

Is Routine Endotracheal Intubation as Safe as We Think or Wish?

EVERY year, millions (tens of millions?) of patients undergo laryngoscopy and tracheal intubation as part of their routine anesthetic care. Although this method was only rarely used in our practice before the 1960s, it is now almost as routine as placing a peripheral intravenous catheter. A great deal of attention is devoted to airway management in general (particularly the patient with a difficult airway), but we rarely give much thought to the consequence of intubation in patients with complete, normal (easy) airways.

In this issue of ANESTHESIOLOGY, Tanaka *et al.* describe measurable changes in the larynx following routine endotracheal intubations.¹ The changes consisted of increased airflow resistance that was attributed to intraoperative swelling of the laryngeal soft tissues in patients who were intubated. Such postoperative laryngeal changes were absent in the patients who received a laryngeal mask airway for anesthesia. It is tempting to dismiss the findings of this study as intuitively predictable or trivial. However, we believe that the findings represent the less severe end of the spectrum of airway injuries caused by tracheal intubation.

The findings of Tanaka *et al.* confirm our long-held belief that even routine tracheal intubation produces changes in the airway. These changes may vary from those that are very mild (detectable only with elaborate methods, such as in Tanaka's study) to the very serious. For example, Domino *et al.* analyzed the claims of airway injuries in the American Society of Anesthesiologists closed claims project.² Of the 266 claims related to airway injury, 87 involved the larynx, with the most common lesions being vocal cord paralysis, granulomas, arytenoid dislocation, and hematomas. However, "80% of laryngeal injuries were associated with routine (non-difficult) tracheal intubation. . .," and only 17 of these cases were associated with a difficult intubation. Airway injuries placed fourth (6%) behind three other major types of injuries: death (32%), spinal cord or peripheral nerve damage (16%), and brain damage (12%). Others have observed serious laryngeal injuries (*e.g.*, vocal cord paralysis, arytenoid cartilage subluxation, laryngeal gran-

ulomas, and scars) following short-term intubation and anesthesia.^{3,4} Paulsen *et al.* compiled a list of several reports of arytenoid cartilage subluxation.⁵ In most of these reports, intubation was performed without apparent difficulty and the patients were intubated for a short period of time. These findings suggest that laryngeal damage was related to intubation and not to duration of surgery. Although there are some differences in opinion with regard to general risk factors, several factors were suggested for arytenoids subluxation, including laryngomalacia, renal insufficiency, acromegaly, chronic glucocorticoid intake, and rheumatoid arthritis.

An important clinical sign of potential postoperative laryngeal injury is voice dysfunction, namely, hoarseness. The most common cause for postoperative hoarseness in patients undergoing procedures that do not involve the head and neck is swelling of the vocal cords. Recent evidence by Mencke *et al.* suggests that the quality of intubation contributes to laryngeal morbidity.⁶ Specifically, intubating conditions that are associated with various degrees of coughing, resistance to laryngoscope blade insertion (difficult laryngoscopy because of the patient being light/inadequately paralyzed), and limb movement are associated with a greater incidence of postoperative hoarseness and laryngeal injuries. Most of the laryngeal trauma occurred in patients who received no neuromuscular blockers to facilitate routine endotracheal intubations. Mencke *et al.* concluded that the use of neuromuscular blockers in the sequence of induction drugs is associated with better intubating conditions and less postoperative hoarseness and laryngeal injury. It seems that under suboptimal intubating conditions, even the mildest body movement may lead to mechanical trauma caused by the laryngoscope or endotracheal tube, resulting in laryngeal or tracheal injury. We find it very interesting that that by taking a simple measure, such as adding neuromuscular blockers at induction, the incidence of postoperative hoarseness dramatically dropped.

Although Mencke's study focused on the role of intubating conditions in postoperative laryngeal morbidity, the incidence of hoarseness or laryngeal dysfunction following routine tracheal intubation is not clear. Previous research by Jones *et al.* found that 54 of 167 studied patients had hoarseness following short-term intubation.⁷ In five of these patients, hoarseness lasted for periods ranging between 9 and 99 days. Kark *et al.* reported hoarseness that lasted for more than 6 months in 3% of women undergoing mastectomies.⁸ The figures presented by Domino, Mencke, Jones, and Kark are

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alarming and should be a source of much concern for us. Such prolonged vocal changes may not be perceived by many of us as particularly serious (as compared, for example, with a myocardial infarction or wound infection or stroke), but they are unquestionably viewed as important, distressing, and disruptive by the patients themselves.

One of the essential skills of anesthesiologists is managing the airway. On a daily basis, we manage it with hardware, plastic-ware, rods, scopes, and inflatable devices. Despite that, we know little about the impact of our manipulations on its function. Yet, based on the available information, one can reasonably conclude that even routine tracheal intubation significantly affects the larynx anatomically and functionally. Much more research is needed to accurately determine the incidence, types, and mechanisms of airway injury following routine endotracheal intubations. The results of this research will aid us in the rational development of methods for avoiding these often-missed (or ignored?) but nevertheless important complications.

One final thought: Neither this editorial nor the studies by Mencke *et al.* and Tanaka *et al.* are about airway injuries produced by difficult intubations. These injuries are well known.⁹ Rather, the current comments and the studies discussed are about airway injuries that follow

routine intubation and anesthesia and are performed correctly and to best of the abilities of anesthesiologists—the airway experts. We should recognize and acknowledge that sometimes, despite a job well done, airway injury happens. It is our obligation to find out why.

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Apples and Oranges: The Fruits of Labor in Anesthesia Care

A RECENT review of the published literature demonstrated a wide range of perioperative mortality rates, which are probably caused by differences in operational definitions and reporting sources, as well as a lack of appropriate risk stratification.¹ In this issue of *ANESTHESIOLOGY*, Sprung *et al.* report on another perioperative outcome, cardiac arrest.² As with perioperative mortality, the literature is replete with studies of cardiac arrest data using a variety of definitions and reporting sources. In the current study, Sprung *et al.* bring us closer to the development of an appropriate risk stratification model

by examining predictors of immediate survival, and survival to discharge from the hospital, following cardiac arrests during anesthesia care. The authors also show a declining rate of perioperative arrests at their institution. But, as with perioperative mortality, we must take a critical look at the effect that methodologic differences have on the interpretation of the data.

Sprung *et al.* state that “the two most recent studies of anesthesia-attributable mortality^{1,3} each found somewhat higher rates” than theirs. The authors then caution against direct comparison of these studies because the study by Newland *et al.* included cardiac surgery. In fact, the study by Newland *et al.* was not a study of anesthesia-related mortality but instead a study of anesthesia-related cardiac arrests that resulted in death. Because an arrested heart is one of the criteria for death, one might assume that the difference is trivial. Keep in mind, however, that an arrested heart is not one of the criteria for a cardiac arrest as defined by Newland or Sprung. Both of these investigators define a cardiac arrest as an event requiring cardiopulmonary resuscitation with closed chest compressions or open cardiac massage. Just as

This Editorial View accompanies the following article: Sprung J, Warner ME, Contreras MG, Schroeder DR, Beighley CM, Wilson GA, Warner DO: Predictors of survival following cardiac arrest in patients undergoing noncardiac surgery: A study of 518,294 patients at a tertiary referral center. *ANESTHESIOLOGY* 2003; 99:259–69.

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“studies of perioperative mortality alone do not include patients successfully resuscitated from cardiac arrest,” these studies of cardiac arrest do not include patients who died without cardiac compression. For example, the outcomes database maintained by the Department of ANESTHESIOLOGY at Montefiore Medical Center contains 253 deaths within 2 days after the procedure, but only 110 of these involved a cardiac arrest during anesthesia care. Of these 110 deaths, peer review judged that the anesthesiologist contributed to only five of them (approximately 1:36,000 anesthetics).

Of particular interest, “unstable patients whose arrest occurred after an anesthetic induction agent was given were not considered as having had an anesthesia-attributable cardiac arrest (regardless of the fact that anesthesia may have contributed)” in the current study. This effectively removes the patients at highest risk of death from the anesthesia-related mortality rates reported by Sprung *et al.* at the Mayo Clinic. Not only are these patients at high risk of death, but their death are also more likely to involve a human error by an anesthesiologist.¹ Previous investigators who chose not to include high-risk patients have also reported lower anesthesia-related mortality rates.⁴ Continuing the comparison between Montefiore Medical Center and the Mayo Clinic, three of the five above-noted anesthesia-related deaths that followed a cardiac arrest at Montefiore Medical Center involved unstable patients. Therefore, following the methodology of Sprung *et al.*, Montefiore Medical Center has an anesthesia-related mortality rate of about 1 in 90,000 anesthetics, which is nearly identical to that of the Mayo Clinic. Similarly, the exclusion of unstable patients from anesthesia-attributable arrests at the Mayo Clinic may also explain the improved survival rate when compared with the report by Newland *et al.* from the University of Nebraska Medical Center. Sprung *et al.* should also consider their exclusion criteria when concluding that their most common etiology of anesthesia-related arrest “contrasts with other studies of anesthesia-related arrest.”

The declining incidence of cardiac arrest at the Mayo Clinic over the duration of the study period is also quite interesting. The authors do not supply enough data

points for statistical process control to detect a trend, but this could probably be overcome by sampling more frequently (*e.g.*, monthly rather than annually). As suggested by the authors, “the decrease in the frequency of perioperative cardiac arrest may imply a significant improvement in patient care.” Keep in mind, however, that cardiac arrests are defined as events requiring cardiopulmonary resuscitation with cardiac compressions while under an anesthesiologist’s care. With changing guidelines for Advanced Cardiac Life Support, the Mayo Clinic anesthesiologists may have become more successful at resuscitating patients with early defibrillation and high-dose epinephrine before cardiac massage. Another possibility is that the emergence of critical care as a subspecialty of anesthesiology has resulted in more patients being transferred directly to the intensive care unit for immediate postoperative management, effectively shortening the observation time of the study patients.

Despite the room for varying interpretation, Sprung *et al.* are to be congratulated for creating and maintaining an exceptional outcomes database. The Mayo Clinic continues to lead the way for researchers in anesthesiology investigating perioperative outcomes. It is now time for these leaders to join forces with other investigators to standardize the methods of data collection and analysis, so that data can be shared worldwide. Large international data pools will allow us to develop risk adjustment models and identify best practices. Before we can discuss the fruits of our labor in anesthesia care, we must ensure that we are not talking about apples and oranges.

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Is Regional Anesthesia Simply an Exercise in Applied Sonoanatomy?

Aiming at Higher Frequencies of Ultrasonographic Imaging

The key steps in any successful regional anesthetic involve identifying the exact position of the nerve, reaching it with a precisely placed needle (without damage to any adjacent structures), and, finally, carefully injecting local anesthetic. Although easy in principle, clinicians are confronted daily with the difficulty of converting this theory into practice. Knowledge of anatomy based on surface landmarks is an essential starting point but is hardly ever satisfactory alone. The introduction of the peripheral nerve stimulator into clinical practice was a major advance. Unfortunately, even with this tool, our performance is still far from perfect, resulting in unpredictable block failures, inadvertent puncture of adjacent structures leading to complications, or frustrating and time-consuming trial-and-error attempts.

Alon P. Winnie once predicted: "Sooner or later someone will make a sufficiently close examination of the anatomy involved, so that exact techniques will be developed."¹ Ultrasonography may represent just such a method for providing a "sufficiently close examination of the anatomy." Studies comparing ultrasound guidance with nerve stimulator guidance have found significantly higher success rates, shorter onset times,² and a decrease in local anesthetic needs³ and complications with the former method. In this issue of *ANESTHESIOLOGY*, Perlas *et al.*⁴ contribute further to our knowledge with a volunteer study, using the newest-technology high-frequency probes (12-MHz) to identify the brachial plexus in five typical locations and to guide a needle and verify its position with nerve stimulation. With figures of excellent quality and high educational value, this article once more demonstrates the technical feasibility of ultrasound-guided brachial plexus block.

Ultrasound-facilitated nerve blocks were first reported in 1978,⁵ and interest has increased in the past 10 yr owing to progress in transducer technology and image processing. Although early studies were limited to vascular identification by Doppler ultrasound, recent stud-

ies have tried to directly visualize the nerves.⁶ Normal peripheral nerves in transverse scans appear as multiple round or oval hypoechoic areas encircled by a relative hyperechoic background. As with tendons, the connective tissue within the nerves (perineurium and epineurium) displays an anisotropic behavior, depending on the angle of the emitted ultrasound wave relative to the long axis of the nerve.⁷

Earlier, low-frequency methods were not ideal. However, the move toward higher frequencies, resulting in the 7- to 15-MHz linear transducers of today, has resulted in much higher image resolution. Moreover, ultrasound has become portable, providing increased flexibility and applicability. However, a fundamental rule of ultrasound physics shows that although higher frequency results in higher spatial resolution, the depth of tissue penetration is also reduced. Accordingly, the highest frequency is not automatically the best choice for all applications. Thus, it is not surprising that in Perlas' study⁴ the 12-MHz probe offered higher resolution compared to lower-frequency transducers in superficial plexus locations but failed to identify the nerves in 73% of the cases in the infraclavicular region, where they can easily be identified with 5-10 MHz in 3 cm distance.⁸ Different frequency probes will probably be needed for different purposes. For example, we use 15-MHz probes in experimental laboratory settings⁹ but 4- to 5-MHz probes for an ultrasound-guided posterior lumbar plexus block with a target deeper than 5 cm.¹⁰ To visualize the brachial plexus in 1 cm depth at the interscalene, supraclavicular, or axillary level, the 12-MHz transducer is obviously an excellent choice.^{4,11,12}

However, we reckon that inserting the needle on the outer end of the transducer and advancing it in a lateral to medial direction, as proposed here, contrary to the common practice of inserting it close to the middle of the transducer and guiding it perpendicularly to the ultrasound beam, makes more sense in deep blocks and is questionable for 1-cm deep targets. This may displace the insertion point unnecessarily far from the nerve and thus increase the length of tissue penetration.

In performing ultrasonography-guided blocks, observing a homogeneous and complete local anesthetic spread around the nerve is the most reliable predictor of block success. This is not necessarily achieved with the needle tip in the closest proximity to the nerve. This signifies a fundamental difference from the blind, one-

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dimensional nerve-stimulator technique in which injections are typically performed after the needle is placed as close to the nerve as possible (as defined by a low stimulating current). Interestingly, in the present study, there was no clear correlation between ultrasonographically verified needle-to-nerve contact and nerve stimulated muscle contraction with currents of up to 1.5 mA. We may speculate about the reasons,¹³ but, above all, this underlines the demand for precisely defined and calibrated nerve stimulators, at least in experimental settings. This is important, because we know, for example, that only with an impulse duration of 0.1 ms will the current correlate with the distance to the nerve. Nevertheless, commercially available devices have recently been identified as being highly variable in their accuracy of current output and preset electrical characteristics.¹⁴

Ultrasound has proved helpful for regional anesthesia in two ways: First, it allows the systematic, noninvasive, *in vivo* assessment of topographic sonoanatomy and its variations.¹¹ Performing careful ultrasound measurements enhances our anatomic understanding, tests the accuracy of common block techniques, and can even result in suggestions to modify them,⁸ as likewise demonstrated with magnetic resonance imaging.¹⁵ We still have much to learn from *in vivo* ultrasound, inasmuch as anatomy textbooks rely mainly on cadaver dissections. Second, and probably most important, ultrasound helps to individually guide the needle in real time. Advantages of ultrasound-guidance include the direct visualization of the nerves, the entire needle, or the needle tip; identification of adjacent structures to avoid; and, finally, monitoring of local anesthetic spread.

In conclusion, the use of ultrasound-guidance in regional anesthesia and interventional pain management is growing. The technique provides information about upper and lower extremity block anatomy,¹² facilitates neuraxial methods,¹⁶ and is being used to guide the performance of a wide range of other blocks, including blocks of the stellate ganglion,¹⁷ coeliac plexus,¹⁸ or lumbar facet nerves.⁹ Currently, many centers have performed thousands of ultrasound-guided procedures (*e.g.*, $n > 2,000$, Vienna Study Group), and detailed descriptions of the methods are entering textbooks.¹⁹

Ultrasound imaging brings light into regional anesthesia, which has hitherto been the domain of extraordinarily experienced "needle-magicians." The statement that nobody has an eye on the needle now can be revised: We have a dispassionate eye above the needle to control what we are doing and to give nerve blocks an objective basis. Ultrasound guidance offers anesthetists a unique chance to improve block success and decrease the rate of complications, even if they are less experienced in

regional techniques. Devices have become user-friendly, more affordable, and can be shared with other specialties. Studies like the one in this issue of ANESTHESIOLOGY⁴ contribute largely to making regional anesthesia more of a science rather than an art. However, despite all the proven advantages of ultrasound-guidance, this method is incredibly underused in daily practice. Consequently, after achieving today's high transmitter frequencies, our next step should be to markedly increase application frequency.

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