PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (**http://bmjopen.bmj.com/site/about/resources/checklist.pdf**) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	The efficacy of various types of laryngoscope (Direct, Pentax Airway Scope, and GlideScope) for endotracheal intubation in various cervical immobilization scenarios: A randomized crossover simulation study
AUTHORS	Kim, Jong Won; Lee, Kyeong Ryong; Hong, Dae Young; Baek, Kwang Je; Lee, Young Hwan; Park, Sang O

VERSION 1 - REVIEW

REVIEWER	Antonio Celenza University of Western Australia Australia
REVIEW RETURNED	02-Feb-2016

GENERAL COMMENTS	Thank you for the opportunity to review this manuscript. Although there have been many published papers on video-laryngoscopy in difficult airway scenarios (both simulated and real), this study does add new information regarding the differences between types of laryngoscopes in different simulated cervical spine immobilisation scenarios. I believe that this paper should be published with minor amendments, as detailed below: Page 1, line 14: The sentence containing "…ETI should only be applied with cervical immobilisation…until any possibility of …injury…is completely excluded" may be ATLS dogma, but is clinically impossible to do in most trauma cases. I would remove this sentence as it does not add to the paper and is not consistent with most clinical practice. If the sentence is to be kept, then I would change the word "applied" to "used". A similar comment applies to page 12, line 5 which states a cervical collar is an essential device. This sentence should also be removed. Page 5, line 41: add "and" before Glidescope. Page5, line 47: change "different" to "differing" Page 6, methods section: No sample size calculation was derived. I assume this was a convenience sample of participants. Page 6, line16: change "setting" to design" Page 7, line 38: I am interested in how the timing was calculated in terms of removal of the endotracheal stylet. Was this used in all cases? If so, this needs to be mentioned in the methodology. In my experience, the PAWS does not use a stylet – how was the timing for completed intubation calculated for the PAWS? Since the operators had 120 seconds for intubation, were multiple attempts

	allowed? Page 8, results section: Although you mention age and ETI experience of the subjects, did you survey their prior experience in the use of the videolaryngoscopes? This may have biased the results in favour of one or the other if there had been prior routine use. Page 8, results section: I am unsure as to the requirements of the journal for confidence intervals, but I would expect confidence intervals to be calculated around any point estimate described. Page 8, line34: change “difference” to “differences” Page 12, line 10: change “cervix” to “spine” Page 14, line 5: change “available” to “suitable” Page 14, line 10: I believe that “the operator should intubate with a neck collar in situ” is inappropriate practice as it will lead to a higher rate of failed airway management. Perhaps the clinical practice is different in Korea, but my understanding is that less restrictive cervical spine immobilisation techniques have not been shown to worsen spinal outcomes, yet allow satisfactory intubation conditions, compared to keeping the collar in situ.

REVIEWER	Seth Manoach MD Assistant Professor of Medicine, Division of Pulmonary and Critical Care Medicine Weill Cornell Medical College 170 Williams Street, 7th floor New York, NY !0035 (n.b. Board Certified in Emergency Medicine, Internal Medicine-Critical Care Medicine, and Neurocritical Care)
REVIEW RETURNED	22-Apr-2016

GENERAL COMMENTS	Interesting paper, multiple problems, requires extensive revisions/commentary/clarification 1. Intubating patients in C-collars was never the standard of care, should never be performed with DL or rigid VL, and is relevant only to exaggerate the effects of manual-in-line stabilization on difficulty in obtaining a direct or rigid videolarygoscopy view of the glottis and passing a tube (see Malik et. al., a similar study to the present one, done in anesthetized humans, for a description of the technique, or the 1984 ATLS manula cited in Manoach et. al.). This must be clearly stated in the manuscript. 2. MILS was long considered the standard of care, although work by Lennsarson et. al., myself with Paladino, Santoni et. al. and others have shown that MILS as classically described increases pressure transmitted to the cervical spine by the laryngoscope blade (proposed as a mechanism by myself and Paladino to explain the work of Lennarson et. al. showing increased subluxation with MILS -- confirmed by Santoni et. al.)

	3. In addition, Santoni et. al. and Thiboutot et. al. have shown that when experienced anesthesiologists are asked to intubate class I and II airways using DL under ideal conditions but with rigidly applied MILS, they fail to do so about 50% of the time. 4. The two above considerations have caused us and others to advocate a slight relaxation of MILS in some situations. We also do so because closed claims analyses have shown that the majority of secondary injury during intubation occurs not with acute trauma but during intubation of elderly patients with degenerative diseases (Hindman et. al.) or in other non-trauma situations. That said, it seems reckless to allow patients' necks to flop around when they are at risk for secondary neurologic injury, so the authors' work still matters. The authors themselves are too rigid in their position on an important issue that deserves a more considered stance. 5. The articles mentioned in #3 above also lead me to question the fidelity of the authors' mannequin model or their MILS technique. Given the failure of experienced anesthesiologists to successfully intubate easy airways when MILS is applied as described, it is difficult to see how relatively inexperienced airway managers (30 intubations is not sufficient to be called experienced with ETI) could successfully intubate 94% of the time with DL. The authors are not alone in reaching a better outcome than would seem possible: in consecutive studies conducted around comparing the performance of emergency medicine and anesthesia trainees in trauma airways, near 99% success was achieved by each group of trainees. Since trauma patients are ideal models for themselves it could also be the case that the mannequin in this study was fine, but the technique in those studies and in this one was not quite as rigid as it is classically intended to be. This matters because I find that the first folks to intubate a patient get rigid MILS, and tend to fail, while the last person, who typically steps up as the patient is desaturating gets none, or almost none, and succeeds. This may cause subtle cognitive harm that needs to be balanced against uncertain benefits and some potential c-spine compromise associated with rigid MILS. The puzzle – why MILS, when applied in controlled situations with experienced anesthesiologists attempting to intubate "easy" patients half the time, while novices with 30 intubations or trainees seem to be able to do so much better – is worth some space in the discussion. Something has to give. The mannequin or the technique. Something always does, I would just like the authors to take their best shot at coming up with an answer, or cede that to an editorialist. 4. The Pentax scope seems to be a great device in the OR and in mannequins, with but, as the authors point out, traumatized humans bleed a lot and vomit, and have a lot of secretions. Unlike the glidescope or DL, the pentax optic, along with the ETT passes through the hypopharynx on its way to the glottis. This is where secretions pool, and this leads me to depend on my anterior-optic (ie higher up in spatial, not anatomical terms in the supine patient) devices like the glidescope or mcgrath mac, or even DL, including straight blades. When the patient's mouth is a mess, as it so frequently is in trauma, the more anterior the optic (and so the tube must pass with a separate stylet) the better. Iif the airway is a terrible mess, the eye in the laryngoscopist's head may be better than any eye (optic) stuck in the patient's mouth. So DL still matters. As before this is a mannequin problem, but it's also an OR-as-emergency-airway-model problem. 5. The authors do state primary and secondary outcomes, but this is a bit confusing in a study with so many measurements. Strictly

	speaking one should have a research hypothesis and state what that is. I am not a firm about this, and not sure if it would have helped here. I still understand what the authors are trying to do. 6. The authors should state why they undertook this study when there are similar human ones. I think a reason to do so may lie with the C-collar intubations. They demonstrate that under ideal circumstances, the pentax seems to be the easiest device to line up and make the curve. If there is another important lesson, it might be that novices seem to do better with the pentax and glidescope. Although this is known, commentary about the soiling issue may be expanded on by the authors (or an editorialist). 6. I just do not believe the glide scope breaks teeth more than other devices. The clicking of a mannequin's plastic teetch cannot be equated to dental trauma as the tissue/material properties are so different (eg elasticity of material that serves the function of soft tissues). DL would be expected to do so more often. Moreover, relatively inexperienced airway managers (eg those with 30-50 intubations) tend to try to hold the glidescope as a direct laryngoscope is meant to be held. I am not sure if this is because it has caused them so much anxiety to learn to hold a directl largyngoscope dibefore they try something else, or there is some other reason. In any case, the curve of the GVL causes it to bend around the teeth when lifted straight up, as one is supposed to use it. 7. I complement the authors on mixing up the device experience to minimize device specific learning. With so many devices, anatomic learning is probably the most important factor in airway management success and solves the problems of teaching the topic as technology changes. I think this piece might be published with major revisions. Many of the limitations are as important as primary outcomes and deserve to be aired. Seth Manoach References: Ann Emerg Med. 2007 Sep;50(3):236-45. Epub 2007 Mar 6. J Neurosurg. 2000 Apr;92(2 Suppl):201-6 J Neurosurg. 2001 Apr;94(2 Suppl):265-70. Anesthesiology. 2009 Jan;110(1):24-31. doi: 10.1097/ALN.0b013e318190b556. Anesthesiology. 2009 Jan;110(1):6-7. doi: 10.1097/ALN.0b013e318190b27b Can J Anaesth. 2009 Jun;56(6):412-8. doi: 10.1007/s12630-009-9089-7. Epub 2009 Apr 24 Anesthesiology. 2011 Apr;114(4):782-95. doi: 10.1097/ALN.0b013e3182104859. Ann Emerg Med. 2004 Jan;43(1):48-53. J Trauma. 2001 Dec;51(6):1065-8.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1:

1. Page 1, line 14: The sentence containing "…ETI should only be applied with cervical immobilisation…until any possibility of …injury…is completely excluded" may be ATLS dogma, but is clinically impossible to do in most trauma cases. I would remove this sentence as it does not add to

the paper and is not consistent with most clinical practice. If the sentence is to be kept, then I would change the word “applied” to “used”. A similar comment applies to page 12, line 5 which states a cervical collar is an essential device. This sentence should also be removed.
>> Reply : As you mentioned, this sentence seemed to be inappropriate. We corrected the sentence in the introduction section. In addition the sentence in the discussion was removed. Thank you for your kind comment.(Page 5, Lines 5-7)

2. Page 5, line 41: add “and” before Glidescope.
Page5, line 47: change “different” to “differing”
Page 6, line16: change “setting” to design”
Page 8, line34: change “difference” to “differences”
Page 12, line 10: change “cervix” to “spine”
Page 14, line 5: change “available” to “suitable”
>> Reply : Thank you for your kind correction. We add and change the word, as the reviewer suggested.

3. Page 6, methods section: No sample size calculation was derived. I assume this was a convenience sample of participants.
>> Reply : We calculated the sample size of the study, but it was not described well in the previous manuscript. This was newly added to the method section in the revised manuscript. (Page 8, Lines 5-11)

4. Page 7, line 38: I am interested in how the timing was calculated in terms of removal of the endotracheal stylet. Was this used in all cases? If so, this needs to be mentioned in the methodology. In my experience, the PAWS does not use a stylet – how was the timing for completed intubation calculated for the PAWS? Since the operators had 120 seconds for intubation, were multiple attempts allowed?
>> Reply : As the reviewer mentioned, the PAWS does not use a stylet. As the reviewer mentioned, PAWS does not use a stylet. We could not calculate the timing by removal of the stylet in the case of PAWS. “Complete ETI” means that the operator can supply ventilation to the patient (a manikin in this study) via an Ambu-Bag. “the time taken to complete ETI” was defined as the time taken between touching each device to the participant and completing intubation when the operator removed the stylet after tube placement in the trachea in the case of DL or GVL, or completing the tube placement in the trachea in the case of PAWS. (Page 7, Lines 19-22)
We allowed multiple attempts within 120 s and added it to the revised manuscript.
(Page 7, Line 12 / Page 9, Line 4-5, 9-10, 15-116 / Page 10, Lines 2-3 / Page 12, Table 2)

5. Page 8, results section: Although you mention age and ETI experience of the subjects, did you survey their prior experience in the use of the video laryngoscopes? This may have biased the results in favour of one or the other if there had been prior routine use.
>> Reply : Video laryngoscopes were introduced to the Korean physicians when we started the experiment, and previously they had no experience in their use. Most physicians were inexperienced and not experts. They also had no prior clinical experience of ETI in patients with an immobilized neck. In this study, participants were not expert or experienced but were “less” experienced or novice intubators. We used the term “experienced” in the previous manuscript, and this should have been corrected (we omitted the term “experienced” in the revised manuscript).
In the present study, to balance the skill levels for each of the airway devices, we held an airway training programme, and all participants were trained in all airway devices (DL, GVL, and PAWS). They achieved similar success in ETI using the three devices before the simulation trial under the various cervical immobilization scenarios. (Page 6, Lines 8-10)
In the MILS scenario, a better result was achieved with a DL than we were expecting with other devices. Although the prior experience of operators with intubation using DL was not extensive, a DL

learning effect might have influenced the results. This should be mentioned, and we added this to the revised manuscript as a limitation of the study. We added a sentence to the discussion section. (Page 19 Lines 1- 6)

6. Page 14, line 10: I believe that “the operator should intubate with a neck collar in situ” is inappropriate practice as it will lead to a higher rate of failed airway management. Perhaps the clinical practice is different in Korea, but my understanding is that less restrictive cervical spine immobilisation techniques have not been shown to worsen spinal outcomes, yet allow satisfactory intubation conditions, compared to keeping the collar in situ.
>> Reply : As the reviewer mentioned, we agree that MILS is the standard of care for airway management of injured patients. Intubation of patients in cervical collars is not standard and may be an inappropriate practice because it might lead to a higher rate of ETI failure.However, considering some practical limitations of MILS, the immediate intubation in patients wearing neck collars without removal of it is not easily abandoned. Intubation under MILS may be delayed in patients wearing a cervical collar because of the requirement for its careful removal. In addition, additional cervical spine injuries may result during emergency removal of the collar. In particular, we should consider the situation where a second rescuer is not available on site or a second rescuer is not able to administer safe MILS technique in an emergency. In some cases, the operator may have to postpone emergency ETI until an expert assistant for MILS is available on site. The most crucial benefit of a cervical collar is that it can immobilize the cervical spine more stably and consistently than MILS.
Many researchers have been tried to demonstrate the feasibility of ETI wearing neck collar using other airway devices, such as supra-glottic airway device, optic device and video laryngoscopy.Recently, many studies have demonstrated that video-assisted laryngoscopy could improve ETI success in difficult airway management. Similarly, we sought to include demonstrations of various airway devices for various neck immobilization scenarios. In the previous manuscript, we did not explain these points well, and we have now added explanations to the revised manuscript. (Page 15, Lines 4-23)
Thank you for your meaningful comments.

[Reply to Review 2’s comments]
Reviewer 2: Dr. Seth Manoach
1. Intubating patients in C-collars was never the standard of care, should never be performed with DL or rigid VL, and is relevant only to exaggerate the effects of manual-in-line stabilization on difficulty in obtaining a direct or rigid videolarygoscopy view of the glottis and passing a tube (see Malik et. al., a similar study to the present one, done in anesthetized humans, for a description of the technique, or the 1984 ATLS manual cited in Manoach et. al.). This must be clearly stated in the manuscript.
>> Reply : As the reviewer mentioned, we agree that MILS is the standard of care for airway management of injured patients. Intubation of patients in cervical collars is not standard and may be an inappropriate practice because it might lead to a higher rate of ETI failure. We clearly stated this point in the discussion section of the revised manuscript.(Page 15, Lines 4-11)

2. MILS was long considered the standard of care, although work by Lennsarson et. al., myself with Paladino, Santoni et. al. and others have shown that MILS as classically described increases pressure transmitted to the cervical spine by the laryngoscope blade (proposed as a mechanism by myself and Paladino to explain the work of Lennarson et. al. showing increased subluxation with MILS -- confirmed by Santoni et. al.)
4. The two above considerations have caused us and others to advocate a slight relaxation of MILS in some situations. We also do so because closed claims analyses have shown that the majority of secondary injury during intubation occurs not with acute trauma but during intubation of elderly patients with degenerative diseases (Hindman et. al.) or in other non-trauma situations. That said, it seems reckless to allow patients’ necks to flop around when they are at risk for secondary neurologic injury, so the authors’ work still matters. The authors themselves are too rigid in their position on an

important issue that deserves a more considered stance.
>> Reply : This issue should be discussed in the article, and we added this issue to the revised manuscript.(Page 13, Lines 16-23)

3. In addition, Santoni et. al. and Thiboutot et. al. have shown that when experienced anesthesiologists are asked to intubate class I and II airways using DL under ideal conditions but with rigidly applied MILS, they fail to do so about 50% of the time.
5. The articles mentioned in #3 above also lead me to question the fidelity of the authors' mannequin model or their MILS technique. Given the failure of experienced anesthesiologists to successfully intubate easy airways when MILS is applied as described, it is difficult to see how relatively inexperienced airway managers (30 intubations is not sufficient to be called experienced with ETI) could successfully intubate 94% of the time with DL. The authors are not alone in reaching a better outcome than would seem possible: in consecutive studies conducted around comparing the performance of emergency medicine and anesthesia trainees in trauma airways, near 99% success was achieved by each group of trainees. Since trauma patients are ideal models for themselves it could also be the case that the mannequin in this study was fine, but the technique in those studies and in this one was not quite as rigid as it is classically intended to be. This matters because I find that the first folks to intubate a patient get rigid MILS, and tend to fail, while the last person, who typically steps up as the patient is desaturating gets none, or almost none, and succeeds. This may cause subtle cognitive harm that needs to be balanced against uncertain benefits and some potential c-spine compromise associated with rigid MILS. The puzzle – why MILS, when applied in controlled situations with experienced anesthesiologists attempting to intubate “easy” patients half the time, while novices with 30 intubations or trainees seem to be able to do so much better – is worth some space in the discussion. Something has to give. The mannequin or the technique. Something always does, I would just like the authors to take their best shot at coming up with an answer, or cede that to an editorialist.
>> Reply : First, we agree with the opinion of the reviewer that experience of 30 intubations is not sufficient to warrant being called “experienced” with ETI. We have therefore revised the manuscript to remove all references to “experienced” in the revised version.The reviewers have questioned the wide gap between the rate of success ETI in previous studies in a clinical setting (failure of ETI in about 50% under rigidly applied MILS found in the study by Thiboutot et al.) and our present study in simulated settings using a manikin (success rate of ETI 94% by DL under MILS).
This disparity of success rate between our study and the study by Thiboutot et al. may be explained by the different design used in the studies. In the study by Thiboutot et al., only 30 s was allowed for the operator to complete ETI successfully, and other applications during ETI were not allowed. By contrast, we allowed multiple attempts for ETI within a maximum 120 s in our study. A longer permitted time and an allowance for multiple attempts may have contributed to the relatively high success rate in our study. In a clinical study by Enomoto et al, researchers set a time limit of 120 s for ETI and allowed multiple attempts, and the success rate of DL for ETI in MILS was 89.4% (93/104). A clinical study by Malik et al. reported a 100% success rate for DL in MILS with indefinite time permitted for ETI. In addition, the simulation environment may be more favourable for high success in ETI than in actual clinical settings. The use of manikins may be a less threatening condition for operators because there is no fear of damage to the body in case of failure. Handling to achieve intubation may be more brutal, and this may lead to the relatively higher success rate. Moreover, manikins have no anatomical variance that can adversely affect the success of ETI. These factors may allow easier ETI in a simulation study than in an actual clinical situation. Other simulation studies have also shown higher success rates than those obtained in clinical studies. We clearly stated this point in the discussion section of the revised manuscript (page 13, Lines 23-24 and Page 14, Lines 1-22)

6. The Pentax scope seems to be a great device in the OR and in mannequins, with but, as the authors point out, traumatized humans bleed a lot and vomit, and have a lot of secretions. Unlike the

glidescope or DL, the pentax optic, along with the ETT passes through the hypopharynx on its way to the glottis. This is where secretions pool, and this leads me to depend on my anterior-optic (ie higher up in spatial, not anatomical terms in the supine patient) devices like the glidescope or mcgrath mac, or even DL, including straight blades. When the patient's mouth is a mess, as it so frequently is in trauma, the more anterior the optic (and so the tube must pass with a separate stylet) the better. Iif the airway is a terrible mess, the eye in the laryngoscopist's head may be better than any eye (optic) stuck in the patient's mouth. So DL still matters. As before this is a mannequin problem, but it's also an OR-as-emergency-airway-model problem.
>> Reply : Thank you for your comment. We add this issue to discussion section in our manuscript.(Page 19, Lines 9-13)

7. The authors do state primary and secondary outcomes, but this is a bit confusing in a study with so many measurements. Strictly speaking one should have a research hypothesis and state what that is. I am not a firm about this, and not sure if it would have helped here. I still understand what the authors are trying to do.
>> Reply : Perhaps the many measurements have confused the reader. The key point is that the success rate is within 120 s. In addition, we measured the time to complete successful ETI. We have revised the manuscript to highlight these parameters. (Page 7, Lines 17-19)

8. The authors should state why they undertook this study when there are similar human ones. I think a reason to do so may lie with the C-collar intubations. They demonstrate that under ideal circumstances, the pentax seems to be the easiest device to line up and make the curve. If there is another important lesson, it might be that novices seem to do better with the pentax and glidescope. Although this is known, commentary about the soiling issue may be expanded on by the authors (or an editorialist).
>> Reply : We appreciate the comments from the reviewers. We have now clarified the reasons that we undertook this study by simulation. (Page 15, Lines 21-24 and Page 16, Lines 1-6)
In addition, we also added another important lesson according to the suggestions of the reviewers.(Page 16, Lines 9-12)

8. I just do not believe the glide scope breaks teeth more than other devices. The clicking of a mannequin's plastic teeth cannot be equated to dental trauma as the tissue/material properties are so different (eg elasticity of material that serves the function of soft tissues). DL would be expected to do so more often. Moreover, relatively inexperienced airway managers (eg those with 30-50 intubations) tend to try to hold the glidescope as a direct laryngoscope is meant to be held. I am not sure if this is because it has caused them so much anxiety to learn to hold a directl largyngoscope di before they try something else, or there is some other reason. In any case, the curve of the GVL causes it to bend around the teeth when lifted straight up, as one is supposed to use it.
>> Reply : We agree with the reviewer's opinion that not all of the clicking sounds from the mannequin's plastic teeth can be equated with dental trauma. This sound may simply indicate that the blade was pressed on the teeth of the manikin and may not directly be defined as tooth injury. As the reviewer mentioned, not all pressure on the teeth may lead to dental injury. In our simulation setting, "pressure to tooth" may be a more suitable term than "dental injury" (although this term seemed to be used in actual clinical settings). We changed the term used in the revised manuscript. However, the Laerdal airway training manikin was designed so that a clicking sound from the teeth was not simply induced by a simple touch and demanded sufficient pressure, and may be equated to dental injury. Therefore, we consider that "pressure to tooth" implies a high risk of dental injury.
Second, considering the high level of difficulty in using DL over video laryngoscopes, the relatively higher incidence of "pressure to tooth" in GVL should be discussed as the reviewer has commented. GVL consists of a blade with a lens and an external display monitor. While intubating patients with a GVL, clinicians seldom check the patient's oral cavity because they should watch the external display monitor to identify the glottic opening. Then, this may easily cause the curved body of the blade to

impinge on the upper teeth when the neck of the patient is immobilized. In the present study, the participants were unfamiliar with GVL and so might have had more difficulty in handling the blade than the DL. In addition, their bold manipulation of the device to achieve quickly successful ETI might also have contributed to increased pressures on the teeth. Otherwise, for DL and PAWS, many participants abandoned advancing the blade to the pharynx when the manikin was fitted with a cervical collar because the oral opening was too narrow, and then the possibility of pressure on the teeth was excluded. Ironically, these devices showed a lower incidence of pressure on the teeth during ETI than GVL, which was attempted frequently and succeeded despite the higher failure rate of repeated attempts.
We added discussion relating with it in the revised manuscript. (Page 17, Lines 4-17)

9. I complement the authors on mixing up the device experience to minimize device specific learning. With so many devices, anatomic learning is probably the most important factor in airway management success and solves the problems of teaching the topic as technology changes.
>> Reply :We enrolled physicians who had some experience of ETI in that they have used DL but not other types of laryngoscopes before the study. They had no prior clinical experience of ETI in patients with immobilized necks. In this study, participants were not expert or experienced but were “less” experienced or novice intubators.
In the present study, to balance the skill levels for each of the airway devices, we held an airway training programme, and all participants were trained in all airway devices (DL, GVL, and PAWS). They achieved similar success in ETI using the three devices before the simulation trial under the various cervical immobilization scenarios.(Page 6, Lines 8-12)

VERSION 2 – REVIEW

REVIEWER	Seth Manoach MD Weill Cornell Medical College United States
REVIEW RETURNED	06-Jul-2016

GENERAL COMMENTS	1. It is not accurate to state as the authors do that the AWS is best because it is fastest in a mannequin MILS scenario. There is a clear trend for the glidescope to perform better than the Pentax as intubation conditions become more difficult with increasing degrees of neck immobilization. This more closely replicates the variation in airway difficulty in humans with differing BMI, thyromental distance, mouth opening, etc. The ability to manipulate both the tube and stylet with the glidescope, as opposed only to the body of the AWS probably accounts for that. Faster intubation time with the AWS under the most permissive conditions can not justify it's recommendation for real-life MILS when patients vary so much more than a mannequin and in MILS are more likely to approximate the restrictive collar conditions when difficult or even slightly difficult airways must be managed with MILS. Since one should not intubate in a collar I assumed the collars were used merely to reproduce more difficult airways in MILS using a standard mannequi. 2. I believe I already commented that the AWS optic must travel with the endotracheal tube through pooled secretions in the hypopharynx, while the glidescope optic is anterior to, or in the supine position"above" and less likely to be obstructed by blood vomitus or secretions there. 3. It is simply incorrect to state that the glidescope intubator/laryngosopist only looks at the screen. He or she should

	follow the stylet with the eye until it can be seen on the screen. Not doing so can cause pharyngeal perforations. 4. The teeth (that is a change in language that must be made, pressure is applied to more than one tooth) in a mannequin are not at all like human teeth, and when held correctly, the GVL blade is curved so that it does not impact the teeth. Novice GVL users tend to deploy the blade like a direct laryngoscope and tilt it iincorrectly. 5. For the record I get no money, not even free samples from the makers of the GVL. I typically use straight DL blades or the McGrath Mac. I occasionally use the GVL. 6. I am writing this on a small screen. Forgive any errors.

VERSION 2 – AUTHOR RESPONSE

1. It is not accurate to state as the authors do that the AWS is best because it is fastest in a mannequin MILS scenario. There is a clear trend for the glidescope to perform better than the Pentax as intubation conditions become more difficult with increasing degrees of neck immobilization. This more closely replicates the variation in airway difficulty in humans with differing BMI, thyromental distance, mouth opening, etc. The ability to manipulate both the tube and stylet with the glidescope, as opposed only to the body of the AWS probably accounts for that. Faster intubation time with the AWS under the most permissive conditions can not justify it's recommendation for real-life MILS when patients vary so much more than a mannequin and in MILS are more likely to approximate the restrictive collar conditions when difficult or even slightly difficult airways must be managed with MILS. Since one should not intubate in a collar I assumed the collars were used merely to reproduce more difficult airways in MILS using a standard mannequin.
Reply : It is not easy to determine which device is the best option for difficult airway management. PAWS is equipped with a side channel through which the endotracheal tube is already placed before the intubation. Once the device is inserted, the glottis is visualized and centred on a target mark on a video display screen; therefore, the endotracheal tube is passed through the glottis immediately. The Pentax device quickly facilitates the passage of the endotracheal tube into the glottis after its visualization. This design feature can contribute to faster intubation than is possible with other devices.
However, as the reviewer mentioned, conclusive support of the Pentax device as the best option because this device allowed faster intubation may be inappropriate. Our study result is not easily applicable to actual situations because it was performed in a simulated setting using a single manikin. Variations in patient anatomy, level of neck immobilization and other situational factors (such as oral contamination) were not reflected, so further clinical evidence should be gathered to determine the best tool for difficult airway management. We have rewritten the conclusion of the study in the revised manuscript.

2. I believe I already commented that the AWS optic must travel with the endotracheal tube through pooled secretions in the hypopharynx, while the glidescope optic is anterior to, or in the supine position "above" and less likely to be obstructed by blood vomitus or secretions there.
Reply: A weak point of video laryngoscopy is that the camera may be obscured by oral contamination. As the reviewer mentioned, the Pentax device may be more vulnerable to blocking of the camera lens than the GlideScope, because the camera lens of the Pentax device is located deeper and lower in the oral cavity area during intubation, compared with the higher and shallower position of the camera lens of the GlideScope device.

3. It is simply incorrect to state that the glidescope intubator/laryngosopist only looks at the screen. He or she should follow the stylet with the eye until it can be seen on the screen. Not doing so can cause

pharyngeal perforations.
Reply: We agree reviewer opinion, then omitted the sentence " ~ clinicans seldom check the patient's oral cavity ~". Thank you for your kind comment.

4. The teeth (that is a change in language that must be made, pressure is applied to more than one tooth) in a mannequin are not at all like human teeth, and when held correctly, the GVL blade is curved so that it does not impact the teeth. Novice GVL users tend to deploy the blade like a direct laryngoscope and tilt it incorrectly.
Reply : As the reviewer mentioned, a better explanation of the frequent pressure from teeth seems to be that it is a result of a common mistake by novice GVL users, in which they tend to use the blade as a direct laryngoscope and tilt it incorrectly. We revised the manuscript accordingly. We changed "tooth" to "teeth"also.

5. For the record I get no money, not even free samples from the makers of the GVL. I typically use straight DL blades or the McGrath Mac. I occasionally use the GVL.
Reply : We have used the DL, and have attempted to use various types of video laryngoscopes (GVL, Pentax and, Disposcope et al). It is not easy to determine which video laryngoscope is most suitable for us. Based on our clinical experience of the use of various video laryngoscopes, our subjective opinion is that the GVL seems to be the most convenient to use.
A well-designed clinical study with a larger sample will be required to answer this question. We appreciate the reviewer's keen interest in our study, and the various airway devices.