PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Trauma airway management in emergency departments: A multicenter, prospective, observational study in Japan
AUTHORS	Nakao, Shunichiro; Kimura, Akio; Hagiwara, Yusuke; Hasegawa, Kohei

VERSION 1 - REVIEW

REVIEWER	Yuko Ono Emergency and Critical Care Medical Center, Fukushima Medical University Hospital, 1 Hikarigaoka, Fukushima 960-1295, Japan
REVIEW RETURNED	02-Oct-2014

GENERAL COMMENTS	General comments I have reviewed your manuscript entitled “Trauma airway management in emergency departments: A multicenter, prospective, observational study in Japan”, which reports that airway management varies among Japanese emergency departments (EDs). I think the topic of your study is interesting, and approve of your tremendous efforts. However, your manuscript has a number of problems that should be resolved before it is fit for publication. Major comments 1. Patient characteristics Essential data describing the characteristics of the trauma patients are missing, such as the injury severity score (ISS) [1, 2], revised trauma score (RTS) [3, 4], and type of injury (blunt or penetrating trauma). These data are fundamental parameters for the assessment of trauma, and without them, the reader cannot accurately picture the clinical presentations of the patients. 2. No consideration of confounding factors The authors describe variation in the method of intubation as the main outcome measure of this study. However, confounding factors are not considered. Many factors can affect the method of endotracheal intubation used in trauma patients, such as severe facial injury (abbreviated injury scale [AIS] face ≥3) [5, 6], shock, and obesity [7]. Rapid sequence intubation (RSI) techniques can induce airway collapse in an injured patient with a fragile airway, or can induce cardiopulmonary arrest in an injured patient with profound shock. Physicians may be hesitant to administer neuromuscular blocking agents or sedative drugs in these difficult cases. The analyses do not adjust for confounding factors that may affect the method or success of intubation in trauma patients, even though it is mentioned that non-RSI methods may have been attempted in patients with potentially difficult airways (discussion, lines 27–30). 3. No information about the EDs The authors do not provide information about the EDs included in the study. Are these EDs level 1 trauma centers, or level 2 or level 3 centers? How many trauma patients are admitted to the EDs? These

	factors can greatly affect the quality of trauma care [8, 9], and may be confounding factors in the analyses. 4. Inappropriate case mix Why are cases of traumatic cardiac arrest included in the analyses? Endotracheal intubation techniques for cardiac arrest patients differ from endotracheal intubation techniques for trauma patients. Many previous studies therefore excluded traumatic cardiac arrest cases from such analyses. 5. Concern about splitting of data I have read the cited a reference published by the same group (Reference No 10: Hasegawa K et al. Resuscitation 2012, 83:428–433), which investigated current endotracheal intubation techniques used in Japanese EDs, including both medical and trauma patients recorded in the same database as used in the current study. That study concluded that there was a high degree of variation among hospitals in emergency airway management techniques and success rates, which is the same conclusion as in the current study. Unless the current study can provide novel information regarding trauma airway management in this patient group, many readers including myself will regard this study as splitting of previously published data. Minor comments Table 3 1. Esophageal intubation† †Pulse oximetry less than 90% during intubation attempt, not a result of esophageal intubation. This footnote is not suitable for esophageal intubation. 2. Hypoxia The definition of hypoxia is required. In conclusion, I have concerns regarding the lack of essential data describing the characteristics of the trauma patients, the lack of adjustment for confounding factors in the analyses, and the splitting of previously published data. I therefore have to recommend “rejection” rather than “major revision” of this manuscript. I hope that my comments do not discourage you from conducting further studies. Presentation of data describing large patient groups is obviously informative, and the topic is interesting. The problems lie in the study design. References 1. Baker SP, O’Neill B, Haddon W Jr, Long WB: The injury severity score: a method for describing patients with multiple injuries and evaluating emergency care. J Trauma 1974, 14:187–196. 2. Baker SP, O’Neill B. The injury severity score: an update. J Trauma 1976, 16:882–885. 3. Champion HR, Sacco WJ, Carnazzo AJ, Copes W, Fouty WJ: Trauma score. Crit Care Med 1981, 9:672–676. 4. Champion HR, Sacco WJ, Copes WS, Gann DS, Gennarelli TA, Flanagan ME: A revision of the Trauma Score. J Trauma 1989, 29:623–629. 5. Cogbill TH, Cothren CC, Ahearn MK, Cullinane DC, Kaups KL, Scalea TM, Maggio L, Brasel KJ, Harrison PB, Patel NY, Moore EE, Jurkovich GJ, Ross SE. Management of maxillofacial injuries with severe oronasal hemorrhage: a multicenter perspective. J Trauma. 2008, 65:994-9. 6. Ono Y, Yokoyama H, Matsumoto A, Kumada Y, Shinohara K, Tase C. Surgical airways for trauma patients in an emergency

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REVIEWER	Colin A Graham Accident and Emergency Medicine Academic Unit Chinese University of Hong Kong Prince of Wales Hospital Hong Kong SAR
REVIEW RETURNED	12-Oct-2014

GENERAL COMMENTS	Interesting paper with surprising results. Worthy of publication but could benefit from expansion of the discussion as indicated in comments to authors. Interesting paper with rather surprising results. It is useful to see the proportion of ED staff who performed intubations, but do you have any data on which other specialists were involved in intubating the non-ED doctor intubated patients? It might be interesting to see what the success rates of ED v non-ED doctors was, and possibly the success rates of specialists v non-specialist doctors. The term 'transitional doctors' needs to be clarified as I am not sure what this means. Are there requirements for procedural credentially (individual or institutional) in Japan? If not, should there be in light of the findings of your study? Some of your results are very concerning given the low success rates in some centres. Finally, could you expand more on the low rate of rapid sequence induction/intubation? This has clearly been described as the technique of choice for emergency intubation in the ED and it seems incredible that this has not been actioned in so many EDs. Another reference that may be useful is Graham CA, et al. Rapid sequence intubation in Scottish urban emergency departments. Emerg Med J. 2003 Jan;20(1):3-5.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name Yuko Ono

"I think the topic of your study is interesting, and approve of your tremendous efforts."

We thank the reviewer for these positive comments.

"1. Patient characteristics
Essential data describing the characteristics of the trauma patients are missing, such as the injury severity score (ISS) [1, 2], revised trauma score (RTS) [3, 4], and type of injury (blunt or penetrating trauma). These data are fundamental parameters for the assessment of trauma, and without them, the reader cannot accurately picture the clinical presentations of the patients."

We thank the reviewer for these thoughtful insights. We completely agree that these variables (ISS, RTS, blunt vs. penetrating injury) would be informative if we are interested in morbidity and mortality in trauma patients. However, we do have the information on the indication of intubation (e.g., head trauma, facial/neck trauma, airway burn in Table 1). We believe that these might be more relevant to the performance of ED airway management (e.g., success rate). Nevertheless, we have acknowledged the lack of this information (e.g., ISS, RTS) in the Limitations section (page 16, para 1).

"2. No consideration of confounding factors
The authors describe variation in the method of intubation as the main outcome measure of this study. However, confounding factors are not considered. Many factors can affect the method of endotracheal intubation used in trauma patients, such as severe facial injury (abbreviated injury scale [AIS] face ≥3) [5, 6], shock, and obesity [7]. Rapid sequence intubation (RSI) techniques can induce airway collapse in an injured patient with a fragile airway, or can induce cardiopulmonary arrest in an injured patient with profound shock. Physicians may be hesitant to administer neuromuscular blocking agents or sedative drugs in these difficult cases. The analyses do not adjust for confounding factors that may affect the method or success of intubation in trauma patients, even though it is mentioned that non-RSI methods may have been attempted in patients with potentially difficult airways (discussion, lines 27–30)."

We agree that the observed inter-hospital variations in the methods of intubation and success rates were confounded by several variables. However, it is unlikely that these factors fully explain the observed large difference in the success rates at the first attempt (from 35.5% to 90.5%). Furthermore, please note that our study objective is to describe the current practice of airway management for trauma patients in Japan. This prospective multicenter data reflect the current airway management in the natural setting of a “real” population and current clinical practice, therefore enhancing the potential generalizability of the findings. We believe that these data represent the best available data. Nevertheless, as the reviewer implied, our observations should facilitate further research to identify the reasons of the inter-hospital variations. In the Discussion (page 16, para 1) and Conclusion section (page 16, para 2), we have acknowledged this limitation and expand the discussion.

"3. No information about the EDs
The authors do not provide information about the EDs included in the study. Are these EDs level 1 trauma centers, or level 2 or level 3 centers? How many trauma patients are admitted to the EDs? These factors can greatly affect the quality of trauma care [8, 9], and may be confounding factors in the analyses."

We appreciate the opportunity to clarify this point. Although the designation of hospitals for trauma care used in the US (e.g., the level I, II, III, IV, and V trauma centers) is not used in Japan, among the 13-participating institutions, 10 institutions are tertiary medical centers that have a capability to manage the most severe trauma patients, and 3 institutions are secondary medical centers that are designated to treat moderately severe trauma patients. As requested, we have added this information in the Methods section (page 7, para 1).

"4. Inappropriate case mix
Why are cases of traumatic cardiac arrest included in the analyses?
Endotracheal intubation techniques for cardiac arrest patients differ from endotracheal intubation techniques for trauma patients. Many previous studies therefore excluded traumatic cardiac arrest cases from such analyses."

Because patients with traumatic cardiac arrest consisted of approximately one-third of the eligible patients, we believe investigation of this population is also important. Similarly, there are several previous studies on airway management that also included patients with cardiac arrest [1-3].
That said, we agree with the reviewer that the patient population and required intubation techniques might be different between patients with cardiac arrest and those without. Therefore, we have performed stratified analysis by the indication of intubation (cardiac arrest vs. non-cardiac arrest) (Statistical Analysis section, page 9, para 2; Tables 2 and 3), and we found similar inter-hospital variations in the intubation success rate at the first attempt and adverse event rate across the strata (Figure 3). We believe this stratified analysis will sufficiently address the reviewer's concern.

"5. Concern about splitting of data
I have read the cited a reference published by the same group (Reference No 10: Hasegawa K et al. Resuscitation 2012, 83:428–433), which investigated current endotracheal intubation techniques used in Japanese EDs, including both medical and trauma patients recorded in the same database as used in the current study. That study concluded that there was a high degree of variation among hospitals in emergency airway management techniques and success rates, which is the same conclusion as in the current study. Unless the current study can provide novel information regarding trauma airway management in this patient group, many readers including myself will regard this study as splitting of previously published data."

We appreciate the opportunity to clarify this point. In the interim analysis of the JEAN registry (including the both medical and trauma patients) published in 2012 [4], the sample size of trauma patients (n=278) was too small to specifically investigate or report the inter-hospital variation in trauma victims. As successful airway management is a cornerstone in the modern practice of emergency and trauma care, in the current analysis we aimed to specifically investigate the trauma airway management by using the complete data set with a much larger sample size (n=723). Therefore, approximately two-thirds (63%) of the patients are NOT overlapped between the two papers. Moreover, this is the first study to report a wide inter-hospital variation in the trauma airway management and its performance.

"Minor comments
Table 3
1. Esophageal intubation†
†Pulse oximetry less than 90% during intubation attempt, not a result of esophageal intubation.
This footnote is not suitable for esophageal intubation.

2. Hypoxia
The definition of hypoxia is required."

We have amended the information in Table 3, as recommended by the reviewer.

Reviewer: 2
Reviewer Name Colin A Graham
"Interesting paper with rather surprising results."

We thank the reviewer for this positive comment.

"It is useful to see the proportion of ED staff who performed intubations, but do you have any data on which other specialists were involved in intubating the non-ED doctor intubated patients? It might be interesting to see what the success rates of ED v non-ED doctors was, and possibly the success rates of specialists v non-specialist doctors."

We appreciate the reviewer's excellent suggestion. Although we collected the information on intubators' level of training (post-graduate years) and specialty (emergency physicians vs. non-emergency physicians), we did not collect the information on specific specialties in the non-emergency physicians (e.g., anesthesiologists). Nevertheless, we have compared the intubation success rates between emergency physicians (n=434) vs. non-emergency physicians (n=289). We found that emergency physicians had a higher success at the first attempt (72.8 % vs. 50.2 %, $p<0.001$) compared to non-emergency physicians. In contrast, there was no difference in the success within three attempts (95.4 % vs. 96.9 %; P=0.36). As requested, we have added this information in the Statistical Analysis section (page 9, para 2), and highlighted the results in the Results section (page 12, para 1).

"The term 'transitional doctors' needs to be clarified as I am not sure what this means."

We thank the opportunity to clarify this point. In Japan, after obtaining their medical license, most physicians undergo 2-year mandatory training, which consists of rotations in different specialties (e.g. internal medicine, general surgery, pediatrics, emergency medicine). Based on the literature[5], these post-graduate year 1-2 physicians are defined as transitional-year residents. As requested, we have highlighted this definition in the Methods section (page 8, para 2).

"Are there requirements for procedural credentially (individual or institutional) in Japan? If not, should there be in light of the findings of your study? Some of your results are very concerning given the low success rates in some centers."

As pointed out, there are no requirements for procedural credentials to perform ED intubations both in individuals and institutions in Japan [4], although teaching hospitals have the responsibility to teach appropriate procedures. This would have contributed, at least partially, to the observed inter-hospital variations in the success rates, As suggested, we have highlighted this issue in the Discussion section (page 15, para 1).

"Finally, could you expand more on the low rate of rapid sequence induction/intubation? This has

clearly been described as the technique of choice for emergency intubation in the ED and it seems incredible that this has not been actioned in so many EDs. Another reference that may be useful is Graham CA, et al. Rapid sequence intubation in Scottish urban emergency departments. Emerg Med J. 2003 Jan;20(1):3-5."

We thank the reviewer for this suggestion. The reasons for the low rate of RSI use in some EDs are unclear and likely multifactorial. It is possible that, in these EDs, non-RSI methods were more frequently attempted in patients who were predicted to have a difficult intubation. However, the difference in the patient population across the EDs cannot fully explain the observed three-fold difference in the use of RSI. Alternatively, it is also possible that non-RSI methods were more frequently used in certain EDs because of the physicians' preference, procedural experiences, training background, availability of medications, and institutional policies. In addition, it is possible that the scarcity of evidence – evidences for accurately predicting patients in whom RSI should be avoided – may have contributed to the practice variations across the EDs. Of note, generally, emergency physicians in Japan are not prohibited from performing RSI in the ED. As suggested, we have discussed issue in the Discussion section (page 15, para 1 and 2). Additionally, we thank the reviewer for the helpful reference [6]. We have cited this interesting article in our manuscript.

REFERENCES

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VERSION 2 – REVIEW

REVIEWER	Colin A Graham Accident & Emergency Medicine Academic Unit Chinese University of Hong Kong Prince of Wales Hospital Hong Kong SAR
REVIEW RETURNED	22-Nov-2014

GENERAL COMMENTS	Thank you for your responses which address my concerns. My only remaining comment relates to your statement 'It is possible that, in these EDs, non-RSI methods were more frequently attempted in patients who were predicted to have a difficult intubation.'. While I agree with you, it may be worth pointing out in your paper

	that these 'difficult' patients are better served by having an RSI rather than any other technique because of its very high success rates compared to other techniques, and the misconception that RSI should be avoided in patients with 'difficult airways' needs to be addressed through good education of ED and ICU physicians. Thank you for your useful addition to the literature on this topic.

REVIEWER	Yuko Ono Emergency and Critical Care Medical Center, Fukushima Medical University Hospital, 1 Hikarigaoka, Fukushima 960-1295, Japan
REVIEW RETURNED	24-Nov-2014

GENERAL COMMENTS	I appreciate your perseverance and your responses to my comments. The current revision clarifies two important aspects of your study: (1) reporting of the large variation in the current airway management of trauma patients in Japan, and (2) facilitation of further research to identify the reasons for this variation. I understand that the objective of this study was to describe current airway management in trauma patients in Japan (page 16, lines 2–4), and that the study did not aim to examine the reasons for variations in management. I can see the scientific value of your study. However, to make it more attractive to readers, please consider followings points. Major comments 1) Use of airway equipment such as a video laryngoscope Most trauma patients who required endotracheal intubation in this study had potentially difficult airways (e.g., head trauma 30.4%, face/neck trauma 8.4%, and airway burns 6.8%). Such patients may have distorted upper airway anatomy, substantial bleeding, and a need for in-line neck immobilization, all of which can increase the difficulty of endotracheal intubation. Past studies [1–4] reported that use of airway equipment such as a video laryngoscope resulted in improved success rates for endotracheal intubation compared with classical direct laryngoscopy in patients with difficult airways. Use of a video laryngoscope or other airway adjunct during the first intubation attempt can therefore affect outcome measures such as the success rate of the first intubation attempt and the success rate within three intubation attempts. It would be useful to include information regarding the number of trauma patients who were intubated using a video laryngoscope or other airway adjunct on the first attempt, and the number who were intubated using direct laryngoscopy. 2) Difference in the rate of successful endotracheal intubation between emergency and non-emergency physicians Your revised manuscript includes an interesting new result in response to a previous comment by reviewer #2 [pages 11–12: emergency physicians had a higher success rate on the first attempt than non-emergency physicians (72.8% vs 50.2%, $p<0.001$)]. This is a very important point. What are your opinions regarding this difference? Further discussion of this finding should be included in

	the manuscript. For example, the success rate of attempted endotracheal intubation performed by non-emergency physicians was unacceptably low (almost half of the attempts were unsuccessful). The consequences of a failed intubation attempt in a patient with a difficult airway can be catastrophic, especially in a patient with low physiological reserve such as a trauma patient. Therefore, airway management of trauma patients should be performed by skilled operators such as emergency physicians. 3) Definitions of hypotension and hypoxia Your manuscript indicates that 16.6% of all trauma patients underwent endotracheal intubation because of shock (Table 1). In Table 3, hypotension (as an endotracheal intubation-associated adverse event) is defined as a systolic blood pressure of <90 mmHg. How is this adverse event defined in patients with preexisting shock? The same applies for hypoxia. Many trauma patients, especially those requiring airway management, have a low physiological reserve, even before endotracheal intubation is attempted. As the rate of adverse events is one of the main outcome measures, this needs to be clarified. For example, Mort TC et al defined "hypotension" (as an adverse event) as a systolic blood pressure of <90 mmHg if this is >20% lower than the baseline blood pressure in critically ill patients [5]. Thank you for the opportunity to review your interesting manuscript. References 1. Sakles JC, Mosier J, Chiu S, Cosentino M, Kalin L. A comparison of the C-MAC video laryngoscope to the Macintosh direct laryngoscope for intubation in the emergency department. Ann Emerg Med. 2012 ;60:739-48. 2. Aziz MF, Dillman D, Fu R, Brambrink AM: Comparative effectiveness of the C-MAC video laryngoscope versus directlaryngoscopy in the setting of the predicted difficult airway.Anesthesiology 2012; 116:629–36 3. Enomoto Y, Asai T, Arai T, Kamishima K, Okuda Y: Pentax-AWS, a new videolaryngoscope, is more effective than the Macintosh laryngoscope for tracheal intubation in patients with restricted neck movements: A randomized comparative study. Br J Anaesth 2008; 100:544–8 4. Jungbauer A, Schumann M, Brunkhorst V, Börgers A, Groeben H: Expected difficult tracheal intubation: A prospective comparison of direct laryngoscopy and video laryngoscopy in 200 patients. Br J Anaesth 2009; 102:546–50 5. Mort TC. Emergency tracheal intubation: complications associated with repeated laryngoscopic attempts. Anesth Analg. 2004;99:607-13

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name Yuko Ono
I appreciate your perseverance and your responses to my comments.
The current revision clarifies two important aspects of your study: (1) reporting of the large variation in the current airway management of trauma patients in Japan, and (2) facilitation of further research to identify the reasons for this variation.
I understand that the objective of this study was to describe current airway management in trauma patients in Japan (page 16, lines 2–4), and that the study did not aim to examine the reasons for variations in management.
I can see the scientific value of your study.

We thank the reviewer for these positive comments.

Major comments
1) Use of airway equipment such as a video laryngoscope
Most trauma patients who required endotracheal intubation in this study had potentially difficult airways (e.g., head trauma 30.4%, face/neck trauma 8.4%, and airway burns 6.8%). Such patients may have distorted upper airway anatomy, substantial bleeding, and a need for in-line neck immobilization, all of which can increase the difficulty of endotracheal intubation. Past studies [1–4] reported that use of airway equipment such as a video laryngoscope resulted in improved success rates for endotracheal intubation compared with classical direct laryngoscopy in patients with difficult airways. Use of a video laryngoscope or other airway adjunct during the first intubation attempt can therefore affect outcome measures such as the success rate of the first intubation attempt and the success rate within three intubation attempts. It would be useful to include information regarding the number of trauma patients who were intubated using a video laryngoscope or other airway adjunct on the first attempt, and the number who were intubated using direct laryngoscopy.

We thank the reviewer for this excellent suggestion! As requested, we have added the information of the number of trauma patients who were intubated on the first attempt using a video laryngoscope (n=30, 4.1%), a bronchoscope (n=17, 2.4%), a lighted stylet (n=1, 0.1%), and the number who were intubated using direct laryngoscopy (n=654, 90.5%) to the Result section (page 10, para 2; page 11, para 1).

2) Difference in the rate of successful endotracheal intubation between emergency and non-emergency physicians
Your revised manuscript includes an interesting new result in response to a previous comment by reviewer #2 [pages 11–12: emergency physicians had a higher success rate on the first attempt than non-emergency physicians (72.8% vs 50.2%, $p<0.001$)]. This is a very important point. What are your opinions regarding this difference? Further discussion of this finding should be included in the manuscript.
For example, the success rate of attempted endotracheal intubation performed by non-emergency physicians was unacceptably low (almost half of the attempts were unsuccessful). The consequences of a failed intubation attempt in a patient with a difficult airway can be catastrophic, especially in a patient with low physiological reserve such as a trauma patient. Therefore, airway management of trauma patients should be performed by skilled operators such as emergency physicians.

We appreciate the reviewer's excellent suggestion. The majority of non-emergency physicians were transitional-year residents (post-graduate years 1 and 2 physicians) (n=237/288), likely contributing to the lower intubation success rate. We have clarified this point in the Result section (page 12, para 1). In addition, we completely agree with the reviewer that the first-pass success is important in critically-ill trauma patients; therefore, the lower success rate by these non-skilled physicians cannot be

justified. To achieve the better patient outcome, our data underscore the importance of optimal training opportunities for non-skilled physicians, such as the use of simulators and training in a more controlled setting. As requested, we have highlighted this point in the Discussion section (page 15, para 1).

3) Definitions of hypotension and hypoxia
Your manuscript indicates that 16.6% of all trauma patients underwent endotracheal intubation because of shock (Table 1). In Table 3, hypotension (as an endotracheal intubation-associated adverse event) is defined as a systolic blood pressure of <90 mmHg. How is this adverse event defined in patients with preexisting shock? The same applies for hypoxia. Many trauma patients, especially those requiring airway management, have a low physiological reserve, even before endotracheal intubation is attempted. As the rate of adverse events is one of the main outcome measures, this needs to be clarified. For example, Mort TC et al defined “hypotension” (as an adverse event) as a systolic blood pressure of <90 mmHg if this is >20% lower than the baseline blood pressure in critically ill patients [5].

We thank the opportunity to clarify this important point. As the reviewer noted, an adverse events in the study are needed to be intubation-related.[1] Therefore, preexisting shock or hypoxemia before an intubation attempt were not counted as an adverse event. And, for example, hypotension was defined as systolic blood pressure less than 90 mmHg. As requested, we have clarified these definitions in the Method section (page 8, para 2).

Reviewer: 2
Reviewer Name Colin A Graham
Thank you for your responses which address my concerns.

We thank the reviewer for this positive comment.

My only remaining comment relates to your statement 'It is possible that, in these EDs, non-RSI methods were more frequently attempted in patients who were predicted to have a difficult intubation.'.
While I agree with you, it may be worth pointing out in your paper that these 'difficult' patients are better served by having an RSI rather than any other technique because of its very high success rates compared to other techniques, and the misconception that RSI should be avoided in patients with 'difficult airways' needs to be addressed through good education of ED and ICU physicians.

We thank the reviewer for this excellent suggestion. We agree with the reviewer that potential differences in the patient population across the EDs cannot fully explain the observed three-fold difference in the use of RSI. Therefore, it is possible that some of the patients might have been inappropriately considered as “difficult intubation,” and intubated with non-RSI methods. We also note that international and Japanese trauma care guidelines recommend the use of RSI as the initial method of emergency airway management in most trauma patients,[2-4] the evidence for accurately predicting patients in whom RSI should be avoided remain limited.[5 6] It is, therefore, plausible that the scarcity of evidence may have contributed to the practice variations across the EDs. Taken together, our data suggest that building more robust evidence on trauma airway management, coupled with improved dissemination of these findings, could decrease the variations in trauma care across the EDs in Japan. As suggested, we have highlighted these discussions in the Discussion section (page 14, para 2; page 15, para 2).

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observational study. Annals of emergency medicine 2012;60:749-54 e2 doi: 10.1016/j.annemergmed.2012.04.005.
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VERSION 3 – REVIEW

REVIEWER	Colin A Graham Chinese University of Hong Kong, Hong Kong SAR
REVIEW RETURNED	06-Dec-2014

GENERAL COMMENTS	Thank you for your revisions.

REVIEWER	Yuko Ono Emergency and Critical Care Medical Center, Fukushima Medical University Hospital, Fukushima, Japan
REVIEW RETURNED	08-Dec-2014

GENERAL COMMENTS	Thank you for your effort. I am sure your manuscript is now informative and interesting. I am just wondering if you furnish the trauma volume of each participating Emergency Departments (EDs). For example (not author's data), the participating EDs had an average trauma admission 1,000 per year (range 300 to 3,000). As I have previously indicated, trauma volume can greatly affect the quality of trauma care [1]. If you are able to provide this information, the reader will be able to picture the clinical settings more accurately. Reference 1. Minei JP, Fabian TC, Guffey DM, Newgard CD, Bulger EM, Brasel KJ, Sperry JL, MacDonald RD. Increased trauma center volume is associated with improved survival after severe injury: results of a Resuscitation Outcomes Consortium study. Ann Surg. 2014, 260:456-64.

VERSION 3 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name Yuko Ono
Thank you for your effort. I am sure your manuscript is now informative and interesting.

We thank the reviewer for these positive comments.

I am just wondering if you furnish the trauma volume of each participating Emergency Departments (EDs).
For example (not author's data), the participating EDs had an average trauma admission 1,000 per year (range 300 to 3,000).
As I have previously indicated, trauma volume can greatly affect the quality of trauma care [1]. If you are able to provide this information, the reader will be able to picture the clinical settings more accurately.

We appreciate the reviewer's helpful suggestion. As suggested, to the Methods section (page 7, para 1), we have added the information about the number of trauma hospitalizations: The participating institutions had a median of 1,000 trauma hospitalizations per year (range, 300–1,500).[1] Although we were aware that the DPC data may not include all of trauma hospitalizations, we used this approach to standardize the method to identify trauma admission across the participating EDs.

REFERENCE
1. Hospital Intelligence Agency. http://hospia.jp/ (accessed 8 Dec 2014).

Reviewer: 2
Reviewer Name Colin A Graham
Thank you for your revisions.

We thank the reviewer for reviewing our manuscript!