PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Comparison of three video laryngoscopes and direct laryngoscopy for emergency endotracheal intubation - a retrospective cohort study
AUTHORS	Suzuki, Kei; Kusunoki, Shinji; Tanigawa, Koichi; Shime, Nobuaki

VERSION 1 - REVIEW

REVIEWER	William Hurford MD Professor of Anesthesia, University of Cincinnati, Cincinnati, Ohio, USA
REVIEW RETURNED	17-Jul-2018

GENERAL COMMENTS	The authors compared the first attempt success rate of emergency endotracheal intubation using 3 different video laryngoscopes and also a Macintosh laryngoscope. Strengths of the study are an appropriately sized study population and appropriate statistical analyses. Weaknesses of the study are its observational non-randomized study design GENERAL COMMENTS 1. The abbreviations of the various laryngoscope groups is confusing. Why not label them as: Pentax, King, McGrath, and Macintosh. 2. Clarify in the text that the Macintosh blade was used as the reference standard. 3. Please provide additional information about the procedures for video recording and including an intubation in the study. What was the total number of emergency intubations performed during the study period? What proportion of intubations were recorded? How was it decided that a patient would be included/recorded? Who recorded the intubation? Who analyzed the recordings? 4. Did you record information concerning Mallinpati score and laryngoscopic view? 5. Please describe how many different trainees were involved in the study – was there overlap between groups? Were certain trainees represented more than others?

	SPECIFIC COMMENTS p. 5, line 29: Please provide evidence that these three VL systems are the main ones used in clinical practice. There is a reasonable variation in these and other VL scopes around the world. p. 8, line 7: Please provide model/catalog numbers and manufacturers of the various devices. Was the Macintosh laryngoscope a video device or a directly viewing scope? What was its brand and model? p. 13, line 26: What is “AWS”? Is this the Pentax scope? PAS and AWS seem to be used interchangeably at times, which adds to the reader’s confusion. Again, better not to use confusing abbreviations for the groups. p. 13, line 45: The first sentence of this paragraph is confusing. Was there statistical significance in intubation time or not? Table 1: Please provide the physical location of the intubations (ED, type of ICU, hospital floor, etc).

REVIEWER	Tanja Rombey Research Associate, Institute for Research in Operative Medicine (IFOM), Witten/Herdecke University, Germany
REVIEW RETURNED	25-Sep-2018

GENERAL COMMENTS	First, I wish to thank the editors for the opportunity to review this manuscript of a prospective cohort study by Kei Suzuki and colleagues. The authors compared the outcomes following in-hospital emergency endotracheal intubation with one of three types of video laryngoscopes or a Macintosh direct laryngoscope. Their aim was to identify the optimal video laryngoscope based on their observed performance. This is a very interesting study that may be relevant to emergency and critical care physicians around the world. I wish to point out that I am not a physician myself, therefore I focused on general rather than clinical aspects when reviewing the manuscript. No major essential revisions are required, but the authors should perform the following minor essential revisions: 1. Abstract, Objective: Please add “to identify the optimal VL among them”. 2. Abstract, Outcomes: It should be "subgroup analysis" instead of "subanalysis". 3. Strength and limitations: Apart from the last bullet point, this section needs to be revised according to the journals guidance (which states that the section should contain up to five short bullet points, no longer than one sentence each, that relate specifically to the methods and should not include the results of the study). 4. Background, ll. 7-9: Please further specify this statement (e.g. say where PAS, KV and MCG are the video laryngoscopes that

	are mainly used in practice: In Japan? Around the world?) and provide a reference for it. Alternatively, if you are relating to your own institutions, please make this clear. 5. Background, ll. 12-15: Here it is stated that video laryngoscopes have been shown to be superior to Macintosh laryngoscopes in viewing the glottis and in successfully completing TI in patients presenting to emergency rooms. This is not completely true and needs to be corrected. First, the RCTs you cite were performed in intensive care units, not in emergency rooms. Furthermore, TI was completed in all patients in Silverberg et al., whether they had been intubated with a video laryngoscope or a direct laryngoscope. Janz et al. only reported the first-pass intubation success (which did not differ between video laryngoscopy and direct laryngoscopy), but not the overall intubation success. You may find it interesting that, in a more recent systematic review on video laryngoscopy for emergency endotracheal intubation in intensive care units and emergency rooms, which exclusively included RCTs (among them Janz et al. and Silverberg et al.), we found that there is no statistical difference between video laryngoscopy and direct laryngoscopy in terms of the first-pass intubation success (DOI: 10.3238/arztebl.2018.0437). 6. Background, ll. 17-19: Please supplement these statements, particularly a), with numbers. 7. Background, last sentence: Please change the wording to “when compared to ML”. Otherwise it may sound like ML is one of the video laryngoscopes you were comparing. 8. Methods, Study design and setting: Please add information about the departmental experience with the different video laryngoscopes. E.g. since when have they been in use at your institutions? What kind of training (type and frequency) did the physicians typically receive for each one of them? 9. Methods, Data collection and measurements: Please add information about who collected the data. You may want to describe any efforts undertaken to address potential sources of bias here, too. 10. Methods, Sample size and statistical analysis, first sentence: Please cite the study that you based your sample size calculation on. 11. Methods, Sample size and statistical analysis, p. 10, l. 8: In the section “Study design and setting” you state that the laryngoscopes, drugs, or operators for the TI procedures were chosen by the attending physician(s) without protocol. How could you be sure that they would use each device equally often so that you would end up with four equally large groups? Was there a decision rule of when to stop data collection (e.g. once there were 60 or more patients in each group)? 12. Methods, Sample size and statistical analysis, p. 10, ll. 11-13: Please clarify if procedures without an accurate measurement of time needed to perform the TI from the video recording and the procedures without descriptions of the subjective difficulty score were excluded from the analysis of the mean time for each VL and

	the analysis of the difficulty of each VL, respectively, or if they were excluded from all analyses. If they were excluded from all analyses, please justify this. 13. Methods, Sample size and statistical analysis, p. 10, ll. 13-14: The sentence about the post hoc analysis you performed needs to be a bit clearer, e.g. “A post hoc analysis was performed by comparing all laryngoscopes pairwise with each other using Turkey’s test.” 14. Methods, Sample size and statistical analysis, p. 11, ll. 4-5: Please correct the formatting of the citations. 15. Results, Characteristics of the study population: Please state how many TI procedures of all emergency TIs could not be video recorded and why. You may want to further discuss this in the limitations section. 16. Results, Characteristics of the study population: Please say whether there was a difference in the number of attempts until successful TI between the four laryngoscopes. 17. Results, Main results, l. 8: It needs to say “PAS” instead of “AWS”. 18. Results, Main results, ll. 8: Please insert “,respectively ,” after “ML”. Otherwise it may appear to the reader as if you analysed data for the PAS and MCG together and the data for the KV and ML together and compared them as two new groups. 19. Results, Main results, ll. 10-13: I believe it should say “[…], the odds for successful intubation at first attempt were significantly higher with PAS and MCG (table 3).” Please also correct this in the abstract and in the first sentence of the discussion. 20. Results, Main results, ll. 14-15: Please insert “when the laryngoscopes were compared pairwise” after “, though no difference was found”. 21. Discussion: Please, discuss the generalisability of your results briefly. Since I am not a native English speaker, I did not focus on grammar or spelling mistakes. The manuscript is very readable, but there might be a few mistakes that need correcting. I have the following discretionary comments for the authors: 22. To be consistent with the literature, I suggest that you use the term "first-pass (intubation) success" instead of "success rate of first attempts at TI". 23. Title: I suggest that you reword the title so that the study design becomes clearer (e.g. “Comparison of three video laryngoscopes versus direct laryngoscopy for emergency endotracheal intubation - a prospective cohort study”). 24. Abstract, Objective: The first sentence is not relevant at this point and could be deleted. You could instead point out that PAS,

	KV and MCG are video laryngoscopes while the Macintosh laryngoscope is a direct laryngoscope. 25. Abstract, Setting: You could add here that the study took place in emergency departments and intensive care units. 26. Keywords: I suggest that you add the key word “video laryngoscopy”. I would be more than happy to review the revision of this manuscript.

VERSION 1 – AUTHOR RESPONSE

Reviewers' Comments to Author

Reviewer #1

1. ”The abbreviations of the various laryngoscope groups is confusing. Why not label them as: Pentax, King, McGrath, and Macintosh."

RESPONSE: Thank you for your advice. We corrected the abbreviations of videolaryngoscopes as suggested (Pentax, King, McGrath, and Macintosh).

2. "Clarify in the text that the Macintosh blade was used as the reference standard."

RESPONSE: We have added the context according to your comments; Three VLs, including the Pentax (Pentax-Airway Scope™; AWS-S100, HOYA Corporation, Tokyo, Japan), King, (King Vision®, King Systems, Noblesville, IN) and McGrath (McGrath™ MAC; 300-000-000, Medtronic Inc, Minneapolis, MN) systems, as well as a Macintosh laryngoscope (Macintosh blade, KARL STORZ SE & Co, Tuttingen, Germany) as a reference standard, were available in this study. (Page 8, lines 3-7).

3. "Please provide additional information about the procedures for video recording and including an intubation in the study. What was the total number of emergency intubations performed during the study period? What proportion of intubations were recorded? How was it decided that a patient would be included/recorded? Who recorded the intubation? Who analyzed the recordings?"

RESPONSE: The total number of emergency intubations performed during the study period was 1282 and the proportion of recorded intubations was 22%. We recorded intubations using fixed or handy video camera in the room. Unfortunately, the record was limited by when the physician were able to take the videos; thus, only approximately 22% of the cases were recorded. We have added this in the limitation section. Authors KS solely analyzed the recordings.

We have added the context according to your comments in the “Limitation of our study” section. “We included video-recorded cases of TI during the study period. Unfortunately, only 22% of cases were recorded due to the limited availability of physicians who were able to operate the video cameras. Thus, there might be a selection bias. The data collection and analysis were performed by a single author (KS), leaving the potential for observer bias. “(Page 17-18, lines 15-17 and 1-2)

4. "Did you record information concerning Mallinpati score and laryngoscopic view?"

RESPONSE: Unfortunately, we did not record the Mallinpati score or laryngoscopic view in our medical charts.

5. "Please describe how many different trainees were involved in the study – was there overlap between groups? Were certain trainees represented more than others?"

RESPONSE: All intubation attempts by non-experts (n=156) were done by the 67 operators with some overlaps between groups. The median number of attempts per operator was 2 (IQR 1-3). Two trainees had nine attempts and three trainees had eight attempts.

SPECIFIC COMMENTS

"p. 5, line 29: Please provide evidence that these three VL systems are the main ones used in clinical practice. There is a reasonable variation in these and other VL scopes around the world."

RESPONSE: We had unpublished data on the use of video laryngoscopes in emergency medical helicopters, in which the use of the Pentax, King, and McGrath were 75%, 3%, and 22%, respectively (there were overlaps). As the reviewer mentioned, we suspect that there is worldwide variation in the use of the various VLs.

We corrected the context in Background. (Page 5, lines 7-8).

"p. 8, line 7: Please provide model/catalog numbers and manufacturers of the various devices. Was the Macintosh laryngoscope a video device or a directly viewing scope? What was its brand and model?"

RESPONSE: Thank you for your comment. We have added the model and manufacturers of the various devices. (Page 8, lines 3-7). King Vision laryngoscope has no model number.

”p. 13, line 26: What is “AWS”? Is this the Pentax scope? PAS and AWS seem to be used interchangeably at times, which adds to the reader’s confusion. Again, better not to use confusing abbreviations for the groups.”

RESPONSE: Thank you to the point. We have corrected the abbreviation of the Pentax airway scope to “PENTAX”.

"p. 13, line 45: The first sentence of this paragraph is confusing. Was there statistical significance in intubation time or not?"

RESPONSE: There was a significant difference in time in analysis using Kruskal-Wallis tests; however, in post hoc analysis, there was no significant difference for all paired comparisons. We have revised the text as follows; " There were significant differences in the times needed to perform TI among the four laryngoscopes, although no differences were observed in pairwise comparisons of the laryngoscopes in the post hoc analysis.". (Page 13, line16-Page14, line 1).

"Table 1: Please provide the physical location of the intubations (ED, type of ICU, hospital floor, etc)."

Our study included tracheal intubations in the ED or ICU. We have added the distributions of these locations in Table 1.

Responses to Reviewer #2

1. "Abstract, Objective: Please add “to identify the optimal VL among them”."

RESPONSE: We have added this text according to the reviewer's comments. (Page 2, lines 5-6).

2. "Abstract, Outcomes: It should be "subgroup analysis" instead of "subanalysis"."

RESPONSE: We have corrected the word according to the reviewer's comment. (Please see P2, line11-12).

3. "Strength and limitations: Apart from the last bullet point, this section needs to be revised according to the journals guidance (which states that the section should contain up to five short bullet points, no longer than one sentence each, that relate specifically to the methods and should not include the results of the study)."

RESPONSE: Thank you for your instructions. We have deleted the 2nd bullet point and add the context about methodological strengths. (Page 4, lines 5-6).

4. "Background, ll. 7-9: Please further specify this statement (e.g. say where PAS, KV and MCG are the video laryngoscopes that are mainly used in practice: In Japan? Around the world?) and provide a reference for it. Alternatively, if you are relating to your own institutions, please make this clear."

RESPONSE: We had unpublished data on the use of video laryngoscopes in emergency medical helicopter, in which the use of Pentax, King, and McGrath were 75%, 3%, and 22%, respectively (there were overlaps).

We have edited this text in Background. (Page 5, lines 7-9).

5. "Background, ll. 12-15: Here it is stated that video laryngoscopes have been shown to be superior to Macintosh laryngoscopes in viewing the glottis and in successfully completing TI in patients presenting to emergency rooms. This is not completely true and needs to be corrected.

First, the RCTs you cite were performed in intensive care units, not in emergency rooms. Furthermore, TI was completed in all patients in Silverberg et al., whether they had been intubated with a video laryngoscope or a direct laryngoscope. Janz et al. only reported the first-pass intubation success (which did not differ between video laryngoscopy and direct laryngoscopy), but not the overall intubation success.

You may find it interesting that, in a more recent systematic review on video laryngoscopy for emergency endotracheal intubation in intensive care units and emergency rooms, which exclusively included RCTs (among them Janz et al. and Silverberg et al.), we found that there is no statistical difference between video laryngoscopy and direct laryngoscopy in terms of the first-pass intubation success (DOI: 10.3238/arztebl.2018.0437). "

RESPONSE: Thank you for your comments and introduction to this interesting systematic review. We have corrected the sentences and cited the references as follows; " However, a randomized trial in intensive care units (ICUs) showed no difference in first-pass intubation success rates between VLs and the Macintosh system [11]. A systematic review of emergency TIs in emergency departments (EDs) and ICUs showed that the use of VLs had no significant advantage with regards to first-attempt success rates, although their use was significantly associated with a lower number of intubation attempts [12]. However, these studies included various types of VL in a single group and did not consider the characteristics of each VL. To our knowledge, no study has examined the relative performance of VLs, especially in emergency TIs." (Page5, line14- Page 6, line 4).

6. "Background, ll. 17-19: Please supplement these statements, particularly a), with numbers."

RESPONSE: We have added the context with numbers according to your comments. (Page 6, lines 6-10).

7. "Background, last sentence: Please change the wording to “when compared to ML”. Otherwise it may sound like ML is one of the video laryngoscopes you were comparing."

RESPONSE: We have changed this text according to your recommendation. (Page 6, line 12).

8. "Methods, Study design and setting: Please add information about the departmental experience with the different video laryngoscopes. E.g. since when have they been in use at your institutions? What kind of training (type and frequency) did the physicians typically receive for each one of them?"

RESPONSE: We have added the context according to your comments as follows; "These VL had been commonly used prior this study for several years in both institutions and there was no specific off-the-job training for these VLs." (Page 8, lines 7-9).

9. "Methods, Data collection and measurements: Please add information about who collected the data. You may want to describe any efforts undertaken to address potential sources of bias here, too."

RESPONSE: We have added this information to the “Methods” and “Study Limitations” according to your comments; "Data collection and analysis were performed by a single author (KS)." (Page 10, lines 2-3 and Page 18, line 1). We have also mentioned the possible bias due to the single observer in the limitation section (Page 18, lines 1-2).

10. "Methods, Sample size and statistical analysis, first sentence: Please cite the study that you based your sample size calculation on."

RESPONSE: We have based our sample size on our own unpublished data (Page 10, line 11).

11. "Methods, Sample size and statistical analysis, p. 10, l. 8: In the section “Study design and setting” you state that the laryngoscopes, drugs, or operators for the TI procedures were chosen by the attending physician(s) without protocol. How could you be sure that they would use each device equally often so that you would end up with four equally large groups? Was there a decision rule of when to stop data collection (e.g. once there were 60 or more patients in each group)?"

RESPONSE: The estimated the frequency of use of each of the four laryngoscopes in the two institutions was mostly equal based on clinical impression. There was not a decision rule for when to stop data collection.

12. "Methods, Sample size and statistical analysis, p. 10, ll. 11-13: Please clarify if procedures without an accurate measurement of time needed to perform the TI from the video recording and the procedures without descriptions of the subjective difficulty score were excluded from the analysis of the mean time for each VL and the analysis of the difficulty of each VL, respectively, or if they were excluded from all analyses. If they were excluded from all analyses, please justify this."

RESPONSE: We have clarified in the methods section that procedures without an accurate measurement of the time needed to perform the TI from the video recording and procedures without descriptions of the subjective difficulty score were excluded from the analysis of the mean time for each VL and the analysis of the difficulty of each VL, respectively. (Page 11, lines 3-5)

13. "Methods, Sample size and statistical analysis, p. 10, ll. 13-14: The sentence about the post hoc analysis you performed needs to be a bit clearer, e.g. “A post hoc analysis was performed by comparing all laryngoscopes pairwise with each other using Turkey’s test.”"

RESPONSE: We have edited the sentences about post hoc analysis according to your recommendation. (Page 11, lines 5-6).

14. "Methods, Sample size and statistical analysis, p. 11, ll. 4-5: Please correct the formatting of the citations."

RESPONSE: We have corrected the citation style accordingly. (Page 11, lines 13-14).

15. Results, Characteristics of the study population: Please state how many TI procedures of all emergency TIs could not be video recorded and why. You may want to further discuss this in the limitations section.

RESPONSE: A total number of 1,282 emergency intubations were performed during the study period, 22% of which were recorded. We recorded the intubations using fixed or handy video cameras in the room. Unfortunately, the recording was limited to cases in which a physician was able to operate the video cameras. We have added this information to the limitation section. KS solely analyzed the recordings.

We have added the following to Study Limitations section according to your comments. “We included video-recorded cases of TI during the study period. Unfortunately, only 22% of cases were recorded due to the limited availability of physicians who were able to operate the video cameras. Thus, there might be a selection bias. The data collection and analysis were performed by a single author (KS), leaving the potential for observer bias.“(Page 17, lines 15-17-Page 18, lines 1-2)

16. "Results, Characteristics of the study population: Please say whether there was a difference in the number of attempts untl successful TI between the four laryngoscopes."

RESPONSE: We have added the number of attempts until successful TI; " The number of attempts until successful TI were 1.3±0.9 with Pentax, 1.4±0.7 with King, 1.3±0.6 with McGrath, and 1.5±0.7 with Macintosh (P=0.007).” There was a significant difference between the four laryngoscopes (Page 12, lines 14-16)

17. Results, Main results, l. 8: It needs to say “PAS” instead of “AWS”.

RESPONSE: We have corrected this text, as suggested by Reviewer #1.

18. Results, Main results, ll. 8: Please insert “,respectively ,” after “ML”. Otherwise it may appear to the reader as if you analyzed data for the PAS and MCG together and the data for the KV and ML together and compared them as two new groups.

RESPONSE: We have inserted “respectively”, after “ML” according to your comment. (Page 13, line 10)

19. Results, Main results, ll. 10-13: I believe it should say “[…], the odds for successful intubation at first attempt were significantly higher with PAS and MCG (table 3).” Please also correct this in the abstract and in the first sentence of the discussion.

RESPONSE: We have corrected this text according to the reviewer's comment as follows; (Page 3, lines 4-7, Page 13, lines 14-15, and Page 15, lines 4-5)

20. Results, Main results, ll. 14-15: Please insert “when the laryngoscopes were compared pairwise” after “, though no difference was found”.

RESPONSE: We have inserted the context as recommended. (Page 13, line 17-Page 14, line 1)

21. Discussion: Please, discuss the generalisability of your results briefly.

RESPONSE: We have edited the text according to your comments; " The results of the present study suggest the usefulness of the Pentax or McGrath VLs for emergency TI performed by novice physicians. However, the generalizability of the results for intubation in other settings (in the operating theater or prehospital settings, or by non-physicians) remains uncertain.” (Page 17, lines 6-9).

22. "To be consistent with the literature, I suggest that you use the term "first-pass (intubation) success" instead of "success rate of first attempts at TI"."

RESPONSE: We appreciate your advice. We have used the term "first-pass (intubation) success" in the text and tables.

23. "Title: I suggest that you reword the title so that the study design becomes clearer (e.g. “Comparison of three video laryngoscopes versus direct laryngoscopy for emergency endotracheal intubation - a prospective cohort study”)."

RESPONSE: Thank you for your advice. We have changed the title to “Comparison of three video laryngoscopes and direct laryngoscopy for emergency endotracheal intubation - a retrospective cohort study”. (Page 1, lines 2-3)

24. "Abstract, Objective: The first sentence is not relevant at this point and could be deleted. You could instead point out that PAS, KV and MCG are video laryngoscopes while the Macintosh laryngoscope is a direct laryngoscope."

RESPONSE: We have corrected this sentence according to your comment as follows: " This study compared the performances of three video laryngoscopes (Pentax-Airway Scope™ [Pentax], King Vision® [King], and McGrath® MAC [McGrath]) with the Macintosh direct laryngoscope [Macintosh], as reference in emergency tracheal intubations (TIs) to identify the optimal video laryngoscopes among them." (Page 2, lines 2-6)

25. "Abstract, Setting: You could add here that the study took place in emergency departments and intensive care units."

RESPONSE: We have corrected this sentence according to the reviewer's comment. (Page 2, line 7)

26. Keywords: I suggest that you add the key word “video laryngoscopy”.

RESPONSE: We have added the key word according to your comment. (Page 3, line 14)

VERSION 2 – REVIEW

REVIEWER	William E. Hurford, MD Department of Anesthesiology University of Cincinnati Cincinnati, OH, 45267, USA
REVIEW RETURNED	07-Dec-2018

GENERAL COMMENTS	The authors compared the first-pass success rates of emergency endotracheal intubations using 4 different laryngoscope designs. The study was designed as a retrospective cohort design using video recordings of intubations at two centers. The Pentax and McGrath laryngoscopes has a higher first-pass success rates, especially with non-expert operators.¶ The revision has greatly improved the paper and the major concerns of the reviewer have been adequately addressed.¶ Several recent studies should be incorporated into the introduction and discussion:¶ Lascarrou JB et al. Video Laryngoscopy vs Direct Laryngoscopy on Successful First-Pass Orotracheal Intubation Among ICU

	Patients: A Randomized Clinical Trial. JAMA. 2017 Feb 7;317(5):483-493¶ Lewis SR et al. Videolaryngoscopy versus direct laryngoscopy for adult patients requiring tracheal intubation. Cochrane Database Syst Rev. 2016 Nov 15;11:CD011136¶ Jiang J et al. Video laryngoscopy does not improve the intubation outcomes in emergency and critical patients - a systematic review and meta-analysis of randomized controlled trials. Crit Care. 2017 Nov 24;21(1):288¶ The discussion should address why the current study, which suggests a benefit for video laryngoscopy, appears at odds with recent studies and reviews that call the benefit of VL into doubt. ¶

REVIEWER	Tanja Rombey Institute for Research in Operative Medicine, Witten/Herdecke University
REVIEW RETURNED	16-Dec-2018

GENERAL COMMENTS	Thank you for the opportunity to review the revised version of the manuscript (now) titled “Comparison of three video laryngoscopes and direct laryngoscopy for emergency endotracheal intubation - a retrospective cohort study” by Suzuki and colleagues. The authors have adequately addressed the majority of the initial comments by the editor, reviewer #1 and myself, leaving only a few more things to say: 1. P. 2, ll. 5-6: Considering that this was in fact a retrospective study, please delete “[…] to identify the optimal video laryngoscopes among them.” Although I was the one initially suggesting that you add this information, I came to the conclusion that the optimal video laryngoscope (more generally, the optimal treatment for a given condition) can only be identified by conducting a prospective study like a randomized controlled trial, while this study is very useful to compare the performance of the video laryngoscopes under real world conditions. 2. Abstract: To be consistent with the journal guidelines for abstracts, please add “Design: Retrospective cohort study.” after “Objective:”. Instead of “Outcomes:” you should say “Primary and secondary outcome measures:”. 3. P. 2, ll. 16, 18, p. 3, l. 3: You should state which tests you have used (e.g., in brackets after the respective outcome measure) when reporting p-values. 4. P. 2, ll. 16-17: You should explain what you tested using the post hoc analysis. Otherwise the sentence does not contain valuable information and could be deleted from the abstract. 5. P. 3, l. 7: You should add here: “than the King (odds ratio = 1.056, 95% confidence interval 0.487-2.289, p = 0.889) when compared to the Macintosh (reference, odds ratio = 1).” 6. P. 3, ll. 8-10: According to the previous comment, you should re-word this sentence: “ … than that of the King laryngoscope

	when compared to the Macintosh laryngoscope, especially for non-expert operators." 7. P. 5, ll. 7-8: Please add "Amongst others" at the beginning of this sentence. 8. P. 6, ll. 11-13: Please re-word according to comment #1. 9. P. 13, ll. 10-11: Here you say that the difference was significant only in the subgroup of non-expert operators. However, the p-values in the legend under Table 2 indicate that it was also significant overall (non-experts plus experts). 10. P. 13, ll. 11-12: This sentence is a little confusing, considering the previous sentence and above comment. Did you mean "OVERALL, the first-pass intubation success rates were similar in non-experts and experts, with 67% and 73%, respectively."? 11. P. 13, ll. 12-15: Please complement this sentence according to comment #6. 12. P. 14, l. 1: Please add "(Table 4)" after "analysis". 13. P. 15, ll. 4-7: Please complement this sentence according to comment #6. 14. P. 19, ll. 2-4: Please complement this sentence according to comment #6. 15. In four instances throughout the manuscript (p. 2, l.13; p. 3, l.9; p. 15, l. 2; p. 15, l. 11) you used "first pass success" instead of "first pass intubation success". Please correct these.

VERSION 2 – AUTHOR RESPONSE

Response to Reviewer #1

Several recent studies should be incorporated into the introduction and discussion:

・Lascarrou JB et al. Video laryngoscopy vs direct laryngoscopy on successful first-pass orotracheal intubation among ICU patients: a randomized clinical trial. JAMA. 2017 Feb 7;317(5):483-493

・Lewis SR et al. Video laryngoscopy versus direct laryngoscopy for adult patients requiring tracheal intubation. Cochrane Database Syst Rev. 2016 Nov 15;11:CD011136¶

・Jiang J et al. Video laryngoscopy does not improve the intubation outcomes in emergency and critical patients - a systematic review and meta-analysis of randomized controlled trials. Crit Care. 2017 Nov 24;21(1):288

The discussion should address why the current study, which suggests a benefit for video laryngoscopy, appears at odds with recent studies and reviews that call the benefit of VL into doubt.

RESPONSE: Thank you for your comment. We have made additions to the text in the discussion section (Page 16, lines 9 - 13; page 17, line 16; and page 18 - line 3) to address this concern.

We also added their respective references. (Page 27, lines 2-10)

Page 16, Line 9; “The use of a stylet facilitates manipulation of the tracheal tube adjacent to the the glottis. However, a randomized clinical trial in the ICU population, which showed no improvement in a McGrath-used first-pass intubation, did not use a stylet, which was used in all McGrath cases in here [20]. This may be the reason for the nonconformance between the studies’ results.”

Page 17, Line 16; Systematic review and meta-analysis of randomized controlled trials revealed that video laryngoscopy does not improve first-attempt intubation success rates compared to that of direct laryngoscopy in emergency, critical and surgical patients [21, 22]. However, multiple models of VLs with various characteristics were combined as a “VL group” in the analysis. Here, we intended to compare the individual performances of VLs.”

Responses to Reviewer #2

1. P. 2, ll. 5-6: Considering that this was in fact a retrospective study, please delete “[…] to identify the optimal video laryngoscopes among them.” Although I was the one initially suggesting that you add this information, I came to the conclusion that the optimal video laryngoscope (more generally, the optimal treatment for a given condition) can only be identified by conducting a prospective study like a randomized controlled trial, while this study is very useful to compare the performance of the video laryngoscopes under real world conditions.

RESPONSE: Thank you for your comment. We have deleted the sentence as suggested.

2. Abstract: To be consistent with the journal guidelines for abstracts, please add “Design: Retrospective cohort study.” after “Objective:”. Instead of “Outcomes:” you should say “Primary and secondary outcome measures:”.

RESPONSE: Thank you for your comment. We have made changes to reflect the terms “Design” and “Primary outcome measure”. (Page 2, lines 6 and 11)

3. P. 2, ll. 16, 18, p. 3, l. 3: You should state which tests you have used (e.g., in brackets after the respective outcome measure) when reporting p-values.

RESPONSE: Thank you for your comment. The test we used has been duly stated as “Fisher’s exact test”. (Page 2, lines 17 18; Page 3, line 3)

4. P. 2, ll. 16-17: You should explain what you tested using the post hoc analysis. Otherwise the sentence does not contain valuable information and could be deleted from the abstract.

RESPONSE: Thank you for your comment. We have deleted the sentence from the abstract as suggested.

5. P. 3, l. 7: You should add here: “than the King (odds ratio = 1.056, 95% confidence interval 0.487-2.289, p = 0.889) when compared to the Macintosh (reference, odds ratio = 1).”

RESPONSE: Thank you for your comment. We have added the sentence as below; “the Pentax (odds ratio = 3.422, 95% confidence interval 1.551-7.550; P=0.002) and McGrath (3.758, 1.640-8.612; P=0.002) instruments showed significantly higher first-pass intubation success odds when compared to the Macintosh laryngoscope (reference, odds ratio = 1). The King instrument, however, (odds ratio = 1.056, 95% confidence interval 0.487-2.289, p = 0.889) failed to show any significant superiority.” (Page 3, Lines 4-9).

6. P. 3, ll. 8-10: According to the previous comment, you should re-word this sentence: “… than that of the King laryngoscope when compared to the Macintosh laryngoscope, especially for non-expert operators.”

RESPONSE: Thank you for your comment. We have re-worded the sentence. (Page 3, lines 11 - 12)

7. P. 5, ll. 7-8: Please add “Amongst others” at the beginning of this sentence.

RESPONSE: Thank you for your comment. We added the phrase to the beginning of the sentence per your suggestion. (Page 5, line 7)

8. P. 6, ll. 11-13: Please re-word according to comment #1.

RESPONSE: Thank you for your comment. We corrected this sentence as below ;

“The aim of this study was to to compare the emergency TI performances of the Pentax, King, and McGrath systems with that of the Macintosh for the emergency TI in the ED or ICU.” (Page 6, lines 11 - 12)

9. P. 13, ll. 10-11: Here you say that the difference was significant only in the subgroup of non-expert operators. However, the p-values in the legend under Table 2 indicate that it was also significant overall (non-experts plus experts).

RESPONSE: Thank you for your comment. We have revised this sentence.

“although there were no significant differences in the expert operators’ subgroup” (Page 13, lines 10 - 11)

10. P. 13, ll. 11-12: This sentence is a little confusing, considering the previous sentence and above comment. Did you mean “OVERALL, the first-pass intubation success rates were similar in non-experts and experts, with 67% and 73%, respectively.”?

RESPONSE: Thank you for your comment. We have revised this sentence;

(Page 13, lines 11 - 12)

11. P. 13, ll. 12-15: Please complement this sentence according to comment #6.

RESPONSE: Thank you for your comment. We have re-worded the sentence. (Page 13, lines 16 - 17)

12. P. 14, l. 1: Please add “(Table 4)” after “analysis”.

RESPONSE: Thank you for your comment. We have inserted “Table 4” in parenthesis into the sentence. (Page 14, line 3)

13. P. 15, ll. 4-7: Please complement this sentence according to comment #6.

RESPONSE: Thank you for your comment. We have re-worded the sentence. (Page 15, lines 6 - 7)

14. P. 19, ll. 2-4: Please complement this sentence according to comment #6.

RESPONSE: Thank you for your comment. We have re-worded the sentence. (Page 20, lines 4 - 5)

15. In four instances throughout the manuscript (p. 2, l.13; p. 3, l.9; p. 15, l. 2; p. 15, l. 11) you used “first pass success” instead of “first pass intubation success”. Please correct these.

RESPONSE: Thank you for your comment. We have revised the respective sections of the text (Page 2, line 14, Page 3, line 6, Page 15, lines 2, 12).

VERSION 3 - REVIEW

REVIEWER	Tanja Rombey Insitut for Research in Operative Medicine, Witten/Herdecke University
REVIEW RETURNED	18-Jan-2019

GENERAL COMMENTS	The authors have adaequately addressed all my previous comments. One last thing: I believe that the second sentence of the abstract needs to be corrected (it seems like there is an "in" missing and "reference" should come after "as").