Suture-method *versus* through-the-needle catheters for continuous popliteal-sciatic nerve blocks: A randomized clinical trial. Anesthesiology 2020; 132:854–66

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Multimodal Analgesia for Spine Surgery: Comment

To the Editor:

With great anticipation, we read the study by Maheshwari *et al.*[1] and congratulate the authors for a prominently featured article and infographic on the cover of Anesthesiology. We were interested to see that the results of the study suggest that multimodal analgesia, as administered in this protocol, did not benefit participants' postoperative quality of recovery, opioid consumption, or pain scores after spine surgery. However, with closer scrutiny, we found several elements of the experimental design that were, perhaps, not well-suited to properly explore the study aim and hypothesis in the context of the primary outcome measure. Surprisingly, the authors did not control for intraoperative analgesic strategies such as surgeon-administered epidural analgesia or local anesthetic wound infiltration. Even more concerning, the authors did not control for postoperative multimodal analgesic medications. Postoperative acetaminophen, gabapentin, tramadol, and ketorolac were not limited to the experimental group. In nearly identical numbers, both groups consumed the same postoperative analgesics for the first 48 h after surgery.

The authors found it surprising that the multimodal analgesic regimen proved ineffective in their patients. We politely disagree. Measuring the Quality of Recovery score 3 days after surgery, as well as other postsurgical pain measures, is confounded when both the experimental and control groups received multimodal analgesics during and after surgery. If the goal of the study was to examine the quality of recovery outcome 3 days after a single preoperative dose of acetaminophen and gabapentin with intraoperative ketamine and lidocaine infusions, then both groups should have received identical multimodal-free medication regimens after surgery. If, however, the objective was to examine multimodal *versus* opioid-only strategies, the control group should have been restricted from nonopioid analgesics during the pre-, intra-, and postoperative periods.

We recognize the challenge in designing studies that restrict therapies that are possibly beneficial to patients. However, we believe that the data presented in this study are inadequate to support the authors' conclusion that "this combination of four analgesics was not beneficial for patients having multilevel spine surgery." We agree with the authors that multimodal analgesia should be formally tested in each clinical context but disagree that the data presented here suggest a lack of benefit in spine surgery patients.

Competing Interests

Dr. Johnson has previously received financial support from the American Board of Anesthesiology (Raleigh, North Carolina), Applied Medical Visualizations (Salt Lake City, Utah), IARS – Anesthesia & Analgesia Editorial Board (San Francisco, California), and the American Society of Anesthesiologists (Schaumburg, Illinois); however, none of these funding sources had any direct conflict of interest pertaining to the content of this letter. The other authors declare no competing interests.

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Multimodal Analgesia for Spine Surgery: Reply

In Reply:

Meier *et al.*[1] contend that our trial poorly tested the effect of multimodal analgesia because we did not control intraoperative local anesthetic use or postoperative analgesia. Epidural analgesia is rarely used for spine surgery in our setting. As presented in Table 1, fewer than 1% of our patients had epidural analgesia.[2] Furthermore, local wound infiltration was used only in one quarter of the patients

in each group: analgesic pathway 39 (26%) *versus* 32 (22%) in placebo.[2] In a robust double-blind randomized trial (n = 299), there is no reason to expect substantive differences between treatment and placebo groups.

Meier *et al.* write that "measuring the Quality of Recovery score 3 days after surgery, as well as other postsurgical pain measures, is confounded when both the experimental and control groups received multimodal analgesics during and after surgery." Confounding—by definition—is restricted to factors that influence *both* exposure and outcome. Randomization usually prevents confounding; but in any case, an intervention *after* a blinded exposure cannot be a confounder. What Meier *et al.* presumably mean is that postrandomization treatments might influence outcomes. We agree, but the fact that patients in each group consumed nearly the same amounts of various analgesics during the initial postoperative days is not a limitation; instead, it confirms that the four combined treatments we tested are ineffective.

A reasonable question is whether Quality of Recovery 3 days after surgery is a suitable primary outcome. Granted, 3 days is distal to the interventions which were largely intraoperative. But Quality of Recovery is a well-validated, patient-centered outcome[3] focused on assessing patient pain and comfort level. Proponents of enhanced recovery or multimodal analgesic pathways presumably believe that their interventions noticeably improve outcomes that patients can detect. Clearly, the four drugs we tested did not. In any case, our predefined secondary outcomes were proximal, namely pain scores and opioid consumption, and neither improved.

Postoperative analgesia certainly matters, and presumably some approaches are better than others. We look forward to trials comparing various approaches, but evaluating postoperative analgesic technique was not our goal. We can, though, conclude that immediate perioperative use of acetaminophen, gabapentin, lidocaine, and ketamine—all of which act *via* separate pathways—contributes little. Future trials might therefore concentrate more on postoperative treatments or different analgesic regimens for perioperative analgesia.

Our results, while robustly equivocal, are nonetheless valuable since all drugs impose risk and cost and should therefore only be used if they are actually effective in a given context. We stand by our conclusion that "an analgesic pathway based on preoperative acetaminophen and gabapentin, combined with intraoperative infusions of lidocaine and ketamine, does not improve recovery in patients recovering from multilevel spine surgery."

Competing Interests

The authors declare no competing interests.

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Lung Ultrasound Training in the Critically Ill: Comment

To the Editor:

We read with interest the excellent study by Arbelot *et al.*,[1] evaluating the learning curve for focused, diagnostic lung ultrasound. The authors should be commended for their heroic effort to conduct a multicenter educational study at 10 intensive care units spanning three continents to address an important question that will inform future training guidelines. But although the study's results are broadly consistent with those of other related publications on this topic,[2,3] we take issue with one aspect of the study's methodology: the authors' unique classification of lung ultrasound pathology. Specifically, the authors asked learners to assign each lung ultrasound exam a score on a five-point scale "according to the worst parenchymal pattern" visible in the exam. The scores ranged from 1 for "normal aeration" to 5 for "lung consolidation." Although the authors' definitions for scores 1 and 5 conform to widely accepted norms,[4] their definitions for scores 2, 3, and 4 contain some irregularities.

The authors define these intermediate points as follows: 2 = "interstitial-alveolar syndrome"; 3 = "interstitial syndrome"; 4 = "pulmonary edema." For these three states, both the authors' numerical ordering and their proposed definitions are problematic for several reasons. First, in the lung ultrasound literature, the terms "interstitial-alveolar syndrome" and "interstitial syndrome" are often used