To the Editor—Heidegger et al.¹ are to be congratulated for a well-performed study that validates the relative safety of their practice. The conclusions from their study, however, can be applied only narrowly. First, as they admit, it was conducted by individuals with extensive previous experience with the technique. Indeed, each had performed more than 200 previous bronchoscopic intubations. Therefore, the study addresses the safety of bronchoscopic-assisted intubation for those needing to maintain, rather than acquire, this skill. It does not serve to document the equivalent safety of this technique with direct laryngoscopy for those with limited previous experience. Second, the study was performed on the very population not requiring flexible bronchoscopic intubation—namely, those with normal airways. The study falls short of documenting the absence of vocal cord sequelae when performed in patients who may require this approach. Third, the authors have compared the vocal cord sequelae resulting from a nasally inserted 6-mm tube with an orally inserted 7- or 8-mm tube. Nasal tubes assume a more vertical passage through the larynx and exert less force on the posterosmedial glottis.² Likewise, smaller tubes probably exert less force on the vocal folds and arytenoid cartilages.³ Finally, their technique involved the induction of anesthesia absent neuromuscular blockers. The national guidelines referred to,⁴,⁵ insofar as they address vocal cord sequelae, could have easily been standardized by the following:

1. Using transtracheal local anesthetics in both the study and control groups.
2. Using identical induction agents. Anesthetic induction in the two groups were managed so differently that comparison of the frequency of vocal cord sequelae (VCS) after fiberoptic tracheal intubation without neuromuscular blockade (NMB) was less than 25% when compared to VCS with NMB. I believe their study fell short of the model for evidence-based medicine.

The study group and the control group were managed so differently that comparison of the frequency of VCS in either group cannot be reasonably compared. Along with objective assessment of the patients, the patients were asked to subjectively assess hoarseness and discomfort with introduction of potential bias. Using subjective assessment tools dependent on the patient’s own feelings, one must consider and attempt to eliminate distracting factors that could significantly impact the objectivity of the assessment.

To truly control for fiberoptic technique and NMB, the two groups could have easily been standardized by the following:

1. Using transtracheal local anesthetics in both the study and control patients.
2. Using identical induction agents. Anesthetic induction in the two groups was achieved with different agents, the study group receiving etomidate and the control group receiving propofol. Side effect profiles of these two agents clearly differ and could result in distraction of symptoms, especially in the study group.
3. Selection of identical endotracheal tubes (ETTs). The authors used a smaller ETT for the study group (6.0 mm ID) and a larger ETT for the control group (7.0 mm ID for females and 8.0 mm ID for males). The control group received a polyvinyl chloride ETT, and the study group received a silicone ETT. Using an identical tube such as a 6.0-mm-ID silicone tube in both groups would seem like a logical choice. In addition, both the study and control groups could have been intubated nasally to establish a more reliable control.

Another concern is the selection of patients undergoing surgery to eye, ear, and salivary glands, which could affect subjective discomfort associated with VCS.

Finally, the assessment of the vocal cords was performed by an ear, nose, and throat surgeon blinded to patient group assignment. It is difficult to believe that an experienced ear, nose, and throat surgeon could not recognize that a patient underwent recent nasal intubation. Even with the best of conditions, minor traumatic injury is often noted along the path of a nasotracheal intubation. The authors did design the study to compare with the trial of Mencke et al.,² which used a silicone ETT for fiberoptic cases and polyvinyl

Dr. Cooper has a small financial interest in Verathon, Bothell, Washington, the company that manufactures the GlideScope® videolaryngoscope.

References


(Accepted for publication January 30, 2008.)

To the Editor—I read with interest the article “Fiberoptic Intubation and Laryngeal Morbidity” by Heidegger et al.¹ The authors’ purpose was to demonstrate that the frequency of vocal cord sequelae (VCS) after fiberoptic tracheal intubation without neuromuscular blockade (NMB) was less than 25% when compared to VCS with NMB. I believe their study fell short of the model for evidence-based medicine.

The study group and the control group were managed so differently that comparison of the frequency of VCS in either group cannot be reasonably compared. Along with objective assessment of the patients, the patients were asked to subjectively assess hoarseness and discomfort with introduction of potential bias. Using subjective assessment tools dependent on the patient’s own feelings, one must consider and attempt to eliminate distracting factors that could significantly impact the objectivity of the assessment.

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chloride for direct laryngoscopy cases, and acknowledge that future study is needed to assess silicone tubes and VCS in oral direct laryngoscopy ETT placement. With respect to anesthetic agents and ETT assignment, the authors state that the goal was to compare two established techniques and not to solely compare NMB versus no NMB. However, this is in contrast to the stated goal of the study and even to the title of the article itself.

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In Reply.—We appreciate Dr. Cooper’s and Dr. Cowles’ comments regarding our article.1 We were delighted to be congratulated by Dr. Cooper and would like to comment on his thoughts. Dr. Cooper’s remarks are important because he tells us that we should only make conclusions that are based on the results, and that we should only write what we know. We attempted to do just this by summarizing that “routine use is justified for anesthesiologists experienced in this technique.” Moreover, we added that we could not rule out that we would have achieved the same results had the intubations been performed by less experienced clinicians. So, Dr. Cooper’s view strengthens our results.

Certainly, it would be interesting to know the incidence of vocal cord sequelae (VCS) in patients with very difficult airways. Performing the same randomized study on patients with an expected difficult airway would be against the recommendation of how to manage an anticipated difficult airway.2 Of course, it might be possible to perform an observational study without a control group. The main focus on those patients, however, is not the occurrence of VCS, but simply to show whether this technique is successful. This has already been shown in an analysis of almost 1,000 nasotracheal fiberoptic intubations performed by clinicians with different levels of experience in this technique.3

The goal of our study was to compare laryngeal morbidity of two well-tried and -tested methods. This was part of the study design; hence, some differences were deliberately included. Because we wanted to compare our study with the results from Mencke et al,4 it was further reasonable to use this approach.

Finally, although the influence of intubating conditions on laryngeal morbidity is still controversial, we are convinced that advancing the tube after loss of consciousness is not only very successful,3 but also difficult airway.2 Of course, it might be possible to perform an observational study without a control group. The main focus on those patients, however, is not the occurrence of VCS, but simply to show whether this technique is successful. This has already been shown in an analysis of almost 1,000 nasotracheal fiberoptic intubations performed by clinicians with different levels of experience in this technique.3

The title was chosen because laryngeal morbidity includes both VCS and hoarseness.

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(Received for publication January 30, 2008.)

Successful Use of Succinylcholine for Cesarean Delivery in a Patient with Postpolio Syndrome

To the Editor.—We would like to comment on the recent case report on the use of succinylcholine in a patient with postpoliomyelitis syndrome (PPS).1 Poliomyelitis results from infection by a picornavirus. The polio virus can cause destruction of anterior horn motor neurons with resultant limb paralysis. Motor axon terminal sprouting reinnervates previously denervated muscle fibers creating a giant mo-