Did Video Kill the Direct Laryngoscopy Star? Not Yet!

To the Editor:

We read with interest "Difficult Airway Characteristics Associated with First Attempt Failure at Intubation Using Video Laryngoscopy in the Intensive Care Unit" (1). In that article, Joshi and colleagues assessed determinants of unsuccessful efforts by physicians in training to perform orotracheal intubation, using predominantly combined video and direct laryngoscopy devices (C-MAC; Karl Storz, Tuttlingen, Germany). After each intubation, the operators completed a data collection form, allowing the authors to analyze factors associated with failure of first-pass placement of an endotracheal tube. They identified several factors that contributed to first-pass failure; notably, blood in the airway, cervical immobility, airway edema, and obesity.

The study by Joshi and coauthors adds to the existing literature in several ways, including the use of a nonanesthesia house staff intubating cohort, data on video-assisted intubations, trainee performance using combined video and direct laryngoscopy equipment, and the pinpointing of possible impediments to successful intubation. However, there are several items that require further clarification.

First, the standard approach employed in this study when trainees used a device with both direct and indirect capabilities, such as the Storz C-MAC or GlideScope Direct Intubation Trainer (Verathon, Bothell, WA), is of clinical consequence. If glottis visualization was obscured, did the residents and fellows perform primarily direct laryngoscopy with a video "rescue," or did they use primarily an indirect approach with a direct laryngoscopy rescue? The reason why blood in the airway caused failure is also of interest. Were these failures primarily a result of inability to visualize the larynx with the video device, or primarily a failure of an effort that combined direct and indirect laryngoscopy? In addition, it would be helpful to disclose details regarding bougie/introducer devices or laryngeal manipulation techniques in patients for whom a view was not possible using a video approach (assuming a nonhyperangulated blade was used).

Second, the influence of the supervising physician is relevant, regarding the success of the procedures. Two recent investigations comparing success rates of direct-to-video laryngoscopy attempted by pulmonary and critical care fellows included immediate attending feedback and coaching (verbal communication) (2, 3). Information on the supervisory role of attending physicians is not included in the current report.

Third, in their investigation of factors associated with failure to achieve first-pass successful intubation, Joshi and associates found that limited mouth opening was highly prevalent in both groups (24/166 of first-attempt failures, 64/740 in first-attempt success). However, an operational definition of limited mouth opening is not clearly stated in the report. In a multivariate risk index study of preoperative endotracheal intubation attempts by experienced anesthesiologists, mouth opening, defined as an interincisor tooth gap of <4 cm, was found to have a positive predictive value for difficult intubation of 25% (4). In another study of difficult routine preoperative intubations, measured mouth opening (interincisor distance) was also strongly associated with easy vs. difficult intubation (5). Therefore, it would be of interest to know how limited mouth opening was defined and measured in the Joshi study.

Finally, knowing the urgency of the endotracheal attempts is necessary to place the results of this investigation in an appropriate clinical context (1).

Author disclosures are available with the text of this letter at www.atsjournals.org.

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Reply

From the Authors:

We thank Drs. Mendelson, Felner, and Kaufman for their kind words and interest in our study "Difficult Airway Characteristics Associated with First Attempt Failure at Intubation Using Video Laryngoscopy in the Intensive Care Unit" (1). As evidence suggests that first-attempt success reduces the risk of complications (2, 3), efforts to identify barriers to first-attempt success are needed. The authors raise a number of interesting questions; unfortunately, not all of them can be answered from our data set.

Mendelson and colleagues point out that some devices, such as the Storz C-MAC, are capable of both direct and video laryngoscopy. This dual function, although clinically useful, complicates the study of these devices. Because the supervising physician can verbally direct the operator using information from the video screen, we believe that direct laryngoscopy using a video laryngoscope cannot be classified as the same as traditional

direct laryngoscopy. For these reasons, we only collect data on the device used and not the fashion in which the operator used it.

Regarding external laryngeal manipulation and bougie use, we do teach external laryngeal manipulation use as standard practice to obtain optimal views of the airway in our airway curriculum. However, we do not track its use in our continuous quality improvement database. The use of the bougie was relatively infrequent in our cohort, with only 22 total attempts using a bougie, of which 16 were cases where the bougie was used for the first attempt.

The issue of supervision is an interesting one; however, we do not have a standardized protocol governing that feedback other than that an attending physician be physically present during all airway management procedures. Experience suggests that often feedback is given in real time during the intubation attempt as well as in debriefing after it; however, we do not collect data on the timing or occurrence of feedback.

Noting that limited mouth opening was prevalent in our cohort, and its prior association with difficult intubation, Mendelson and colleagues inquire about how we defined limited mouth opening. We use a group of difficult airway characteristics that can easily be evaluated at the bedside, even during emergent circumstances. To that end, simplicity is paramount; we only specify quantitative criteria for hypotension and hypoxia, and the remainder of the difficult airway characteristics are qualitatively assessed by the operator as either present or not. In the study by el-Ganzouri and colleagues referenced by the authors, the positive predictive value of an interincisor distance of less than 4 cm to predict difficult intubation was only 25% (4). In the study by Karkouti and colleagues, limited mouth opening was associated with increased odds of a difficult intubation, but prediction of the difficult airway could only be interpreted in the context of chin protrusion and neck extension measurements and only for direct laryngoscopy (5). Our study did not evaluate difficult intubation or direct laryngoscopy but rather evaluated anatomic characteristics associated with first-attempt failure using video laryngoscopy.

We agree that the urgency of intubation may create a quite variable clinical context, and this is likely a contributing factor in the increased risk of complications, as patients requiring urgent or emergent intubation in the intensive care unit are more likely to have cardiopulmonary derangements reducing the time for adequate evaluation and preparation, as well as their tolerance for apnea or transition to positive pressure ventilation, independent of operator or device contributions to the risk. Although we generally view intensive care unit intubations as quite urgent, some are likely relatively routine, whereas others are done under emergent circumstances. It is difficult to quantify the urgency of tracheal intubation attempts, and we do not have data reflecting urgency in our cohort.

We thank the authors for continuing this important discussion and raising these questions. The multidisciplinary interest in improving the practice of airway management will most certainly improve the safety of airway management for these high-risk patients.

Author disclosures are available with the text of this letter at www.atsjournals.org.

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