In Response
We thank Eckle and Grasshoff1 for their interest in our article.2 We agree that there are several factors that might influence the duration of action of succinylcholine.

The Danish Cholinesterase Research Unit has used genotyping of the butyrylcholinesterase gene, including detection of the K-variant, since 2007, and we have had 1 similar case of postpartum hemorrhage with prolonged neuromuscular blockade after succinylcholine 1.6 mg/kg. The butyrylcholinesterase activity (4146 U/L) was less than the normal range. The duration of the neuromuscular blockade to full recovery was 90 minutes. The patient was heterozygous for the K-variant.

Several factors may influence the duration of action of neuromuscular-blocking drugs as described by Eckle and Grasshoff,1 but we wish to add that a very low butyrylcholinesterase activity may also result from concomitant use of various medications among other cyclophosphamide, tacrine, bambuterol, and ecothiophate eyedrops.3–5 Furthermore, the duration of succinyllcholine may be prolonged in patients with malignant disease.5

Clinically important prolongation of the duration of action of neuromuscular-blocking agents is therefore not uncommon, and we emphasize the importance of neuromuscular monitoring when using neuromuscular-blocking agents.5

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REFERENCES

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Bioacoustics, Breaths, and Biostatistics

To the Editor
Although Ramsay et al.’s1 evaluation of a bioacoustic sensor (BAS) to measure respiratory rate (RR) and apneas in patients at risk for respiratory depression appears to hold promise in the postanesthesia care unit—an area with considerable ambient noise—we question their methodology for generating reference values for RR and apneas and their comparisons between capnometry and BAS. The reference RR, from which accuracy and precision for the 2 monitors were calculated, was derived from a subjective assessment of the respiratory cycle by technicians, hired by the sponsor, who simultaneously viewed the capnograph and BAS waveforms, while listening to the breath sound signal. An unspecified “postprocessing” step generates a “reference RR” for each monitor, without detail on the thresholds for CO2 and the acoustic signal technicians used to establish airflow (breathing). The subjects were largely obese and had a high prevalence of obstructive sleep apnea. These patients typically experience hypopneas (reductions in airflow) after anesthesia that do not produce crisp end tidal plateaus on a capnograph nor clean airway sound envelopes.

This study would have benefited from an objective, “gold standard” reference signal, such as the pneumotachometer Yu et al.2 used to detect apneas from tracheal sounds. This allows proper definition of a “functional breath” using a flow rate threshold (i.e., 3 L/min), eliminating the subjective interpretation of the respiratory cycle by technicians. An unbiased reference signal would also allow specificity to be calculated which is relevant to the recent Joint Commission Sentinel Event alert on alarm fatigue.3 If the BAS sensitivity for respiratory pauses is indeed superior to capnometry, does it come at the expense of specificity, and clinically unacceptable high false alarm rates? Since both devices have received Food and Drug Administration/CE Mark clearance for reliably measuring RR and have configurable alarm limits, a post hoc comparison of bedside-triggered alarms in this clinical setting would have been informative. More noninvasive, unencumbering, continuous respiratory monitors are desperately needed to detect opioid-induced ventilatory impairment, yet this article fails to convince us that BAS trumps our successful experience with capnography.

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In Response

We agree with Overdyk et al.¹ re their major concern that a more noninvasive, unencumbering, continuous respiratory monitor is desperately needed to detect opioid-induced ventilatory impairment. The Joint Commission and the Anesthesia Patient Safety Foundation have both come out in support for this type of monitoring technology.²,³

Understanding the shortcomings of capnography in terms of patient tolerance and other limitations,⁴,⁵ we decided to evaluate the new noninvasive bioacoustic sensor that has recently become available, in a robust clinical setting, that of the postanesthesia care unit. The postanesthesia care unit is an area with much extraneous noise and activity and also an area where respiratory depression is common in all patients, those with or without sleep apnea. We felt that this would give a good clinical test of the new technology.

We did use technicians, hired especially for this trial by the sponsor, to count waveforms and respiratory rates. They were blinded to the study protocol and had no knowledge as to the end points of the study. We agree we could have differentiated between low and high gastric fluid volume states. A tentative grading system could serve as a screening tool to differentiate between low and high gastric fluid volume states. As grade 1 was associated with gastric fluid volume >100 mL in 75% of subjects with gastroscopically measured fluid volume. As grade 1 was associated with gastric fluid volume >100 mL in 23% of subjects, while gastric fluid volume was >100 mL in 23% of subjects, any of which can lead to gastric distension and/or coughing, factors that may cause episodes of gastroesophageal reflux. The combination of a gastric volume >0.8 mL/kg with such risk factors may be sufficient to cause significant aspiration with pulmonary damage.⁶,⁷

Ultrasound Assessment of Gastric Volume: What Is the Best Threshold?

To the Editor

Using ultrasound, Perlas et al.¹ assessed the accuracy of a 3-point grading system (0, 1, 2) describing the appearance of the gastric antrum and its correlation with gastroscopically measured fluid volume. As grade 1 was associated with gastric volume >100 mL in 23% of subjects, while gastric fluid volume was >100 mL in 75% of subjects with a grade 2, the authors concluded that this 3-point qualitative grading system could serve as a screening tool to differentiate between low and high gastric fluid volume states.

However, based on the results of studies conducted in animals suggesting a volume of 50 mL (or 0.8 mL/kg) as a critical volume for severe aspiration in humans,² it may be that a threshold of 100 mL above which patients were considered at risk of aspiration was too high. The pathophysiology of aspiration during general anesthesia involves several risk factors, any of which can lead to gastric distension and/or coughing, factors that may cause episodes of gastroesophageal reflux. The combination of a gastric volume >0.8 mL/kg with such risk factors may be sufficient to cause significant aspiration with pulmonary damage.³,⁴

REFERENCES


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