

blockade) is not the monitor but the anesthetist.” To reduce the incidence of residual blockade and adverse respiratory events in Evanston, we recommend that these authors and practitioners more carefully evaluate the degree of neuromuscular blockade required for their surgical patients, and either follow the recommendations above for using conventional nerve stimulators or use acceleromyography to guide the timing of tracheal extubation.

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In Reply:—We thank Dr. Horowitz for his comments on our study.¹ We welcome the opportunity to address his criticisms of the methodology used in our investigation and of our conclusions related to the effect of acceleromyography monitoring on residual neuromuscular blockade and adverse postoperative respiratory events.

First, we agree with the statement that nuances in neuromuscular management protocols may affect outcomes. Practices related to dosing, monitoring, and reversal of neuromuscular blocking agents may vary widely between institutions. The protocol used in our control group (conventional qualitative train-of-four [TOF] monitoring) was designed to reflect “optimal” neuromuscular management, as defined by Kopman *et al.* (use of intermediate-acting muscle relaxants, avoidance of total twitch suppression, anticholinesterase reversal of blockade at a TOF count of 3–4).² These techniques, which may reduce the incidence of residual paresis in the postanesthesia care unit, are routinely used at our institution in surgical patients requiring muscle relaxation. Dr. Horowitz suggests that the methodology of neuromuscular monitoring used in the conventional TOF group was flawed, because use of visual evaluation of TOF responses may result in an underestimation of the level of the blockade and an overestimation of neuromuscular recovery. Available evidence does not support this hypothesis. Two studies specifically comparing visual *versus* tactile assessment of fade concluded that the ability of both techniques to detect fade was comparable at TOF ratios below 0.4 and between 0.4–0.7.^{3,4} The sensitivity in detecting fade was poor with both methods at all TOF ratios > 0.4, and no statistically or clinically significant differences were observed when either visual or tactile assessments were evaluated.^{3,4} Therefore, we do not believe that using tactile instead of visual evaluations of TOF responses would have influenced our findings in the conventional TOF group. In addition, there are no clinical studies demonstrating that the use of tactile assessments of TOF responses results in a reduced incidence of postoperative residual blockade when compared to visual evaluations.

Second, Dr. Horowitz questions our use of interoperative acceleromyography monitoring in our study group. We agree that quantitative neuromuscular monitoring does not provide any additional information over standard peripheral nerve monitoring during moderate levels of neuromuscular blockade (TOF count of 2–3) required for surgical relaxation. As described in our article, the value of acceleromyography monitoring is primarily during neuromuscular recovery. Our data sug-

gests that acceleromyography monitoring allows for more rational and precise neuromuscular management during the last 45–60 minutes of the anesthetic.

Third, Dr. Horowitz states that we “did not follow common practices of neuromuscular monitoring and management of extubation when using a conventional monitor.” Dr. Horowitz does not define what these “common practices” are. Current evidence suggests that “common practices of neuromuscular monitoring” are not evidence-based, and techniques proven to reduce the incidence of residual neuromuscular blockade are infrequently applied by clinicians. Surveys from Germany, Denmark, France, and Great Britain all indicate that quantitative and qualitative monitoring is rarely used in daily clinical practice.^{5–7} In addition, knowledge about appropriate neuromuscular and clinical criteria required to exclude residual paresis before tracheal extubation is lacking.^{5–7} Although we did not follow “common practices of neuromuscular management” (which would have increased the incidence of residual neuromuscular blockade in the conventional TOF group), we believe that our neuromuscular management protocol represented the best available evidence. In fact, the two previous randomized acceleromyography trials compared a group of patients monitored with acceleromyography with a control group receiving no neuromuscular monitoring (the more “common clinical practice of neuromuscular monitoring” referred to by Dr. Horowitz).^{8,9} Of interest, the incidence of residual paresis was significantly reduced by acceleromyography monitoring in all three randomized trials. Furthermore, neuromuscular blockade was reversed at a mean visual TOF count of 4 in both groups, which represents good “evidence-based” practice.

Methods proven to reduce the incidence of postoperative residual blockade (use of intermediate-acting neuromuscular blocking agents, avoidance of total twitch suppression, anticholinesterase reversal of blockade at a TOF count of 3–4) should be adopted by clinicians. However, current data does not support the belief expressed by Dr. Horowitz that use of tactile assessment of TOF responses is superior to visual evaluation in reducing the risk of residual neuromuscular blockade and adverse postoperative outcomes. At the present time, “evidence-based standards for conventional monitoring” as described by Dr. Horowitz do not exist. Such guidelines would likely result in increased routine use of neuromuscular monitoring and anticholinesterase agents, and reduced complications related to incomplete neuromuscular recovery in the postoperative period.

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Two or Three Interviews?

To the Editor:—We have read with great interest the manuscript by Davidson *et al.*, related to the incidence of awareness in a pediatric population. We should congratulate the authors for their effort.¹

They report an incidence of awareness of 0.2%. This value is significantly lower than others studies, including a previous one from the same author.²

We would like to add some comments to the discussion, and specifically another possible explanation for the lower incidence of awareness.

The authors in this study conducted only two postoperative interviews, at 24 and at 72 hours. They claimed that a third interview at 30 days had low positive findings, although in the previous study by the same authors they conducted three interviews and the last had a positive findings of 29%. Two of the seven reported cases appeared with the third interview.² The overall incidence of awareness in the pediatric population was 0.8%, over 921, significantly higher than the present study.

The Brice test,³ to our knowledge, seems to be the best methodology to study this complication, with different modifications depending on the population undergoing the study. According to that test, ideally three interviews should be conducted: within 24 hours, between 24 and 72 hours, and at 30 days after surgery.⁴⁻⁵

In a clinical condition as the one reported, we should ideally follow methodology already validated or at least accepted by current anesthe-

sia practice. In this study, the change in the protocol may be one of the reasons explaining the lower incidence of awareness.

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In Reply:—I wish to thank Dr. Delfino for his comments. I agree that not having an interview at 1 month may have increased the false negative rate; however, I believe this is unlikely to have made a substantial difference to our result.¹ In children, the proportion of cases of awareness first detected at 1 month is appreciable but not large. In our earlier study we detected 2 of the 7 at 1 month, and Lopez *et al.* detected one of their 5 confirmed cases at 1 month, while at 1 month Blusse *et al.* detected no extra cases of true awareness.²⁻⁴ A 25-30-percent increase would not have changed our overall finding of a lower rate of awareness. It should also be noted that adhering to three interviews may result in an increase in false positives, as there is good evidence to suggest that repeated questioning increases implanted memory in children.

Dr. Delfino raises the very important point of validity of awareness assessment. We have found 76 papers describing awareness under anesthesia. Authors describe all sorts of different numbers, timing, and design of interviews. There is no single accepted methodology, and

certainly none has been validated; indeed, it is difficult to see how one can be validated. Could it be that the variation in methods used implies that no method is clearly the best? Interestingly many people claim to use the Brice study design, although their study design bears little resemblance to his (Brice played auditory stimuli during anesthesia and interviewed the patients three times in hospital within the first week⁵). Similarly, the questions Brice used have been modified. The phrase "Brice interview" is becoming meaningless, as increasingly authors use appropriately different and improved study designs and interviews.

I agree that by using the same measure researchers can better understand the etiology of awareness and better compare interventions. However, current measures are still too subjective. Even the measures described by Myles *et al.*⁶ and Avidan *et al.*⁷ rely on subjective ratings by adjudicators. We should not yet accept any awareness assessment method as a gold standard, but continue to seek more accurate ways to measure this important phenomenon.

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