


(Benefits of Adding Sciatic Nerve Block to Femoral Nerve Block for Total Knee Arthroplasty

To the Editor:

Recently, Abdallah et al.1 reported the results of a double-blind, placebo-controlled, randomized trial that demonstrated the additional analgesic benefit of adding to a continuous femoral block, either a proximal (infragluteal) or a distal (popliteal) sciatic nerve block. There are a number of points in this article, which I would like to discuss. Contrary to the authors’ claim, this is not the first article to demonstrate that sciatic block makes an important contribution to continuous femoral block for analgesia after total knee arthroplasty (TKA). For example, they cite the study by Ben-David et al.2 who showed this quite convincingly more than 10 yr ago. In that study, the authors found that approximately 80% of patients, similar to the finding of Abdallah et al., have significant pain without the addition of a sciatic block. The following year, Pham Dang et al.3 conducted a randomized trial to confirm the finding of Ben-David et al. This recent article corroborates those decade old findings. Second, the failure of single-shot blocks to provide adequate duration of analgesia after TKA has been demonstrated previously.4

Furthermore, other important issues warrant consideration. First, a preoperative single-shot sciatic block precludes distinguishing a postoperative foot drop as being due to the surgery (thus requiring action to be taken) as opposed to the block. For this reason, we recommend placing the continuous sciatic catheter with only saline and infusing local anesthetic only once intact sciatic function is demonstrated postoperatively. Second, a popliteal approach to sciatic block for TKA surgery is problematic, because it leaves any sciatic branch of the sciatic nerve injured because of the surgery.—with obvious medicolegal implications. A more proximal block of the...
sciatic nerve, such as with a gluteal approach, allows greater discrimination between a sciatic nerve injury because of the block as opposed to the tourniquet or the surgery.

Competing Interests
The author declares no competing interests.

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(Received for publication March 5, 2015.)

In Reply:
We thank Dr. Merman for her comments regarding the novelty of our study, the duration of analgesia provided by a single-shot sciatic block, and the safety concerns associated with using a distal sciatic block in the setting of knee arthroplasty.

Although the work published in 2004 by Ben-David et al.2 may signal a benefit to sciatic block in treating posterior knee pain after knee arthroplasty, any conclusions drawn from this study are significantly undermined by its observational design and limited sample size of only 12 patients. In the 2005 randomized trial by Pham Dang et al.,3 neither the patients nor the assessors were blinded, and the authors did not specifically examine the effect of sciatic block on posterior knee pain. Therefore, neither of these two earlier studies can be considered definitive.

We agree with Dr. Merman that a continuous catheter-based perineural infusion can prolong the duration of analgesia associated with sciatic nerve blockade; however, the clinical importance of prolonged sensory blockade may be offset by a delay in mobilization, a critical requirement in the contemporary clinical pathways that emphasize early ambulation.

Finally, we aimed to definitively quantify the analgesic benefits of sciatic nerve block after knee arthroplasty, and our results suggest that both proximal and distal sciatic nerve blockade similarly improve analgesic outcomes. Our study was not sufficiently powered to demonstrate differences in the rate of block-related nerve injury. Although Dr. Merman’s comments regarding the safety of tourniquet use in the immediate vicinity of a perineural injection around the popliteal sciatic nerve may seem reasonable, these concerns remain speculative.

Competing Interests
The authors declare no competing interests.

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(Received for publication March 5, 2015.)

Propofol-induced Electroencephalogram Dynamics: A Missing Piece

To the Editor:
We want to congratulate Akeju et al.1 for their interesting work on the electroencephalographic dynamics of propofol- and dexmedetomidine-induced loss of consciousness (LOC). Nonetheless, we feel that some details should be added in order to apply the provided information to the clinical practice.

The authors used an effect-site (ES) target-controlled infusion (TCI) of propofol starting with a target concentration of 1 μg/ml up to 5 μg/ml and staying 14 min in each target. However, they missed referring which pharmacokinetic model was used to calculate the ES concentrations and to drive the propofol infusion. Some authors used a similar approach in another study2 to induce LOC with propofol, where probably the Schnider model3,4 was used and presumably LOC occurred at 2 μg/ml, which seems to be a very low ES concentration to induce LOC.5–7

From a pharmacokinetic/pharmacodynamic point of view, it would be interesting to correlate the electroencephalographic