LETTERS

Methodological Weaknesses of Block Study Make Analyzing Results Very Difficult

To the editor: We were interested to read the article by Roussel and Thirkannad regarding brachial plexus block for cubital tunnel release. Initially, we were intrigued by the use of the axillary block for surgeries at or distal to the elbow and its effectiveness generally used for surgeries at or distal to the elbow and its effectiveness is questionable for more proximal surgeries. The authors’ Figure 1 shows a very proximal dissection, and despite the reported results, we continue to question the effectiveness of an axillary block for such proximal procedures of the arm.

Furthermore, the authors did not clearly define the surgical procedures being performed in these 120 patients. “Cubital tunnel release procedures” can refer to any of several distinct surgeries, including simple decompression, medial epicondylectomy, or direct release with or without an anterior subcutaneous or submuscular transposition. A medial epicondylectomy or submuscular transposition may require more profound blockade than a simple decompression or a subcutaneous transposition, both of which can be done under local anesthetic injected at the operative site by the surgeon. While the intercostobrachial nerve contributes to complete anesthesia of the arm for some surgical procedures, the block failures noted in the study, as we previously stated, may instead be due to a poorly chosen block for the surgery performed. In addition, failure of surgical blockade of the brachial plexus, defined as any need for supplemental systemic analgesia or local anesthetic injection, has been cited as 11%-13% in a recent large, systematic review and at least some of the failures noted in the study may be due to the inherent failure rate for certain approaches to the brachial plexus.

Finally, the authors’ conclusion that reliably blocking the intercostobrachial nerve is more important than the particular approach to blocking the brachial plexus is not supported by the data provided, especially since no test of statistical significance is anywhere cited. Instead, their assertion appears to rely completely on casual inspection of Figure 4, which omits even the raw number of subjects in the 3 BMI categories, which were presumably not evenly distributed. Although the failure rate for each of the 3 block approaches (axillary, infraclavicular, and supraclavicular) is described for the initial cohort of 90 subjects, it is not described for the subsequent cohort of 30 subjects, making comparisons impossible.

This retrospective study set out to determine if infraclavicular and supraclavicular blocks were superior to axillary blocks for cubital tunnel release and evolved into a prospective study of intercostobrachial local anesthetic volumes. This study, as presented, would best be viewed as a preliminary data supporting the need for a properly designed randomized controlled trial. In order to better determine the contribution of the intercostobrachial nerve, such a trial would standardize the procedure being performed, randomly assign subjects to a nerve block approach or choose a single block technique appropriate for all the surgeries being studied, and be mindful of the known failure rates of these techniques. Only then could the contribution of the intercostobrachial nerve be reliably determined.

REFERENCES


Roland A. Flores, Jr, MD
Neil Bailard, MD
Raja Palvadi, MD
Houston, Texas

Response: We would like to thank Drs Flores, Bailard, and Palvadi for their careful reading and attention to our observational case series. We were also intrigued by the use of the axillary block for cubital tunnel procedures extending up the medial aspect of the arm, and this was one of the reasons we reported on this. The axillary block can be used for these surgeries. The incision, although proximal, does not extend near the shoulder or shoulder joint. The authors rightly point out that cubital tunnel release and transposi-
tion incisions can vary in depth and length. They mention epicondylectomy as part of these procedures; however, that is a separately consented procedure and thus would be excluded from our case series. Regardless of the depth or extent of the incisions for cubital tunnel procedures at our institution, all nerves to these surgically manipulated tissues can be reliably blocked with any of the 3 techniques described with the exception of the intercostobrachial nerve. The cubital tunnel incision starts proximal to the elbow and extends just distal to it, so the axillary block can also be used. The authors suggest that the axillary block, as a poor choice for these surgeries, led to some of the failures. Although our hypothesis suggested this, the failures in the first cohort of 30 patients in each group showed the same number of failures in the infraclavicular group and a higher number in the supraclavicular group compared to the axillary group. Since all blocks were tested for complete blockade of the radial, median, ulnar, and musculocutaneous nerves and recorded as completely blocked prior to operating room entry, all blocks were properly performed. Therefore, it is not logical to state that axillary blocks are not appropriate for these types of surgeries.

The authors are right in that statistical significance was not presented. Since this was a case series, we decided to present the data using descriptive statistics only. We tried not to present conclusions as statistically significant but rather as observations as one would do with a case report. We were careful to point this out, but perhaps we could have clarified this more in the article. The fact remains that in a cohort of 90 patients, only 3 of them required more than sedation following the block. In the patients who required local anesthetic supplementation, a small volume was injected cutaneously only and always proximally. From direct observation, all 3 blocks appeared to be appropriate for these surgeries, and it also appeared that cutaneous blockade was more problematic than block selection. We also agree that since the second cohort was smaller, it is difficult to compare them to the first cohort. This was not part of the original design but rather a response to clinical observations. A larger cohort would have been beneficial.

A randomized, prospective study would be more conclusive, which we stated in the article. A better indicator, as the authors suggest, would have been to randomize block selection, have one anesthesia provider perform all of the blocks uniformly, and then have 1 surgeon perform the same surgery with the same incision each time. We considered performing this as a study as opposed to an observational case series. However, the base objective was not to produce a study for academic purposes but rather to assess and improve our performance. We are a small group in private practice, and we wanted to improve care to our patients while improving surgical conditions for all of our surgeons whom we regard as our customers. Some anesthesia providers prefer not to administer infraclavicular or supraclavicular blocks, and having them do so under requirement of randomization is not comfortable for them, is potentially harmful to the patient, and would not (or should not) receive IRB approval.

We remain convinced that our issue was not the block chosen but the reliable blocking of the cutaneous branches in the proximal arm. We suggest that it is the intercostobrachial nerve because the other cutaneous nerves can be blocked by any of the 3 blocks chosen. We look forward to any studies that help to provide solutions and particularly articles that use ultrasound to describe methods to block reliably the cutaneous branches.

REFERENCES

John Roussel, CRNA, MS
Louisville, Kentucky