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

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References


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In Reply.—We thank Calder et al. and Mhyre et al. for their comments on our editorial.1

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 article2 or of our editorial.1 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 editorial1—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 Redelmeier3 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, 5.5) was acute epiglottitis, especially relevant to this discussion as it involves both airway management and out-of-hours care.3

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.4 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.5 Because of the context of very sick patients and variable settings, study would be logistically difficult, leading to many exceptions and missed cases, and 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 analyses6 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 we are testing, 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” (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.

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References


7. Skrabak P: The emptiness of the black box. Epidemiology 1994; 5:553–5

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