Author’s response to reviews

Title: A randomized controlled comparison of non-channeled King Vision, McGrath MAC video laryngoscope and Macintosh direct laryngoscope for nasotracheal intubation in patients with predicted difficult intubations

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Author’s response to reviews:

We are grateful to the reviewers for their constructive comments and thoughtful suggestions. The comments were very helpful as we revised and improved our submission. Based on these comments and suggestions, we have made careful modifications to the original manuscript. Below, the original comments are in black, and our responses are in blue.

Massimiliano Sorbello (Reviewer 1):

1. Paper by Haozhen and colleagues covers an interesting topic in the field of research of direct vs indirect laryngoscopy comparison, in the peculiar setting of nasotracheal intubation. English needs minor review; please uniform verb tenses (sometimes present, sometimes past and sometimes past perfect).

Response: Thanks a lot for the suggestions. We have made some minor corrections of English expression.

2. EGRI 1-7 is really large variation, which might embrace different degrees of difficult laryngoscopy/intubation. This might result in some "generalization" bias, as the same EGRI value could be reached with deeply different difficulty parameters. Just think of inter incisors
distance, which could make particularly hard a channeled video laryngoscope insertion. And, in any case, use of patient's weight rather than BMI remains an important limitation of EGRI.

Response: We greatly appreciate reviewer’s suggestions. We are glad to make some explanations. Common risk factors of difficult intubation include: higher mallampati score, reduced thyromental distance, reduced mouth opening, reduced cervical motion and documented history of difficult intubation. In some studies (Anaesthesia 2012, 67, 132-138; Eur J Anaesthesiol 2011; 28: 673-674; Anesthesiology 2012;116:629-636.), patients with predicted difficult intubations were included if one or more of the above predictors were identified. But the combinations of difficulties were not mentioned. We don’t know how many patients have more than one risk factor. To clearly compare the combination of risk factors between groups, we chose EGRI scores for airway evaluation. The EGRI scores system developed by El-Ganzouri and colleagues combines and stratifies seven variables derived from parameters and observations individually associated with difficult intubation (Anesth Analg 1996; 82: 1197-204). After randomized allocation, our results showed EGRI scores were similar between groups. Of cause, the EGRI scores system is not a perfect one. We totally agree with the comment “the variation of EGRI scores of enrollment is really big”. We also agree with that the same EGRI value could be reached with deeply different difficulty parameters and BMI is better than body weight. Therefore we further compared distribution of each risk factor such as BMI, neck movement <80°, Mallampati III or IV, interincisor gap <3cm, thyromental distance <6cm, ability to prognath (see table 1). These parameters were also comparable between three groups. So we believe the bias of risk factors distribution was effectively reduced. We have made some revision, see page 12, words in blue.

3. Please add some comment on the 15% failure rate of Macintosh laryngoscopy, as it is quite high. Similarly for the 5 CLIV cases you encountered; this is also a quite high incidence.

Response: Thank you so much for pointing this out. We are glad to make some explanations. Firstly, our study was designed as a single-center, randomized controlled trial. We agree that there might be some institutional differences in patients’ characteristics. Secondly, we collected patients only scheduled for elective oral and maxillofacial surgery. These patients have higher rate of airway problems compared to the others. Finally, our results are similar to previous studies (Anesthesiology 2012;116:629-36; Saudi J Anaesth 2018; 12: 35-41). We have added some related comments to the Discussion (page 12 words in blue).

4. OSA was exclusion criteria; you mean previously diagnosed OSA. Was any OSA screening performed during airway evaluation? Could you exclude there was some undiagnosed OSA between the enrolled patients? Accordingly to available literature, this situation is not uncommon in surgical population. Please comment.
Response: We greatly appreciate reviewer’s suggestions. Sorry for the lack of clarity. Severe OSA is closely related to difficult mask ventilation. (J Clin Anesth. 2018;45:63-68.). Therefore, those diagnosed with severe OSA (AHI>30) were excluded during bedside screening. Undiagnosed OSA was included unless mask ventilation was difficult. Based on the suggestion, we have added some comments (page 5-6, words in blue).

5. I would also suggest to insert some comment regarding limitation of video laryngoscopy, that is anyway opportunity of failure, including difficulty to enter the mouth, and caveat that no video laryngoscope allows oxygenation, thus a rescue plan should always be provided (see: Sgalambro F, Sorbello M. Videolaryngoscopy and the search for the Holy Grail. Br J Anaesth. 2017 Mar 1;118(3):471-472.)

Response: Thank you for your comments and valuable suggestions on our manuscript. We do agree with the reviewer. Video laryngoscope is not the Holy Grail. Actually, VLs will fail under certain conditions; the total success range was 37-98% in literatures. Although our results seem to suggest a 100 % success rate of VLs intubation, our result should be interpreted with caution due to small sample size. Also, the results of this study may not be applicable to other types of patients, such as severe OSA or morbid obesity. We have added some comments (page 12-13, words in blue).

6. It would have been nice to have anesthesia depth monitoring, especially if hemodynamic parameters were measured. So to be sure all patients received adequate induction and anesthetic plan at intubation was similar.

Response: Thank you for your reminding. Indeed, anesthesia depth and muscle relaxant was monitored during induction and the surgery. So we sure intubation conditions were similar in all patients. We have added these important information (page 6,7, words in blue).

7. As comparing direct and indirect laryngoscopy, use of POGO or Freemantle score would have been more fair and precise for such a comparison. CL remains always unfair when comparing Macintosh with any videolaryngoscope.

Response: Thank you very much for these comments. We totally agree with the reviewer. If we compared both GL and POGO scores, our results should be more convincing. We have added it to the limitations of the study (page 14, words in blue).
Alaa M Khidr, MD. ABA (Reviewer 2):

Thank you very much for your great efforts, but I have some comments.

1. As you mentioned in your limitations ethically it is not accepted to try a new technique or device or doing a research in patients with a predicted difficult airway which I think from my side a major limitation of your work even with your results which showed no complications.

Response: Thank you for your valuable suggestion. We believe it ethically questionable to test a new intubation device on genuine difficult airway patients. Therefore, further studies may be carried out to clarify these issues.

2. I think it is not acceptable to insert an arterial line to monitor the hemodynamics. What is the problem with NIBP monitoring?

Response: Thank you for your suggestion. We would like to explain it. We applied invasive BP for hemodynamic recording in previous studies (Eur J Anaesthesiol 2010;27:461-467; Eur J Anaesthesiol 2011;28:774-780). Though it was an invasive procedure, it could avoid missing any hemodynamic response. And our protocol was strictly reviewed by IRB from Shanghai Ninth People’s Hospital Affiliated to Shanghai Jiao Tong University School of Medicine (2017-308-T228), and registered at clinicaltrials.gov (NCT03126344). All participants have signed written consents before enrollment and we explain the process to patients carefully before study.

3. I think Cormack-Lehane score is not applicable to VL because the difference in design of each blade and also the presence of the camera make it difficult to compare MAC with VL. BOGO score is used for VL.

Response: Thank you very much for this comment. We totally agree with the reviewer. If we compared both GL and POGO scores, our results should be more convincing. We admit this is a weakness of the experiment. We have added it to the limitations of the study (page 14, words in blue).

4. I used both Macgrath and King vision VL and what I know is that the quality of the image with King Vision is HD with a higher resolution in comparison to MackGrath VL which is not HD with lower resolution and quality.

Response: Thank you for your suggestion and point this out. We totally agree with that King Vision is HD with a higher resolution. But the quality of view on display did not differ between King Vision and McGrath video laryngoscopes. In our study, when using video laryngoscope,
intubators paid more attention to glottis exposure than screen definition. We believe if there is an outcome of image clarity, King Vision will be the better one.