Author's response to reviews

Title: Comparison of the Glidescope(R) and Pentax AWS(R) laryngoscopes to the Macintosh laryngoscope for use by Advanced Paramedics in easy and simulated difficult intubation.

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Author's response to reviews:

Dear Editor,

Please find enclosed a revised manuscript entitled ‘Comparison of the Glidescope® and Pentax AWS® laryngoscopes to the Macintosh laryngoscope for use by Advanced Paramedics in easy and simulated difficult intubation’ by Nasim et al for consideration for publication in BMC Emergency Medicine.

We would like take this opportunity to thank the reviewers for their constructive comments regarding our manuscript. We have revised the submission in response to these comments, and provide a point-by-point response to these comments in the paragraphs that follow.

Response to Reviewer 1

Comment 1: P8 L11: The author mentions that the severity of dental compression is significantly greater with the Macintosh laryngoscope than the AWS and the Glidescope. According to table 1, the severity of dental compression is also significantly greater with the Glidescope than the AWS. The author should clearly describe this significant difference.

Response: We thank the reviewer for pointing this out. We have specifically referred to this difference in dental compression severity in the revised manuscript [Page 8 lines 14 - 15].

Comment 2: P9L23: The meaning of this sentence is unclear. According to table 1 and 3, intubation is all successful with the Macintosh laryngoscope. Therefore, it is impossible to compare the duration of the first “but not the successful” intubation attempt in the scenario 1 and that of the scenario 3.

Response: All AP’s did successfully intubate the trachea in the third scenario, as pointed out by the reviewer. With the exception of one AP, who needed more
than one attempt with the AWS device, all intubation attempts were successful. Therefore, the first intubation attempt was generally, but not always, also the successful attempt. We have rephrased these sentences to make this distinction more clear [Page 9 lines 19 – 21 and lines 23 - 25]

Comment 3: P16L12: There is no symbol denoting the significant difference from same device at the start of the protocol in figure legend of figure 2.
Response: We thank the reviewer for pointing this out. We have inserted the symbol in the revised figure legend [Page 16 line 12].

Response to Reviewer 2

Comment 1: This is a reasonable paper but has a big limitation in that it is a manikin study and thus has very limited applications to live patients. Videolaryngoscopes give excellent views of the airway, but because they are optical devices they are extremely prone to fogging and lens contamination. Fogging and contamination of the lens is the biggest problem one encounters when using these devices in patients, particularly emergency patients. If the lens gets contaminated you can't see anything and the device is useless. This is a real limitation and care should be taken not to extrapolate these data to real patients. This should be emphasized in the paper.
Response: We agree with the reviewer’s point, and did point this out in the original manuscript. Nevertheless, we have revised the limitations section to more fully emphasise the limitations pointed out by the reviewer in regard to extrapolating to the clinical emergency setting. [Page 14 lines 9 - 15]

Comment 2: The difference you were looking for in time to intubate was only 10-20 seconds and I doubt that has any real clinical significance even though there is statistical significance. I can't imaging that a 5 sec difference that you found has any real clinical meaning.
Response: In the emergency setting, brain hypoxia may have a much faster onset to that seen in the operating room, due to the fact that emergency patients may not be pre-oxygenated sufficiently, if at all. Give this well recognized fact, we contend that a difference of 10 – 20 seconds in intubation time may be critical. It is for this very reason that intubation attempts in the emergent setting are generally limited to 30 seconds in duration. In addition, we did not confine our assessment to the duration of tracheal intubation attempts, although this was our primary outcome variable. We also measured a large number of additional variables regarding the difficulty of intubation in these studies, including the need for additional maneuvers to optimize the view of the glottis, and the severity of dental compression. We have revised the discussion to discuss this issue in more detail. [Page 12 line 25 – Page 13 line 5]

Comment 3: The airway on this manikin must have been very easy to intubate as the times to intubate very very fast and the success rates perfect. It look like it doesn't matter what they use, they'll get the tube in.
Response: It should be emphasized that the participants constitute a subgroup of
paramedics termed Advanced Practitioners, are therefore are highly trained in
the use of the Macintosh laryngoscope, and have the opportunity to use this skill
relatively frequently in clinical practice. The APs made good use of optimization
maneuvers, especially in the more difficult cervical immobilization scenario.
Notwithstanding this, the APs did cause a significant amount of dental
compression, attesting to the difficulty of this scenario.

Comment 4: In your conclusion you state that the Pentax AWS performed best
overall, but the way I see it there success rate was 100% for all 3 devices, there
were only a few seconds difference between intubation times and I don't know
what the clinical significance of the dental pressure is. One could make the
argument that it is not justified replacing a 50 dollar laryngoscope that works very
well with a $10,000 videolaryngoscope which also works well.

Response: The aim of the study was to determine whether the AWS and
Glidescope possessed any advantages over the Macintosh laryngoscope when
used by AP's. If these devices did not demonstrate any advantages I these
studies there would be little point in proceeding to clinical studies. As we have
stated in the manuscript, our findings would have to be supported by clinical
studies demonstrating similar advantages for these devices, prior to any
consideration of whether these devices should be introduced into the clinical
setting. We agree that careful consideration would have to be given to the
cost:benefit of introducing one of these devices, but the first step is to determine
whether thee devices have utility in the pre-hospital setting.

We have amended the revised manuscript to specifically allude to the fact that
the cost implications of introducing these devices into the pre-hospital emergency
care setting. [Page 14 lines 13 – 15]

Comment 5: The text is some places is difficult to follow. Please re-read it and try
to make it more understandable (for example in the Abstract, the first sentence of
the Background doesn't read correctly; also the fist paragraph in the Background
needs work, part of it is redundant)

Response: We have revised these sections as suggested, simplified some of the
phrasing and removed redundancy. [Page 2 line 2; Page 3 lines 2 – 5]

Comment 6: I think a better measure for the time to intubation would be from the
time the device is inserted into the mouth until the cuff passes the vocal cords. If
you have those numbers I would use those. The problem with the time interval
you chose is that the process of withdrawing the stylet, inflating the cuff and
confirming tube placement don't have any relevance to the time it takes to
intubate with the device. By counting that extra time interval you might be adding
times on that are independent of the devices.

Response: We have used the measure of tracheal intubation duration advocated
by the reviewer. We defined the duration of each tracheal intubation attempt as
‘the time taken from insertion of the blade between the teeth until the ETT was
deemed to be correctly positioned by each participant’. Where the participant
visualized the ETT passing through the cords, the attempt was considered
complete. Where the participant was unsure as to the position of the ETT, the
time taken to connect the ETT to an Ambu® bag and inflate the lungs was also
included in the duration of the attempt.
We have revised the methods to clarify this point. [Page 5 lines 6 – 7]

Response to Reviewer 3

Comment 1: The authors' institution has published a lot of articles regarding
indirect laryngoscopes. With respect to the BMC Emergency Medicine, we can
see a manikin study, in which the Airtraq and Truview laryngoscopes were
compared to the Macintosh laryngoscope (BMC Emergency Medicine 2009,9:2).
The current report seems to be one of this series. The study is well designed and
performed. The conclusion is drawn from the results of the investigation.
Response: Thank you for your kind comments.

Comment 2: My significant concern, however, is the discussion section. The
second paragraph (Page 11, line13 - Page 12, line 9) is identical to the first and
second paragraph of the previously published discussion by the same author
(BMC Emergency Medicine 2009,9:2, page 5 of 9). This copy and paste will
disappoint the readers of the BMC Emergency Medicine. The other approach for
discussion is likely to attract the interest of many readers. [Major Compulsory
Revisions]
Response: We contend that the issues discussed in these paragraphs, namely
the difficulties in performing tracheal intubation in the pre-hospital setting, are
central to the rationale for these studies, as they were for our earlier study. We
have retained the major sense of these paragraphs but have reworded them
slightly. [Page 11 lines 12 – Page 12 line 9]

Comment 3: Page 8, line 11, Figure 2; line 16, Figure 3. The indicated figures are
not coincident with the text sentences. There are a lot of typographical errors in
figure numbers (Page 9, lines 11, 23, and page10, lines 1, 8). [Minor Essential
Revisions]
Response: Thank you for pointing out this problem. The system for uploading
manuscripts has mis-labelled Figure 1 Panel B as Figure 2. For this reason all
subsequent figures are mis-labelled. For this reason, we have renumbered the
figures, and amended the figure legends and references to the figures in the text
accordingly.

Comment 4: Page 14, the last line – Page 15, line 1. The authors declare that
AM, JO'D, and BDH participated in the study, recruited patients... However, the
reported study is a manikin study, but not a study with patients. [Minor Essential
Revisions]
Response: Thank you. This should have read ‘participants’. We have corrected
this error. [Page 15 line 13]

Comment 5: Page 16, Figure legends, Figure 1. Panel A (Figure 1). The legend
has a strange sentence. Since the GlideScope does not have any side channel, it
cannot be equipped with a tracheal tube. Indeed, I cannot find the tracheal tube
in Figure 1.
Response: Thank you. We have corrected this error and rephrased the legends to include details regarding their use. [Page 16 lines 2 – 8]

Comment 6: Panel B (Figure 2). This is strange, too. The legend confuses the readers. The Pentax-AWS has a built-in LCD monitor, but not eyepiece. [Major Compulsory Revisions]
Response: Thank you. We have corrected this error and rephrased the legends to include details regarding their use. [Page 16 lines 2 – 8]

Comment 7: Page 16, Legend for Figure 2. The legend for a box plot graph should be for Figure 3. [Minor Essential Revisions]
Response: Thank you for pointing out this problem. The system for uploading manuscripts has mis-labelled Figure 1 Panel B as Figure 2. For this reason all subsequent figures are mis-labelled. For this reason, we have renumbered the figures, and amended the figure legends and references to the figures in the text accordingly.

Comment 8: Page 16, Legend for Figure 3. This legend for a graph should be for Figure 4. [Minor Essential Revisions]
Response: Thank you for pointing out this problem. The system for uploading manuscripts has mis-labelled Figure 1 Panel B as Figure 2. For this reason all subsequent figures are mis-labelled. For this reason, we have renumbered the figures, and amended the figure legends and references to the figures in the text accordingly.

Comment 9: Page 18, Reference 16. Cite reference is incomplete. Please provide volume and page numbers. [Minor Essential Revisions]
Response: Thank you. We have corrected this error. [Page 18 line 23]

Comment 10: Pages 21-24. Tables (1, 2, 3 and 4) fail to display the P values for the initial 3-group analysis. Please provide P values of initial 3-group analysis and then, post poc analysis for between-group comparison. In addition, please remove all vertical lines from the tables. [Major Compulsory Revisions]
Response: Thank you. We have provided the P values for the ANOVA. However, the post hoc tests do not give P values, but rather state whether P is less than 0.05. We have indicated this with the asterixes in the tables.
In regard to the tables, while the vertical lines appear in the document, they do not print out. [see revised Tables 1 – 4]