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 article[1]; 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

Dr. Brull has had investigator-initiated funded research from Merck, Inc. (Kenilworth, New Jersey; funds assigned to Mayo Clinic); is a shareholder and member of the Board

of Directors in Senzime AB (Uppsala, Sweden); serves as a member of the Board of Directors for Anesthesia Patient Safety Foundation (Rochester, Minnesota); is a member of the Scientific Advisory Board for ClearLine MD (Woburn, Massachusetts) and The Doctors Company (Napa, California); and has a patent-licensing agreement with Mayo Clinic (Rochester, Minnesota). Dr. Kopman declares no competing interests.

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Intraoperative Mean Arterial Pressure Targets: Can Databases Give Us a Universally Valid "Magic Number" or Does Physiology Still Apply for the Individual Patient?

To the Editor:

With great interest we read the article by Salmasi *et al.*[1] reporting the results of a database study investigating the relationship between acute postoperative kidney and myocardial injury and intraoperative hypotension (IOH) either defined as a reduction from baseline mean arterial pressure (MAP) or absolute MAP thresholds. The authors, again, need to be commended for providing another piece of the puzzle on how to better define and understand IOH using their impressive database. In line with other data,[2] this study demonstrates a gradually increasing risk for both kidney and myocardial injury for longer exposure beneath certain MAP thresholds (both absolute or relative) and therefore adds to the evidence that IOH-associated organ failure is a function of hypotension and time.[3] Yet, the main new question this study aimed to answer was whether a definition of IOH should be based on absolute MAP thresholds or on a relative decline from baseline MAP. The authors' conclusion seems to make our daily practice as anesthesiologists very easy: "a strategy aimed at maintaining MAP above 65 mmHg appears to be as good as one based on the percentage reduction from baseline."[1]

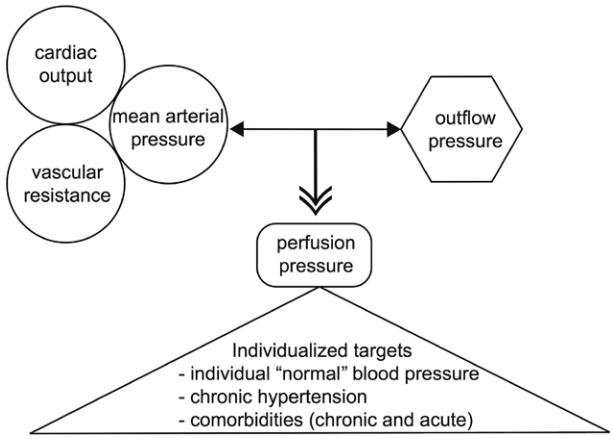

Fig. 1. Individualized perfusion pressure targets. This figure illustrates that perfusion pressure depends on inflow pressure (mean arterial pressure) and outflow pressure. Mean arterial pressure, in turn, is a function of blood flow (cardiac output) and systemic vascular resistance. Individualized targets for perfusion pressure should consider individual "normal" blood pressure, chronic hypertension, and chronic and acute comorbidities.

This database study has many strengths and provides robust results based on sound statistical analyses accounting for many confounding clinical factors. In contrast to many previous studies that used preinduction MAP as "baseline value," the authors defined baseline MAP as "average of all MAP readings in the 6 months before surgery, excluding measurements during a hospital stay."[1] Given the fact that a very recent study[4] again emphasized that preinduction MAP is markedly higher than "normal" preoperative MAP, this chosen definition is very thoughtful. That said, we would like to take the position of the devil's advocate and question the authors' conclusions about the indiscriminate use of an absolute MAP threshold of 65 mmHg in all patients.

The patient characteristics as well as the C-statistic suggest that this study included a highly heterogeneous group of patients with many potential confounding factors that might have influenced the association between MAP and IOH. If clinicians take the authors' conclusion about intraoperative blood pressure management based on a single, universally valid "magic number" (absolute MAP target of 65 mmHg) literally, this might put individual patients at marked risk of hypoperfusion and organ failure for several reasons related to cardiovascular physiology:

First, perfusion pressure—not blood pressure—is our ultimate target during perioperative hemodynamic management. As perfusion pressure is "inflow pressure" (*i.e.*, MAP) minus "outflow pressure" (fig. 1), no general MAP targets can be recommended but MAP must be adjusted considering the individual patient's outflow pressures (*e.g.*, central venous pressure, intrathoracic pressure, intra-abdominal pressure). For instance, a patient with high intra-abdominal