

easy to organize. That said, the fact that apparently suitably anesthetized patients move during isolated forearm test (IFT) after induction and tracheal intubation is well established and unsurprising. Long reported, with systematic review showing 31 previous papers with more than 1,300 patients studied,² a positive response to IFT is easily reproducible by any anesthesiologist, anywhere, at any time. There is some modest interest in the now reported response rate (~5%)¹ being lower than the aggregate of these previous studies (~40%),² but it is difficult to see what else is novel about this latest report.

Pryor and Veselis³ offer important advice for the direction of future research and I would like to add two suggestions based on paradoxes in the observations. By paradox I mean responses that appear difficult to reconcile, given the stimulus. During the IFT, when we observe the patient moving only to verbal command but not to the obvious, ongoing, and greater stimulus of surgery, we properly regard that as surprising enough to develop sophisticated theories of dysanaesthesia,^{4,5} connected consciousness,¹ or cognitive unbinding.⁶ Yet, when a patient during IFT fails to move to verbal command, but makes other spontaneous movements that appear purposeless, we dismiss these movements as reflex or light anesthesia. We do not seem equally surprised that a patient light enough to move will not also respond to command. Perhaps it is time to study also this second apparent paradox in more detail, especially if brain imaging coupled with IFT is a way forward, as Pryor and Veselis suggest.³

A much more important paradox is why the finding cannot be reproduced in the nonparalyzed (*i.e.*, patients who have received no neuromuscular blockade). I have already reported on the impossibility of eliciting a positive IFT response to verbal command during surgery in these circumstances.⁷ Even when patients retain the ability to move spontaneously to stimuli, they fail to respond to verbal command if unparalyzed, even when they have received the same anesthetic doses and are at similar bispectral index levels as those reported in previous studies. Why this paradox? Why do things change when they are (save the isolated forearm) paralyzed? This distinguished and experienced team has the infrastructure now to explore this paradox more robustly than I previously reported. So, in good spirit I challenge Sanders *et al.*¹ to harness their international collaboration and report a single case of positive IFT in an apparently suitably anesthetized but unparalyzed patient, anywhere in the world. Or, if they are unable to do so, to explain why this is impossible and how this paradox fits into existing theories of a positive IFT response.

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This letter was sent to the authors of the original article referenced above, who declined to respond.—Evan D. Kharasch, M.D., Ph.D., Editor-in-Chief

Competing Interests

The author declares no competing interests.

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Current Status of Neuromuscular Reversal and Monitoring: Posttetanic Neuromonitoring and Other Considerations

To the Editor:

The recent comprehensive review article by Drs. Brull and Kopman¹ outlines the challenges and opportunities of the current status of neuromuscular reversal and monitoring. Their superlative and informative review is clearly destined to be a go-to reference on the subject. Importantly, it should serve as a rallying point for advancing future neuromuscular blockade (NMB) and function monitoring.

Several aspects of this article do warrant additional comment, however. First, the article deals with many important concepts in NMB monitoring and reversal, including not only perioperative considerations, but issues pertinent to the intensive care unit (ICU) where residual neuromuscular blockade, and associated patient awareness, has occasionally been reported.² Given that the article will rightly take its place as a definitive article on the subject, and as an advocate for postpublication peer-review, I was curious as to why

the section discussing awareness from residual paralysis in the ICU included a reference to an article on hypothermia in the ICU (that does not actually mention awareness at all).³ That minor irregularity aside, the excellent text, tables, and figures make for an easy to understand description of all the important concepts in NMB monitoring.

A second issue that was particularly interesting was in the discussion of posttetanic count (PTC) as it pertains to posttetanic facilitation. Although the important information the authors provided was accurate, it incompletely addressed an often-misunderstood PTC concept—that is, the time period following a tetanic stimulus that the neuromuscular junction is affected and that subsequent train-of-four (TOF) monitoring might be impaired. Indeed, Hakim *et al.*⁴ recently dispelled the common misconception that PTC impairs the NMB for a protracted period of time, showing that TOF responses are reliable as early as one minute after a PTC. I think it is worthwhile bringing this to the readers' attention, particularly in a definitive and comprehensive article.

Lastly, both Brull and Kopman, as well as the accompanying editorial by Naguib and Johnson,⁵ highlight the importance of moving forward the “state of the art” of NMB monitoring. Importantly, the editorial highlights the American Society of Anesthesiologists' significant gap in providing guidance on neuromuscular blockade monitoring, particularly when compared with other similar anesthesia societies.^{6,7} Articles such as this one from Brull and Kopman will, we can hope, encourage the American Society of Anesthesiologists to take a more progressive stance on the subject and advocate for the use of NMB monitoring whenever neuromuscular blocking drugs are used.

Competing Interests

The author declares no competing interests.

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In Reply:

We wish to thank Prof. Hilary Grocott for his excellent letter and for the kind words regarding our review article¹; we are honored by his praise. In his letter, Prof. Grocott had several important comments to which we would like to respond. First, we attempted to remind the reader that the issue of unintended patient awareness during periods of neuromuscular paralysis may occur in various clinical settings, including the intensive care unit. Specifically, it has been reported that neuromuscular blocking agents may be employed to control shivering (and decrease oxygen consumption) during induction of therapeutic hypothermia, and such therapy “may mask insufficient sedation” that may result in unintended patient awareness and recall.² This was the basis for our inclusion of the reference.¹

Our discussion of posttetanic count included a description of the “transient increase in the amount of acetylcholine released,” and stated that, “the intensity of subsequent muscle contractions will be increased (potentiated) briefly (period of post-tetanic potentiation, which may last 2 to 5 min).”¹ The period of posttetanic potentiation is based on the results reported by Brull *et al.*,³ which are consistent with the subsequent reports by Hakim *et al.*,⁴ as Prof. Grocott correctly points out. These effects are short-lived (minutes) only during clinical situations of steady-state neuromuscular block, however (*i.e.*, during continuous infusion of neuromuscular blocking agents). During recovery from bolus doses of neuromuscular blocking agents, tetanic stimulation shortens the time to 75% recovery of vecuronium from 7.4 ± 2.8 min to 5.0 ± 2.6 min, “such that the response of the tested site may no longer be representative of other muscle groups.”⁵

Finally, we are in complete agreement with, and fully supportive of, Prof. Grocott's call for the American Society of Anesthesiologists to “take a more progressive stance on the subject and advocate for the use of monitoring whenever neuromuscular blocking drugs are used.”

Competing Interests

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