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Concerns about the Purported Safety of Elective Flexible Bronchoscopic-assisted Intubation

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 posteromedial 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 the anticipated difficult airway, recommend the preservation of spontaneous ventilation. This demands much less medication than Heidegger *et al.* administered and results in a higher probability of coughing and difficulty in advancing the tube. These may increase the probability of vocal cord sequelae.

Skill at intubating with a flexible bronchoscope is essential to the safe practice of anesthesia. It is important that this skill be acquired and maintained in a manner that simultaneously meets our professional needs and protects our patients from harm. Heidegger *et al.* have demonstrated that their methods achieve those ends for experienced clinicians on patients with normal airways, but their findings cannot be extrapolated to dissimilar practices.

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Regarding Fiberoptic Intubation and Laryngeal Morbidity

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.

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

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 important for the low incidence of VCS.

We also thank Dr. Cowles for his interpretation of the results of our study. He expresses some misunderstandings, especially with the aim and the design of the study. We disagree with Dr. Cowles' statement that our study does not meet the criteria for the "model" of evidence-based medicine. We believe it adds meaningfully to the current evidence in the medical literature and among experienced clinicians that skill at fiberoptic intubation is essential for the safe practice of anesthesia. Consequently, studies in this area are likely to be beneficial for our patients. Further, it is generally known that a prospective randomized clinical trial provides the strongest level of evidence to answer a clinical question.[5] Because almost all studies have some flaws, it is of course crucial to discuss their limitations and make conclusions that are only based on the results.

As mentioned above and explained in detail in the article itself, we consciously accepted a different study design. Dr. Cowles expresses concern that the control group (standard practice) and the study group (fiberoptic intubation) were treated so differently. This is because standard practice of induction of anesthesia is so different from fiberoptic intubation. By controlling the variables as he suggests, the study would have been, in our opinion, meaningless to true clinical conditions.

We chose patients for eye, ear, or salivary gland surgery for practical reasons and believe that these types of cases are unlikely to affect the laryngeal region.

Because the vocal cords were only assessed by oral stroboscopy, it was very unlikely that the physician was aware of the intubating approach.

The aim of the study was to confirm the hypothesis that VCS after fiberoptic intubation without using neuromuscular blocking agents are below a maximum tolerable inferiority compared with conventional intubation using neuromuscular blocking agents. The results showed that with the described method for fiberoptic intubation, we do not harm the patient. By using a similar method for intubation as Mencke *et al.*[4] used in their study (control), we also had an opportunity to validate our method of assessing VCS (incidence in the neuromuscular blocking agents group was similar to that in the study of Mencke *et al.*).

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

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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-