

articles. They make no comment on the fact that residents did not use a NMBD to facilitate *emergency* intubation in 83% of patients. This makes us wonder whether the authors believe that it is in some way virtuous for residents to avoid their use. An inexperienced anesthetist could conclude that he or she might be criticized for giving an NMBD, which raises the awful prospect of a patient perishing while the anesthetist hesitates.

Of course there are patients to whom it is unwise to give an NMBD, but these are mainly those to whom it is unwise to give any sedative drug, plus those with allergies or certain neuromuscular diseases. However, when general anesthesia has been induced we believe that it is more dangerous to inhibit trainees from using NMBDs than to encourage them to use them if they think it might help.<sup>11</sup> In airway management under general anesthesia, NMBDs are much more often the answer than the problem.

**Ian Calder, M.B., Ch.B., F.R.C.A.,\* Steve Yentis, B.Sc., M.B.B.S., F.R.C.A., M.D., M.A., Anil Patel, M.B., B.S., F.R.C.A.** \*The National Hospital for Neurology and Neurosurgery, London, United Kingdom. icalder@aol.com

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## Is Faculty Presence during Emergent Tracheal Intubations Justified?

*To the Editor:*—We read with interest the prospective trial by Schmidt *et al.*<sup>1</sup> and the accompanying editorial,<sup>2</sup> which suggest that faculty supervision during emergency endotracheal intubations decreases the rate of airway complications. However, as with any observational study, confounders must be considered.

First, several variables suggest that the set of intubations supervised by faculty anesthesiologists may have been slightly less emergent than those accomplished without faculty supervision. The reasons for intubation in the supervised group were more commonly airway protection and "other." The supervised group was more likely to use neuromuscular blockade to accomplish tracheal intubation. The supervised group performed a higher proportion of intubations in the intensive care unit, a setting in which decompensating patients are more likely to be recognized before complete physiologic deterioration. Moreover, unlike some floor settings, intensive care unit beds are uniformly equipped with functioning suction, oxygen, and devices to deliver positive pressure mask ventilation and staffed by support personnel who are more experienced in identifying, mobilizing and participating in emergent clinical scenarios. Regardless of location, urgent and semiemergent intubations are more likely to allow time to assemble a full complement of personnel and equipment, to optimize patient position, and to consider aspiration prophylaxis, all of which should minimize risk of various complications.

Second, as the authors suggest, the presence of a second anesthesia provider, irrespective of the level of training or experience, may facilitate safer tracheal intubation. Based on a multivariate logistic regression analysis of data collected in a prospective multicenter study,<sup>3</sup> tracheal intubation managed by an anesthesia team (including a junior and a senior provider) as opposed to a single senior provider, was shown to protect against airway complications. Our institution's experience and data support the conclusion that a second anesthesia provider, as opposed to a faculty anesthesiologist, is the process characteristic responsible for improved outcomes. The emergency intubation team at our institution includes a junior (CA-1 or CA-2) and

senior anesthesiology resident (CA-3 with at least 24 months of laryngoscopic experience). Typically, a faculty member is present when difficult intubation is anticipated. Preliminary analysis of the electronic medical records for 2,460 emergent intubations over a 4-yr period revealed a 2.3% composite complication rate, with no differences based on faculty presence. Operator-reported complications included aspiration (n = 37), dental injury (n = 4), and esophageal intubation (n = 15). Of note, 8.4% of tracheal intubations were accomplished with the aid of a bougie introducer. The availability of this adjunct or providers' experience in its use was not presented by Schmidt *et al.* This may be responsible for the rate of frequent esophageal intubation in their studied population.

Finally, time of day has been shown to affect survival to discharge after cardiopulmonary arrest.<sup>4</sup> The data collection sheet used by Schmidt *et al.*, which was presented in previous work,<sup>5</sup> includes the date and time of intubation. An analysis to evaluate the effect of nighttime or weekend intubations would be helpful. If nighttime intubations, or weekend intubations, or both, result in more complications, the explanation may be decreased faculty presence, but may also be a result of decreased nursing vigilance delaying the recognition of a need for emergency intubation, increased time from code activation to presence of the anesthesiology team, or circadian biologic factors in both patients and staff attempting the intubation.

Faculty presence is the standard of care for intubations in the operating room. Extending this standard to emergency intubations would be desirable if it were to improve patient safety. However, undesirable effects on perioperative patient safety and healthcare costs must also be considered. During nights, weekends, and other periods of limited staffing, emergency intubations may pull on-call faculty away from the operating room or intensive care unit. Dedicated faculty to cover emergent intubations will entail increased on-call commitments and economic costs in many centers. These concerns justify a prospective study in either the intensive care unit or the floor with systematic or even randomized allocation of faculty presence to clarify the con-

tribution of faculty anesthesiologists, urgency, location, time of day, and other confounders on significant patient airway outcomes.

**Jill M. Mhyre, M.D.,\* Elizabeth D. Martin, M.D., Satya Krishna Ramachandran, M.D., F.R.C.A., Sachin Kheterpal, M.D., M.B.A.**  
\*University of Michigan Health System, Ann Arbor, Michigan.  
jmmhyre@umich.edu

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*In Reply:*—We thank Calder *et al.* and Mhyre *et al.* for their comments on our editorial.<sup>1</sup>

Calder *et al.* make several points on the topic of neuromuscular blockade. Although an element of both articles, neuromuscular blockade is not the main point of the original article<sup>2</sup> or of our editorial.<sup>1</sup> We are not certain what the “take home” message of Calder *et al.* is regarding neuromuscular blockade in acute airway management, but in case there is any misunderstanding we take this opportunity to provide ours. In our opinion, all clinicians—but especially those with less experience—should always exercise careful judgment about using neuromuscular blockade in acute airway management. The evidence for this opinion is our experience of success and failure in the area, and our understanding of how the agents usually work according to plan, and how they sometimes do not. Inexperienced trainees who do not know how to manage an acute airway should try to obtain expert assistance; neuromuscular blockade should not be considered a safe or effective substitute for such assistance.

We fully agree with the suggestion by Mhyre *et al.*—as we pointed out in our editorial<sup>1</sup>—that constraints and tradeoffs (*e.g.*, staffing, scheduling, budgets, and so forth) are key considerations in making additional layers of expertise more widely available for emergency care. Some of their other concerns, though, such as problems at peripheral locations, circadian variation in the incidence of crises, and so forth, might support rather than negate our suggestions about the role of expertise. In a landmark article, Bell and Redelmeier<sup>3</sup> pointed out that of 100 acute illnesses warranting hospital admission, 23 diagnoses were associated with significantly higher mortality during out-of-hours admission, suggesting that reduced on-site staffing or expertise might affect outcome. In that report, the diagnosis associated with the greatest increase in mortality (odds ratio, 5.3) was acute epiglottitis, especially relevant to this discussion as it involves both airway management and out-of-hours care.<sup>3</sup>

Mhyre *et al.* conclude by suggesting that a prospective, possibly randomized study may be justified. Perhaps they are correct, but we suggest that any group planning such a study consider the following points. First, not all experts are created equal. Although attending physicians on average will be more expert than trainees, there is heterogeneity in both groups, and experts often have steep learning curves, if United Kingdom cardiac surgical skills are any guide.<sup>4</sup> Thus standardization for and extrapolation from such a study would be difficult.

Second, because it is a form of “parachute medicine,” emergency airway care is usually not trial-based. Indeed, the notion of randomized controlled trials in such areas has been ridiculed by some.<sup>5</sup> Because of the context of very sick patients and variable settings, study would be logistically difficult, leading to many exceptions and missed cases, and

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the baseline crude incidence rate of failure (*i.e.*, death) is low. Thus, the sample sizes required might be prohibitive.

Third, not being based on randomized controlled trials or on aggregates thereof, such as meta-analyses or systematic reviews, does not mean that the practice is not evidence-based. Far from being practiced in an evidence-free zone, clinical anesthesia is based on anatomic and physiologic rationale as well as experience. Accumulated experience such as closed claims analyses<sup>6</sup> is a potent form of evidence, and has possibly been the most influential driver of anesthesia care to date. Further research in this area will be challenged by confounders and clinical factors (*e.g.*, quality) that are difficult to quantify.

Finally, the major grounds for caution about such a study are that we are not sure what it is that would be tested. If we don't have insight into what it is attending physicians do that trainees don't do, then we don't have a grasp on the mechanism (*i.e.*, basis) of the effect. Indeed, if we did understand the critical mechanism, we might opt to rapidly transfer that knowledge to trainees, rather than embarking on what would amount to a study of whether better doctors are better than worse doctors. Conversely, if we don't understand the mechanism of the effect (or at least a plausible mechanism), then we don't have an hypothesis to test unless we opt for a version of “black box” medicine<sup>7</sup> (*i.e.*, a practice of medicine devoid of its mechanisms) in the guise of a pragmatic study. We do not think that such an undertaking would leave our profession or our patients better off in the long term.

**John F. Boylan, M.B., F.R.C.P.C.,\* Brian P. Kavanagh, M.B., F.R.C.P.C.** \*St. Vincent's University Hospital, University College Dublin, Dublin, Ireland. brian.kavanagh@sickkids.ca

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