Magnetic resonance imaging–induced vertigo in anesthesia providers may become more common as the strength of MRI scanners increases. We think that education and prevention regarding this problem are imperative. Anesthesia providers should be instructed to stay as far away from the scanner as possible while still providing safe patient care. When possible, clinicians should avoid leaning directly into the bore of the MRI scanner. Rapid movements, including both linear translation and head rotation, should be avoided. Finally, back-up personnel should be available in the event that a provider experiences intense vertigo that impairs his or her ability to safely care for patients. It is our hope that future revisions of the Practice Advisory on Anesthetic Care for Magnetic Resonance Imaging will address these concerns.

Competing Interests

The authors declare no competing interests.

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In Reply:

On behalf of the American Society of Anesthesiologists Task Force on Magnetic Resonance Imaging, we appreciate the efforts of Dr. Gorlin and coauthors to publicize the phenomenon of vertigo and other physical effects that may be experienced by healthcare professionals who work in the magnetic resonance imaging (MRI) environment. They note that these effects are neither widely known nor commonly experienced by anesthesia professionals, even among those who spend a great deal of time providing anesthesia care for patients undergoing MRI. Although there is a lack of strongly supportive evidence, we believe that these experiences may well be related to the strength of the static magnetic field, designated by the tesla number of the scanner, and movement of the individual (or more specifically one's head movement) within that field near its central region. Nurses routinely working with patients lying within 1.5- and more so 3-tesla scanners have reported associated health complaints.[1] Leaning inside the scanner bore to locate the pulse oximeter, find an IV injection port, or assess a patient's airway may be the kind of activity that could produce this sensation. It may be that anesthesiologists do not encounter these effects as much because they are less apt to engage in this activity to the extent as do the nurses with whom we work. Despite the decades-long recognition of MRI-associated vertigo and other neurobehavioral effects, no long-term deleterious consequences have been documented to date.[2]

Awareness of this phenomenon and prudent caution to avoid sudden head movement in the area of the scanner bore would appear to be common sense advice; however, at this time, the accumulated evidence needed for such a recommendation is not available. Admittedly, protocols at some research MRI facilities where 7-tesla and higher static field magnets are in operation prohibit technicians from working alone in the scanner as a precaution against the effects of disabling vertigo and its untoward consequences. At present, we cannot recommend without more compelling evidence that "anesthesia providers be instructed to stay as far away from the scanner as possible" or that "clinicians should avoid leaning directly into the bore of the MRI scanner." Finally, having "back-up personnel available in the event that a provider experiences intense vertigo that impairs his or her ability to safely care for patients" cannot be recommended without evidence that such resources are justified. Although we do agree that this phenomenon may occur more often in the future as higher field strength magnets evolve from the research arena into clinical imaging, the American Society of Anesthesiologists process of practice parameter development will require convincing evidence to make appropriate recommendations regarding neurobehavioral and cognitive effects of MRI. The task force does intend to address this issue in a future update of the Practice Advisory.

Competing Interests

The authors declare no competing interests.

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Suboptimal Protocol?

To the Editor:

Park *et al.*[1] compared surgical pleth index (SPI)-guided analgesia with conventional analgesia during adenotonsillectomy in 45 pediatric patients. The authors confirmed their primary outcome that intraoperative fentanyl requirements are lower in SPI-guided patients. However, they failed to confirm any secondary outcomes, instead showing that intraoperative sevoflurane consumption, emergence agitation, pain, and analgesic requirements were all aggravated in SPI-guided patients. The authors concluded that SPI may not be valid in children.

I congratulate the authors for carefully blinding study personnel, which surely promoted accurate results. However, I am concerned by the authors' analgesic protocol. Adenotonsillectomy is a short and painful procedure, in this case, averaging only 25 to 30 min of anesthesia and just 15 to 18 min of surgery.

Given the authors' protocol for analgesic administration, it seems likely that patients in both groups were undertreated. No analgesics were given before incision; moreover, the protocol mandated analgesic administration only after SPI increased to at least 50 or an increase in blood pressure or heart rate to at least 120% for a minimum period of *3 min* for the initial event and *5 min* for subsequent events. This seems a remarkably long cycle time for such a short operation. Many clinicians would argue that participating patients should have been preemptively treated and that a shorter cycle period would be appropriate.

Patients in the SPI-guided group were, on average, given just a single 0.5-μg/kg bolus of fentanyl (average total dose only 0.4 μg/kg). Patients in the control group were given approximately three boluses (average total fentanyl dose of 1.7 μg/kg). A more typical preincision loading dose would be 1 to 3 μg/kg fentanyl for adenotonsillectomy if fentanyl is used as single agent for analgesic treatment both intraoperatively and postoperatively.[2–4] A consequence of avoiding preemptive analgesia and a protocol-mandated long cycle time is that at least some patients may never have reached analgesic equilibrium—thus not truly testing the efficacy of SPI guidance. The high incidence of tachycardia events in both study groups (67%, no difference between groups) is consistent with this theory. Given what appears to be inadequate analgesic administration, it is perhaps unsurprising that patients in both groups were suffering and agitated in the postanesthesia care unit.

The results reported by Park *et al.* are presented as a failure to validate SPI in children but instead appears to be a predictable consequence of their protocol. Thus, whether SPI is helpful in children remains unanswered.

Competing Interests

The author is a consultant at Medasense Ltd. (Ramat Yishai, Israel), a company currently developing a nociception monitor based on the Nociception Level index. The author receives a consultant fee and has company option shares.

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In Reply:

The efficacy of surgical pleth index (SPI) to guide the nociception–antinociception balance has been proven in study settings during general anesthesia rather than before anesthesia induction or during emergence from anesthesia. Considering that SPI only works well in anesthetized patients[1] and that operation time of adenotonsillectomy is usually short, the authors have designed the analgesic protocol of this study without preemptive analgesia so as to adequately verify the efficacy of SPI in children undergoing the surgery under general anesthesia. Preemptive analgesia given in a short procedure may provide over the necessary amount of