

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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Anesthesiology 2008; 108:1151

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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,⁵ 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

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(Accepted for publication January 30, 2008.)

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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(Accepted for publication January 30, 2008.)

Anesthesiology 2008; 108:1151-2

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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 picornavi-

rus. 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-