and C7 nerve roots, the dorsal scapular nerve branches from the C5 nerve root, while the nerve to the subclavius branches near the junction of C5 and C6. How can we be certain that the divisions that Dr. Narouze and Dr. Filipe refer to are not branch points for these nerves? Whether these are divisions within the internal architecture of the cervical roots; or the different combinations of roots, trunks, or divisions of the brachial plexus; or branch points for other nerves that accounts for the 3 round hypoechoic structures commonly seen when performing an interscalene nerve block, further research is needed to definitively understand the sonoanatomy of the brachial plexus block.

John Antonakakis, MD
Department of Anesthesiology
University of Virginia Medical Center
Charlottesville, VA

REFERENCES

Reply to Dr. Borgeat
Accepted for Publication: 20 July 2009

To the Editor:

We would like to thank our colleagues for their observations regarding our recent article. For clarity, we will respond in the same order as they listed their comments:

First, whether a specific surgical procedure may be described as “moderately painful surgery” depends on the definition of “moderate.” In the United States, the shoulder surgery procedures included in our study are performed—nearly without exception—in the outpatient setting, with most institutions providing exclusively oral analgesics (after, at most, a single-injection brachial plexus block). If a procedure is commonly performed with solely oral analgesics provided and is defined as “very severe,” then how do we classify surgical procedures that require intravenous opioids? In 3 previous randomized, double-masked, controlled studies involving ambulatory perineural interscalene ropivacaine infusion including nearly identical surgical procedures, we described the procedures as “moderately painful,” without comment from manuscript reviewers or journal editors. Conversely, we believe that rotator cuff repair can result in pain legitimately described as “severe” and do not disagree with our colleagues if they would choose to describe the pain resulting from these procedures as such. However, we do not believe the issue of how to describe pain that results from a specific surgical procedure has much to do with our study under discussion or its hypothesis; therefore, we are unclear why it warrants comment in a letter-to-the-editor.

Second, we do not understand why our colleagues believe “the choice of an electric pump is not satisfactory regarding patient’s comfort.” Elastomeric pumps offer the advantage of independency from electric current or batteries and also offer the possibility of supplementary bolus.” Electronic and elastomeric infusion pumps certainly have various strengths and weaknesses, but following multiple studies using various electronic infusion pumps (and thousands of treated patients), we have not found their statement to be accurate; nor do we believe there are any data to support our colleagues’ assertion that patients’ subjective report of “pain is more reliable than a neurological self-testing in [the] ambulatory setting.” In fact, we speculate that sensation (binary response: can or cannot feel someone else touching an extremity) is perhaps a less subjective end point than reported pain, which may make it a more reliable end point.

Fourth, there is no difference in reliability between the end point sleep disturbance and any other end point as there are always many contributing factors that affect each end point. It is for this reason that the randomized study design is the criterion standard: factors other than those specifically under investigation are randomly assigned to each of the treatment groups, presumably equalizing the effect of extraneous/confounding factors (both identified and unidentified) on the end points under investigation. The influences that our colleagues list—“snoring partner, babies in the same household, etc.”—potentially affect not only the sleep disturbance outcome measure, but every other measure as well. Our colleagues are correct in their assessment that there are “many confounding” factors influencing the sleeping end point, but are incorrect in their assertion that for this reason the sleep disturbance variable is any less “reliable” an end point than any of the other end points included in the study. Importantly, this was a randomized study, and therefore, the confounding factors should be minimized on all outcome variables (at least, minimized to the maximum extent possible with a prospective clinical trial).
Fifth, we certainly concur that there is an association between the lowest current eliciting muscular contraction and needle/catheter distance. However, we are unclear as to why our colleagues feel our choice of a needle-current end point of 0.3 to 0.7 would confound the results in any way. As described in the Methods section, all catheters were placed before randomization, and as just described in the previous paragraph, the power of randomizing subjects is that all variables have an equal chance of distribution to each treatment group. And, this was certainly the case in our study: as listed in Table 2, the minimum current via the needle and catheter differed between treatment groups by only 0.01 mA each (0.54 vs 0.53 and 0.55 vs 0.56 mA, respectively). We thus disagree with our colleagues that “…the range of stimulation current from 0.3 to 0.7 mA is too large to allow valid comparison between [sic] patients.”

Sixth, we agree with our colleagues regarding the importance of registering bolus applications and explicitly noted this in our discussion section, “although each patient-controlled bolus dose delivered the same ropivacaine dose for both treatment groups (8 mg available every 30 mins), the actual delivered doses for each group are unavailable. Therefore, it is possible that patients assigned to one of the treatment groups self-administered a greater number of bolus doses resulting in a higher total dose of delivered ropivacaine. This methodological weakness decreases confidence in our results. Future investigation is required that corrects for both the limitations on the primary end point external validity (incidence of an insensitive limb) and the lack of total local anesthetic delivered in each treatment group (including patient-controlled bolus doses).” However, we also noted that “even given the weaknesses of this study, the results are of clinical use: if practitioners desire maximizing analgesia and patient satisfaction during continuous interscalene nerve blocks, this study provides valid information to help achieve these goals. In other words, regardless of the total amount of local anesthetic delivered to each treatment group, providing a 0.2% ropivacaine infusion at a basal rate of 8 mL/hr resulted in improved analgesia and patient satisfaction relative to a 0.4% ropivacaine infusion at a basal rate of 4 mL/hr.”

Seventh, waiting for a complete resolution of a surgical block to begin the peripheral local anesthetic infusion will result in hours of significant pain—at the very time that we want to provide maximum analgesia. Waiting for partial resolution of the surgical block in an attempt to decrease the duration and intensity of this analgesic deficit will then not guarantee identification of all nerve injuries, because most result in partial rather than complete deficits. Given this risk-benefit ratio, it is understandable that most publications (>95%) involving postoperative perineural local anesthetic infusions use the technique we described to avoid a period without analgesia. We are surprised by our colleagues’ suggestion that they recommend otherwise because they themselves report using this same technique in nearly all of their publications involving an initial preoperative bolus of local anesthetic (usually ropivacaine or bupivacaine) followed within 6 hrs by an infusion of local anesthetic (always ropivacaine or bupivacaine).12-17

Lastly, we agree with our colleagues that a similar investigation should be undertaken in an inpatient setting to add to the validity and reliability of resulting data, and we have recently completed precisely such a study. The articles publication is imminent!18

Brian M. Ilfeld, MD, MS
(Clinical Investigation)
University of California San Diego
San Diego, CA

Linda T. Le, MD
University of Florida
Gainesville, FL

Vanessa J. Loland, MD
Edward R. Mariano, MD, MAS
(Clinical Investigation)
University of California San Diego
San Diego, CA

J.C. Gerancher, MD
Wake Forest Medical Center
Winston-Salem, NC

Daniel I. Sessler, MD
The Cleveland Clinic
Cleveland, OH

REFERENCES

© 2010 Lippincott Williams & Wilkins
We would like to thank Drs. Fitzpatrick and Dolan for bringing attention to the use of peripheral nerve stimulation in patients with permanent pacemakers. Because there is no clear consensus in the anesthesia literature on the safe use of nerve stimulation in patients with permanent cardiac pacemakers, or any implantable electrical device, our recent brief review article focuses on the inherent dangers of nerve stimulator use, existing evidence of untoward events, and available guidance for its application in pacemaker patients. Indeed, the use of ultrasound guidance always for performing nerve blocks is feasible in trained hands and increasingly popular with better appreciation of sonoanatomy and sonopathy. A preprocedure systematic sonographic survey can help to identify nerves and distinguish these from nonneural structures. However, we would emphasize that, at present, much of ultrasound-guided regional anesthesia relies on pattern recognition. That is, anesthesiologists, novices and experts alike, routinely rely on eliciting sonographic images with landmark structures in their expected locations and telltale tissues with characteristic echogenicity. Pattern recognition is only as good as the reliability of the pattern, and as we all continue to gain more scanning experience, we realize that the “textbook” pattern of nerve locations is highly variable. Therefore, we continue to preach and practice the use of low-current peripheral nerve stimulation for objective confirmation of nerve identification once nerve localization is achieved using ultrasound guidance.

Baskar P. Manickam, MD, FRCA
Richard Brull, MD, FRCPC
University of Toronto, Canada

REFERENCES