

the femoral nerve (anterior *versus* posterior) on quadriceps weakness and cutaneous sensory block. *Anesth Analg* 2012; 115:721–7

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In Reply:

Our goal was to determine whether electrical stimulation, either through the needle or catheter, reduces the risk of secondary failure after femoral nerve catheter insertion using ultrasound.¹ The results of our well-powered trial are clear: neither type of stimulation improved analgesia after total knee arthroplasty—and both stimulation techniques took longer to perform and cost more.

Dr. Chelly highlights the importance of providing analgesia sufficient for physical therapy. We used low concentrations of ropivacaine for our femoral catheter infusions specifically because ropivacaine is motor sparing compared with bupivacaine. Bupivacaine is less expensive than ropivacaine, as Chelly notes, and it is possible that a low concentration of bupivacaine would be equally suitable; however, we did not consider the choice of anesthetic or concentration, a topic that has been addressed in numerous previous studies.^{2–5} Our primary result—that stimulation through the needle or catheter is unnecessary—presumably applies perfectly well to ropivacaine.

Dysfunction of quadratus femoris muscle after total knee arthroplasty is mainly due to muscle edema,⁶ rather than the regional block.⁷ But to optimize patient safety, our Acute Pain Service team works closely with Nursing and Physical Therapy and reduces the ropivacaine infusion rate when patients develop weakness. In practice, the system works well and our patients participate actively in physical therapy. We agree that patient falls are dangerous and often disastrous. However, they are also far more likely to occur after discharge than in the hospital where patients are watched closely.^{8,9}

Chelly asserts that we failed to report the time required to perform blocks with each approach. In fact, the results are presented in the first paragraph under the header “Secondary Outcomes.” The mean block performance time was 177 s for the stimulating catheter group, 150 s for the stimulating needle group, and 110 s for the ultrasound-only group. Mean block performance time for the ultrasound-alone group was significantly less than the time for either other groups. But as specified in our article, these differences are not clinically meaningful.

Levine *et al.* postulate that our “simple” study has an excessively complex statistical analysis. However, the study was not simple. There were two primary outcomes (pain scores and opioid consumption) and three interventions to be compared: ultrasound-alone, stimulating needles, and stimulating catheters. Our analysis was the proper one for this research question and design, and no more complex than necessary. As specified in our article: “We evaluated these outcomes using joint hypothesis testing”¹⁰ because

differences in pain scores and opioid consumption are difficult to interpret in isolation. We thus considered a block approach superior only if it was shown to be noninferior on both pain and opioid consumption and superior on at least one of the two outcomes.”

Our sophisticated and appropriate analysis was a strength of this article: instead of choosing either pain score or opioid consumption as primary and relegating the other to secondary, as is sometimes done, we kept them both as primary because each is integral to pain management. We required superiority on at least one of them and evidence of being “not worse” (*i.e.*, noninferior) on the other before an intervention would be considered more effective than another—hence, our inclusion of both noninferiority and superiority testing. We thus used a powerful and appropriate approach to joint hypothesis testing.

We also powered the study appropriately and planned interim analyses so that the study could stop early, if warranted, for either efficacy or futility. We believe this should be the standard for most clinical trials, along with the graphical display of treatment effect over time that we reported for each comparison of interest.

We are nonetheless sympathetic to Levine’s broader concern that clinical research articles have become more statistically complex in recent years. Undoubtedly, inclusion of sophisticated statistical analyses makes clinical research harder to read and understand. But it is important to recognize that the statistical complexity in recent articles is not included for amusement of the authors. It is instead included to protect the anesthesia community from research conclusions that are subsequently contradicted¹¹ or simply wrong.¹²

Our primary outcome was secondary failure, defined by need for supplemental analgesia, as in previous studies.^{13,14} We did not evaluate sensory or motor block. The use of ultrasound is helpful in identifying the spread of local anesthetics in the proximity of the femoral nerve, but does not identify catheter position. Therefore, secondary failure remains an important problem with continuous femoral nerve blocks and was the basis for our study. We fail to understand how our study (or any study, for that matter) could “close the door to further novel research.” All we did was ask—and answer—an important question, namely whether stimulation augments the efficacy of ultrasound guidance for insertion of femoral nerve catheters.

Levine *et al.* note that some patients have residual pain in the sciatic distribution after knee arthroplasties. However, the femoral nerve provides 80% of the sensory supply to the knee joint. Consequently, only 12% of our patients required supplementation with single-injection sciatic blocks, and the blocks were evenly distributed among the three groups.

We agree and specified that a difference of just 1 min in block performance time, while statistically significant, is not clinically important. But the stimulating catheter not only took longer, it cost more. The \$50 difference—which

provided no clinical advantage whatsoever—is certainly clinically important. Based on our large, well-powered trial, we thus conclude that ultrasound guidance alone is generally sufficient for accurately positioning femoral nerve catheters.

Competing Interests

The authors declare no competing interests.

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Another Role of Limb Remote Ischemic Preconditioning in Patients with Lung Cancer

To the Editor:

Li *et al.*¹ have demonstrated that limb remote ischemic preconditioning (RIPC) attenuates lung injury after lung resection in patients without severe pulmonary disease under propofol–remifentanyl anesthesia. The authors concluded that lung reexpansion from the one-lung ventilation provoked severe oxidative injuries, which was shown by increased serum levels of an oxidative product malondialdehyde,² and that RIPC reduced the oxidative stress, resulting in improvement of pulmonary function after the lung resection.¹ However, I would like to suggest another role of RIPC in the decreased oxidative stress in the study. When I looked at figure 3C of the article, patients receiving RIPC showed a higher serum malondialdehyde level from the time point 2, which was before resuming two-lung ventilation, and the level did not further increase 30 min after reexpansion (the time point 3).¹ These results indicate that the mechanism other than reperfusion should have caused oxidative stress, resulting in increased levels of malondialdehyde. It is interesting to note that patients with lung cancer have a higher production of reactive oxygen species than that in the normal population and that manipulated lung tissue is a source of reactive oxygen species.^{3–5} In the study by Li *et al.*,¹ the postoperative morbidity rate for acute lung injury and adult respiratory distress syndrome was too high (8.2 to 17.5%) compared with that reported in previous studies (2.45%),⁶ indicating that their study population contained severe lung cancer patients, who were exposed to increased oxidative stress. Therefore, it is likely that RIPC can reduce oxidative stress induced by advanced lung cancer during the operation. I would await additional data regarding this issue from their further studies.

Competing Interests

The author declares no competing interests.

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