microsurgery

Version: 0 Date: 13 Feb 2019

Reviewer: David Polaner

Reviewer's report:

This is a study comparing the operating conditions (specifically surgical visibility as subjectively rated by the surgeon using a previously described and accepted scale) between two anesthetic techniques. The investigators controlled for heart rate as an effect modifier by choosing anesthetic techniques for each study group that included remifentanil, suggesting that the difference between groups was more likely due to the vasodilatory effects of the volatile anesthetic on middle ear mucosal blood flow, although they did not measure this metric directly. The primary outcome and secondary outcome variables are defined a priori and are well described. The limitations are mostly well explicated, and the conclusions (with one easily modifiable exception as noted below) are justified by the data. The text is clear, concise and well written and does not need any substantial editing for language or clarity.

Although a statistical difference between groups was measured, the clinical difference between them was minimal. The authors note this in the discussion p9, l47-57), however this important result is notably absent from the final conclusion. I think that it should be added - such a qualifier (that when remifentanil is used in combination with desflurane similarly acceptable operative conditions can be achieved) is pertinent to the final conclusion.

Other major comments:

p10, l18: I do not believe this is correct. It is more likely that the reduced remifentanil requirement in the desflurane group was due to the far more potent analgesic contribution of desflurane compared with propofol, which has primarily sedative hypnotic (and minimal analgesic) effects.

Middle ear microsurgery for cholesteatoma is a common operation in children as well as in adults. I suggest adding a sentence in the limitations section noting that, and due to the study population these results are limited to adults and might require further confirmation in children. (I am revealing my bias as a pediatric anaesthetist here!).

In figures 2 and 3A and B I suggest eliminating the connecting lines between data points. Your data here are actually discreet and categorical, not continuous, and the lines connecting the points
suggest that you are displaying the intermediate values measured between those points. Certainly there were intermediate clinical measurements during those periods, but those data points were neither collected nor analysed in the study, so it is best not to imply that there was a continuous transition between the time points unless they were in fact analysed.

Minor comments:

pdf p5, l28: To more completely define the patient population I suggest modifying this to read "Adult patients".

p6, l25: Please clarify if the hydromorphone was consistently given at the end of the anesthetic for postoperative analgesia only, or if it contributed to the intraoperative opioid load in some patients (the former is implied, but not explicitly stated)

p6, l40: It would be helpful to the reader unfamiliar with the Boezaart scale to add a phrase at the end of this sentence "where 0 denotes the best and 5 the worst visibility", especially since the meaning of the score's magnitude is not immediately intuitive (one would expect a higher score to imply better visibility). I would not change the sentence on p7 l10, however, as this reminder further enhances the clarity.

p8, l47-49: I know that you have defined the abbreviations "PR" and "DR" at their first mention in the beginning of the manuscript, however it is my personal preference (and I leave it to the editor to agree or disagree with me here) that in the first sentence of the discussion, where the primary outcome result is stated, to spell these out.

p8, l54-57: the opening phrase of this sentence is redundant; you can simply begin the sentence "This confirms previous findings that…".

p9, l8-10: One might consider adding a phrase to the end of this sentence noting that these findings suggest that when well controlled, heart rate itself is not the determinant of operating conditions and that other mechanisms (as you discuss below) are in play.

p9, l30: I would delete the word "combination"

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 am able to assess the statistics

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Please indicate the quality of language in the manuscript:

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