Reviewer’s report

Title: Differential Lung Ventilation for Increased Oxygenation during One Lung Ventilation for Video Assisted Lung Surgery

Version: 0 Date: 26 Feb 2019

Reviewer: Peter Alston

Reviewer’s report:

This paper reports a study that compares two methods of preventing hypoxia during lung isolation for VATS surgery and one-lung ventilation that is applying to continuous positive airway pressure (CPAP) or mechanical ventilation to the surgical lung. The paper has a number of important limitations.

1. The use of improved in the title of 'improves' is pejorative and insufficiently scientifically objective. Something like 'increases' would be more appropriate.

2. An estimate of the study population sample size should have been provided. It is inadequate to justify the population sample size used in the present study simply by reporting a previous study in the area. This is particularly important for the reader to be able to interpret non-significant results such as the surgical exposure when p = 0.073. This marginally non-significant result may caused by Type II error if too few patients were included in the study.

3. A cross-over study design was used and should have been explicitly stated in the methods section. A cross has strengths and weaknesses. An important weakness is carry-over effects and its influence on interpretation of the findings should have been addressed in the discussion section.

4. Multiple statistical tests have been applied to the data greatly inflating the risk of Type I error. A primary outcome was declared in the methods section but the risks of multiple testing in the primary and secondary outcomes should have been controlled using something like Bonferronisi correction.

5. Non-parametric statistics tests were applied without justification. If the distribution was examined then how it was done should have been described. If the data was truly abnormally distributed then the use of non-non-parametric statistics is justified but their use leads to loss of information. It would have been far better to arithmetically transform the data to near-normal using something like the natural logarithm.

6. Importantly, a more powerful and valid way to have analyse data from a cross-over design would have been to use analysis of variance (ANOVA) and this would have limited the risk of Type I error.
7. Page 5, sentence beginning line 9: one-lung ventilation is not a primary goal in itself but is a consequence of using lung isolation to facilitate surgical access.

8. An explanation why the study appears to have been registered after it was undertaken should have provided.

9. The computerised system used for randomisation should have been described and when and who undertook the randomisation should have been reported.

10. It is stated that the surgeon was blinded but not how this was undertaken. The way the use of a second ventilator was disguised should have been described. Also, the anaesthetist was not blinded and this limitation should have been discussed in the discussion section.

11. It should have been reported whether positive end expiratory pressure was used on the ventilated lung.

12. This reviewer finds the multiple different terminologies used to described lung isolation in the literature confusing and it is particularly so in the paper. Patients who have lung isolation are not always placed in the lateral decubitus position so dependant and non-dependant are not always accurate descriptions. Something like 'surgical or non-ventilated' and 'ventilated' lung seems a better description and less confusing and should have been used consistently throughout the paper.

13. PaO2 has been used as surrogate measure of outcome. Why this was used instead of pulse oximetry should have been explained and if it was measured it would have been useful to report the findings.

14. The figures are poor with no legends to explain the symbols make them extremely difficult to interpret. The PaO2 data should have been described so see the effect of changing technique in both order not just as one overall group to see if there is any carry-over effect.

15. Differences in PaO2 should have been reported as mean differences and 95% confidence intervals if the data was normally distributed or medians with interquartile ranges to facilitate interpretation of the clinical importance of the findings.

16. Just because there are significant difference in PaO2 does not make them clinically important. Reporting the incidences of hypoxia e.g. SpO2 < 85% would have been valuable interpreting the importance of the findings.

17. It is unclear why spirometric findings were collected and there is not explanation provided why they might be affected in the ventilated by the use of CPAP or DLV in the non-ventilated lung. If they add no value to the primary goal of the study then there is not point in reporting them. Similarly, was not explained what the value of reporting pH and bicarbonate was to aim of the study.

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An article of importance in its field

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

Not suitable for publication unless extensively edited

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