In Reply:

We appreciate the opportunity to review and respond to the letter to the editor by Culp, Brewster, and Wernicki, in response to our review article. We thank the authors for their complimentary remarks. We are also pleased that they chose to respond and emphasize the need for prevention and rescue of drowning persons to further improve the survival rate from this disaster.

They make the point that it is far more efficient and effective to prevent or interrupt the drowning process than to treat it after it renders the patient unconscious or even lifeless. We agree fully. Unfortunately, not all swimming pools have lifeguards in attendance. Further, although we believe that most lifeguards are superb in fulfilling their responsibilities, not all lifeguards are fully trained in lifesaving technique and basic cardiopulmonary resuscitation. In our experience, we have reviewed cases where some lifeguards did not give their undivided attention to their lifeguarding duties, whereas others were very lax in fulfilling their responsibilities. Still others tolerate pools with inadequate maintenance to where the pools themselves presented a hazard to swimmers.

We endorse the authors concluding paragraph "Drowning is a global problem that can be dramatically reduced by teaching people how to swim, by encouraging swimming in lifeguarded areas, and by improving field resuscitation techniques. Promoting attention to the entire continuum of the drowning prevention spectrum will result in the best possible outcome." We would add, however, that lifeguards should receive proper, extensive formal training, leading to certification as a lifesaver and in basic cardiopulmonary resuscitation. Further, lifeguards should remain conscientious and vigilant in carrying out their duties and in providing continuous attention to their responsibilities at all times.

Jerome H. Modell, M.D., D.Sc.(Hon.),* A. Joseph Layon, M.D. *University of Florida, Gainesville, Florida. jmodell@anest.ufl.edu

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Residual Neuromuscular Blockade and Upper Airway Muscles

To the Editor:

I read with great interest the article by Herbstreit *et al.*[1] describing the effect of residual neuromuscular blockade on upper airway collapsibility in humans. The authors observed that upper airway integrity was impaired when the train-of-four ratio was maintained at the level of 0.5 or 0.8 with rocuronium. In a former study in volunteers, I and others could not demonstrate any susceptibility to upper airway obstruction when the train-of-four ratio was maintained to 0.5 with an infusion of vecuronium.[2] In our study, decreased inspiratory negative pressure, which is known to exaggerate the possibility of upper airway collapse, was elicited by increasing ventilation during carbon dioxide rebreathing or by adding an inspiratory resistance. Breathing with pressure at airway opening held at a pressure from -5 to -40 cm H_2O was also tested. No effect of partial neuromuscular blockade maintained at a train-of-four ratio of 0.5 on upper airway could be observed. When the upper airway pressure was progressively decreased to -40 cm H_2O, no participant showed evidence of upper airway collapse or flow limitation at any time period. The main difference between the two studies is that subjects were breathing through a mouthpiece in our study[2] instead of a nasal mask in the study by Herbstreit *et al.*[1] Therefore, the flow limitation observed by Herbstreit *et al.*[1] may be related to resistance at the velopharynx rather than to an impaired compensatory response of the genioglossus muscle.

I agree with Herbstreit *et al.*[1] when they state that the upper airway muscles are more susceptible to neuromuscular blocking agents than the diaphragm. However, because the diaphragm is the most resistant muscle to neuromuscular blocking agents among all skeletal muscles, this comparison is not valid to point out a particular susceptibility of upper airway muscles to neuromuscular blocking agents. For example, the geniohyoid muscle, which also contributes to upper airway integrity, was shown to be more susceptible than the diaphragm but less susceptible than the adductor pollicis to mivacurium in humans.[3] Finally, as suggested by Herbstreit *et al.*,[1] further work on the function of other airway openers should be performed. In the future, for a rigorous evaluation of the effect of residual neuromuscular blockade on the function of upper airway muscles, the investigator should be blinded to the level of neuromuscular blockade.

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In Reply:

We thank Dr. Duvaldestin for his comments regarding our article.[1] Dr. Duvaldestin points out that our assessor-blinded

study is at odds with his own findings.[2] We believe that findings from both studies can be easily reconciled by taking into account some overlapping findings as well as a few obvious methodologic differences between these studies.[1,2]

In accordance with our data, Duvaldestin *et al.* did not observe flow limitation during normal breathing. However, our data show that integrity was impaired during airway challenges.

Duvaldestin and coworkers[2] correctly point out that, in their study, volunteers were breathing *via* a mouthpiece whereas we used a nasal mask during our experiments.[1] Indeed, this is an important methodologic difference. In fact, our approach allows for analysis of the pressure–flow relationship of the whole supraglottic airway, whereas the method of Dr. Duvaldestin is restricted to the oropharyngeal airway only. However, it is clinically important to evaluate both, the retropharyngeal and retroglossal upper airway.

We have shown that the effects of partial neuromuscular blockade on the upper airway muscles are significantly greater in the retropalatal compared with the retroglossal airway.[3] In accordance, Schwab and coworkers[4] showed that the soft palate plays the predominant role in mediating airway narrowing during sleep, and this is thought to be related to a decrease in upper airway dilator muscle activity. Thus, the retropalatal area seems to be particularly susceptible to a decrease in upper airway dilator tone. Accordingly, the technique used by Dr. Duvaldestin and coworkers is not sensitive to detect upper airway collapse in its most collapsible segment.

Although in Dr. Duvaldestin's opinion this circumstance is the main difference between the two studies, we believe that further differences in methodology exist with far greater impact on the results.

First, Dr. Duvaldestin and coworkers studied six volunteers, and there is no information provided how the number of volunteers was determined. Our study was performed following a power analysis based on pilot experiments and we examined 15 volunteers. Thus, one might speculate that Dr. Duvaldestin's study lacked the power to demonstrate significant results—absence of significance does not reflect significance of absence.

Second, Dr. Duvaldestin and coworkers conducted a negative pressure challenge using a stepwise decrease in airway pressure from ambient pressure to -40 cm H_2O with a decrease in airway pressure by 5 cm H_2O implemented every three respiratory cycles. This technique is assumed to assess active dynamic responses to airway obstruction, and the critical airway pressure obtained is thus the so-called active Pcrit.[5] Depending on the volunteers' respiratory rates, the time between the onset and the nadir of the negative pressure challenge with this technique varies and occurs over time. Most likely, this results in differences in compensatory mechanisms such as airway muscle activation or changes in respiratory drive. In our study, in contrast, volunteers were exposed to short random pressure drops alternating with longer periods of breathing at a (slightly positive) holding pressure. This latter technique is suitable to assess the passive mechanical properties of the upper airway and has thus been coined the passive Pcrit.[5] This variable reflects the mechanical integrity of the upper airway and, potentially, the patient's ability to compensate for challenges such as a forced inspiration.

Although not addressed in any of the publications, upper airway muscles are likely more susceptible to neuromuscular blocking agents than the diaphragm. Whether this is due to particular resistance of the diaphragm to such drugs or to particular susceptibility of the upper airway muscles has not been elaborated.

Accordingly, although we agree with Dr. Duvaldestin that further work on the susceptibility of the airway muscles is warranted, this issue does not alter our findings or dilute their significance.

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Inhibition of Muscle Acetylcholine Receptors by Nondepolarizing Drugs: Humans Are Not Unique

To the Editor:

We have several concerns about the data and the conclusions of the article by Fagerlund *et al.*[1] that reported on block of adult human muscle acetylcholine receptors (nAChR) by nondepolarizing neuromuscular blockers (NDMBs). Overall, the study by Fagerlund *et al.*[1] confirms that nondepolarizing neuromuscular blocking drugs have both competitive and noncompetitive blocking actions at neuromuscular nicotinic receptors. However, the study does not have the resolution to define the time or receptor state dependence of the block and, hence, provides no insights into the relative roles of the mechanisms in the clinically relevant actions of