In Reply:

We appreciate the thoughtful comments engendered by the updated Guidelines for Perioperative Management of Patients with Obstructive Sleep Apnea (OSA).[1] As Dr. Corso *et al*. mention, there are numerous clinical methods for assessing the likelihood that a patient suffers from obstructive sleep apnea. Chung *et al*. demonstrated that the STOP and Berlin questionnaires as well as the checklist proposed in the American Society of Anesthesiologists Guidelines are all valid screening tools for OSA in surgical patients,[2] and no statistically significant differences regarding specificity and positive predictive values were found among the three screening questionnaires. While STOP and STOP-BANG scores were shown to correlate with the probability of sleep apnea, it was not established that they correlate with its severity.[3] Because we did not find clear evidence of improved benefit from using any one particular OSA screening questionnaire, the key consideration is obtaining information relevant to an assessment of the potential for sleep apnea from the patient or the patient's family.

The primary role of the "assessment of risk" checklist in the Guidelines is to encourage anesthesiologists and surgeons to consider the various factors which might predispose an individual patient to develop complications in the perioperative period. Patients with a score of 5 or 6 may be inappropriate candidates for outpatient surgery, and in some cases may warrant continuous monitoring of ventilatory function for several days postoperatively. Unfortunately, there is no guarantee that patients with a score of "4" will always have a favorable outcome following discharge to an unmonitored setting; residual anesthetics, sedatives, and opioid analgesics all increase the risk that a patient will fail to "self-resuscitate" from an obstructive episode. While there is no guarantee, observing patients while breathing room air in an unstimulated environment following emergence from anesthesia offers some degree of assurance that residual anesthetic effects will not contribute to an adverse outcome following discharge.

From a safety perspective, one could argue that *any patient* with even the slightest risk of sleep apnea should be monitored postoperatively in an acute care setting for several days, so that any episode of severe hypoxemia could be recognized and treated by trained personnel, as suggested by Dr. Rothfield. Such a requirement could mandate a significant increase in monitoring capabilities for hospitalized OSA patients, but also preclude the possibility of conducting outpatient surgery for many patients with suspected or confirmed OSA. While some hospitals have instituted continuous oximetry and capnography with central station monitoring for all postoperative patients diagnosed with OSA, others may be reluctant to do so because of cost and implementation issues. Until we have a reliable method of selecting the small fraction of OSA patients most likely to fail to "self-resuscitate" from a postoperative sleep apnea episode, we must either (1) admit all at-risk patients—even those who are undiagnosed or would normally qualify for outpatient surgery—to a monitored hospital setting for up to 3 days (because of rapid eye movement–rebound risk) or (2) accept the risk that in the absence of universal monitoring rare but serious postoperative complications are bound to occur.

The Guidelines state that, "An RCT indicates improved ventilatory function for OSA patients when postoperative CPAP is compared with no postoperative CPAP."[4] Dr. Roesslein correctly notes in his letter that this RCT did not compare "CPAP" with "no CPAP." Rather, it compared early (immediate) post-extubation CPAP with 30-min post-extubation initiation of CPAP, and respiratory measurements were not taken within the 30-min "non-CPAP" phase of the comparison group. Although information obtained from a few case reports[5–7] suggests that postoperative institution of CPAP may improve postoperative pulmonary function in OSA patients, we agree that more well-controlled studies are needed to correctly evaluate the beneficial effects of postoperative CPAP. On the basis of this limited information and our survey findings, the assessment of risk included in the Guidelines assigns a decreased risk of perioperative complications to OSA patients who are compliant with CPAP and will be using this modality during the postoperative period.

Competing Interests

The authors declare no competing interests.

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(Accepted for publication June 3, 2014.)

Single-injection and Continuous Femoral Nerve Blocks Are Associated with Different Risks of Falling

To the Editor:

I read with interest the superb retrospective database study of Memtsoudis et al. titled, "Inpatients falls after total knee arthroplasty: The role of anesthesia type and peripheral nerve blocks."[1] Of more than 190,000 patients who underwent total knee arthroplasty (TKA), there was a fall incidence of 1.6%. Twelve percent of all patients had a "peripheral nerve block" (PNB), yet, as stated in the abstract, "Contrary to common concerns, no association was found between PNB and IF [inpatient falls]." However, within the limitations section, the authors explain that, "The database used contains limited clinical information and thus some important factors cannot be taken into account… With regard to information concerning PNB, specific details on the exact type of block, if it was a continuous or single-shot application… are not readily discernible." In other words, it remains unknown how many—or even if any—of the nearly 23,000 patients with "PNB" had a *continuous* PNB.

This is a critical piece of (missing) information because the available data from previous studies that *were* able to differentiate between single-injection and continuous PNBs suggest a strong association of the latter with an increased risk of falls. In a retrospective database study, Wasserstein et al. found that—like Memtsoudis et al.—patients who underwent TKA with a *single*-injection femoral nerve block had the same risk of falling as patients without any type of PNB.[2] However, the presence of a *continuous* femoral nerve block increased the odds ratio of falling to 4.4 ($p = 0.04$). In a meta-analysis of three randomized, placebo-controlled trials involving femoral and posterior lumbar plexus catheters for TKA and total hip arthroplasty, respectively, Ilfeld et al. found that no subjects receiving perineural saline (n = 86) fell (0%) while there were seven falls (7%) in patients receiving perineural ropivacaine (n = 85; $p = 0.01$), strongly suggesting a causal relationship between the continuous blocks and falling.[3] Since there were no falls in the placebo group, an odds ratio cannot be calculated; but, if even just a single fall occurred in this group, the odds ratio would be 5.5 (therefore, the actual odds ratio is *at least* 5.5, but possibly higher). Finally, an additional meta-analysis including 4 randomized, controlled trials and one retrospective cohort study, Johnson et al. calculated an odds ratio of 3.9 ($p < 0.01$) of falling for subjects with a continuous femoral or posterior lumbar plexus block of greater than 12 h (incidence = 2.2%) compared with subjects with either no block, a single-injection block, or a perineural infusion of less than 12 h (incidence = 0.5%).[4] To my knowledge, there are no data contradicting these findings when single-injection and continuous PNBs are differentiated.

Why this apparent difference in the risk of falling exists between single-injection and continuous PNB remains unknown. Nevertheless, one may speculate that the reason single-injection blocks do not increase the risk of falling is simply because patients with flaccid quadriceps are not permitted out of bed—and do not attempt ambulation—without a good deal of caution and assistance. In contrast, patients with continuous PNB are not only permitted to get out of bed, but ambulate repeatedly in the early hours/days following surgery. Given that falls with 4-day continuous PNB occur not only in the two days following surgery, but postoperative days 3 and 4 as well—after patients have successfully ambulated multiple times during physical therapy—there is a high probability that patients become more confident and do not continue to take the same precautions as during early ambulation attempts.[3] To support this supposition, the majority of falls in patients with continuous PNB occur when patients are unaccompanied/unassisted and going to the restroom, often in the middle of the night.[5]

Therefore, while I agree with the authors' statement that their, "data should provide encouragement to not shy away from the use of PNB;" it should also not lull healthcare providers into a false sense of security regarding the risks of continuous PNB. Research involving the etiology of patient falls and their association with various regional analgesic interventions must not decrease due to the important findings reported in the recent study by Memtsoudis et al. In addition, this letter should not be construed as criticizing the study by Memtsoudis et al.—the authors accurately and responsibly identified the limitation of their analysis within their discussion section—but, rather, a caution to readers of their article. Practitioners should at least be aware of the data specific to *continuous* PNB, as decreased cognizance or even denial of the issue may only increase the potential risk to our patients.

Competing Interests

The author declares no competing interests.

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