Accurate Placement of Ultrasound-Guided Lateral Popliteal-Sciatic Perineural Catheters

Accepted for Publication: 8 October 2010

To the Editor:

I read with interest the article by Mariano et al1 describing the placement of ultrasound-guided perineural catheters for continuous lateral popliteal-sciatic nerve block. Catheters were initially advanced 5 cm beyond the needle tip and gradually withdrawn to a position inferred by the injection of air boluses. However, the positive or negative predictive values of this test remain unknown. A suboptimal location of the ultrasound-guided catheters in this study may be suggested by mean postoperative pain scores of 4.5 (range, 0–7.1), which are significantly higher than those in other studies using neurostimulation to place continuous popliteal-sciatic catheters for pain control.2 Their relatively superficial location and transverse trajectory ensure that long-axis ultrasound images of perineural catheters are readily obtained during the lateral approach to the popliteal-sciatic nerve (Fig. 1). Catheters can be continually visualized, and the spread of local anesthetic around the sciatic nerve can also be confirmed.3 Sonographic placement of perineural catheters may offer a number of advantages compared to landmark-based neurostimulation techniques alone.4 However, significant improvement in postoperative pain scores may be difficult to confirm if, as in the study by Mariano et al,1 ultrasoundography is used to simply infer catheter position rather than to view the catheter in real time and to confirm precise placement of the catheter tip and spread of local anesthetic within the perineural sheath.

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FIGURE 1. Ultrasound-guided lateral popliteal-sciatic catheter. The 3 arrows indicate the perineural catheter. LA indicates local anesthetic; LAT, lateral; MED, medial; SN, sciatic nerve.

Ultrasound-Guided Peripheral Nerve Block in a Patient With Neurofibromatosis

Accepted for Publication: 16 June 2010

To the Editor:

We would like to congratulate Dr. Sites and his team for a thorough and interesting discussion of clinical sonopathology.1,2 Although they include neurofibromas in their discussion of neural tumors, a sonographic example is lacking. We would like to contribute our specific sonographic experience administering a peripheral nerve block to a patient with neurofibromatosis (NF).

Neurofibromatosis is an autosomal dominant neurologic disorder characterized by neurofibromas and hyperpigmented (café au lait) spots of the skin. Type 1 NF, von Recklinghausen disease, is the most common form and is characterized by cutaneous, nodular (peripheral nerve), and paraspinal neurofibromas.3 The presence of nodular neurofibromas poses a unique challenge to the performance of peripheral nerve blockade.4

Our patient was a 47-year-old woman with Type 1 NF who had a history significant for postoperative nausea and vomiting after previous general anesthetics. She presented for excision of a painful lesion in her left lateral foot and ankle and requested to have a regional anesthetic. Ultrasound allowed the placement of a sciatic nerve block in the popliteal fossa without concerns of an intraneural injection. The nerve was identified and appeared to have hypoechoic structures resembling vessels within it. Neither were these structures compressible nor did they have evidence of blood flowing through them when Doppler was applied. In the absence of direct visualization or pathologic section, we could only presume that the hypoechoic areas may represent neurofibromas (Fig. 1). Our observations are consistent with those of Reynolds et al5 who sought to describe the sonographic characteristics of peripheral nerve sheath tumors. They note that such tumors are often hypoechoic, with posterior acoustic enhancement and thus may simulate a ganglion cyst. Beggs, too, describes neurofibromas as well-defined solid hypoechoic masses that have faint distal acoustic enhancement. Occasionally, a coarse echotexture or discrete focal area may be
appreciated within the lesion, which is secondary to collagen deposits. Thirty milliliters of mepivacaine 1.5% was injected, and the patient underwent an uneventful procedure with minimal intravenous sedation and no narcotics. She did not experience postoperative neurologic complications.

The case highlights the utility of ultrasound in placement of peripheral nerve blocks in NF patients and presents the associated sonographic findings.

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FIGURE 1. Sciatic nerve in popliteal fossa. Arrows highlight the intraneural fibromas.

Continuous Femoral Nerve Block Under Ultrasound Guidance: Perineural Opening Before Catheter Placement Is Perhaps No Longer Necessary

Accepted for Publication: 24 September 2010

To the Editor:

D r. Ficarrotta et al1 have made an extraordinary effort in clarifying the need for perineural space before expansion before catheter placement for continuous femoral nerve block (CFNB).2 We applaud their complex achievement in eliminating bias, which could have influenced our previously reported data.2 We accept their evidence-based conclusion that dilating the perineural space is not necessary to place a catheter around the femoral nerve.

We wish to add 2 important points. Their results were obtained under ultrasound guidance, which itself constitutes a bias. Also, they used a different material for CFNB (Stimucath is more rigid than Stimulong). This makes it more difficult to compare our work with theirs. Furthermore, ultrasound imaging shows that injectates deposited around the femoral nerve expand the perineural space between the nerve and the fascia iliaca, a condition of success either for a single-shot block or for a catheter placement. Ultrasound imaging also shows that, in an out-of-plane approach (did Dr. Ficarrotta et al use this in their study?), the Tuohy needle can be placed nearer the nerve, beneath the fascia iliaca, which leads to similar success. Thus, ultrasound imaging is a visual alternative accurate enough to partly replace dextrose 5% in water, which we used in our blind anatomic techniques for anatomic expansion and for maintaining accurate electroloration of the nerve.2,3 We further suggest that the conclusion that perineural opening is unnecessary has to be viewed also from the perspective of anesthesiologists to whom an ultrasound machine is not yet available. In summary, opening the perineural space may be necessary in blind techniques as shown in our study2 but is unnecessary when ultrasound guidance is used.1 The coming questions posed by ultrasound imaging for CFNB should be what types of motor responses would we accept from the stimulating catheter? Only quadriiceps contraction as the authors suggested?1 Is electrostimulation of the nerve from the catheter the sole end point or should it be combined with other end points, such as (to cite only one) image of perineural expansion after injection via the catheter? Such a combination of several end points could perhaps help us understand the failed blocks encountered by the authors.1

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Injection of Injectates Is More Than Just for “Opening the Perineural Space”

Accepted for Publication: 6 October 2010

To the Editor:

W e read with interest the recent article by Ficarrotta et al,1 in which they conclude that the concept of “opening the space” surrounding the femoral nerve with 5% dextrose (D5W) before the perineural placement of stimulating femoral catheters does not add value, in particular to the catheter threading time and number...
Letters to the Editor

Regional Anesthesia and Pain Medicine • Volume 36, Number 1, January-February 2011

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Ultrasound Refraction Artifact Is Unlikely
A Response to Duplication of Brachial Plexus

Accepted for Publication: 22 October 2010

To the Editor:

We read with interest the letter by Saranteas and colleagues1 reporting 2 brachial plexus duplication artifacts visualized using ultrasound. Two quality images were published to illustrate an image duplication of the divisions of the plexus and the C5 nerve root. The authors attributed these duplications to refraction of ultrasound beams similar to previously published reports of deep structures (aorta, kidney) duplicated within the abdomen using ultrasound visualization.2,3

On initial evaluation of the published images, we agree that the structures highlighted appear quite similar. However, upon further inspection, we disagree with the authors’ explanation that these structures were duplications resulting from ultrasound beam refraction within the human body.

To analyze these images, we first used Snell’s law of refraction, which describes the relationship between the angle of incidence and refraction when referring to acoustic waves passing through an interface between 2 different media. We calculated the expected refraction of the ultrasound beam based on the speed of sound values provided by the authors, that is, 1540 m/sec (assumed speed of sound in soft tissue) and 1450 m/sec (speed of sound in adipose tissue). If we assume a generous angle of incidence of 30 degrees, then the maximum angle between the refracted and incident beam to be approximately 2 degrees (Fig. 1). This small angle would result in the duplicated neural structures being much closer to the actual neural structures than depicted in the authors’ images. In fact, Figures 2A and B demonstrate that the smallest angle of refraction that justifies the lateral displacement for each image is 30 and 60 degrees, respectively. Furthermore, even if we use higher ultrasound velocities in biologic media (1620 m/sec), the amount of refraction does not increase significantly.

Therefore, we conclude that the authors’ explanation regarding refraction artifacts needs to be reconsidered. A

![Figure 1](image-url)
thorough investigation similar to the in vivo or in vitro studies referenced by the authors should be undertaken when potential artifacts are discovered. In this case, we would first encourage the evaluation of the ultrasound by the manufacturer or other appropriate servicing department. Also, other anechoic structures can appear similar to the C5 nerve root in this area including other nerve roots (eg, C4 or C6) and blood vessels (eg, transverse cervical artery). In addition, the apparent duplication in Figure 2C could result from the presence of an anatomic anomaly of C5. These other structures should be included in the differential.

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We too have found it to be an excellent educational tool for ultrasound-guided neuraxial blockade.

In our experience, the greatest difficulty in preparing the carcass relates to instillation and retention of fluid in the intrathecal space. This, as the authors have pointed out, is essential if good images are to be obtained.

We have found the following steps helpful in this regard. First, a 3- to 4-cm length of spinal cord must be exposed with its covering meninges intact (Fig. 1B). This requires disarticulation and removal of the uppermost cervical vertebra. This can be difficult, especially at the facet joints; we find a chisel and a stout pair of bone cutters or similar instrument to be helpful here. At the same time, it must be done carefully to avoid tearing the dural sleeve around the spinal nerve roots, which would lead to subsequent leakage of fluid from the thecal sac.

Next, the proximal end of the spinal cord is ligated with a 2-0 silk suture to create a watertight seal. A small piece of latex (cut from a disposable glove) is placed under the ligature to prevent it from cutting through the meninges as it is pulled tightly (Fig. 2).

Finally, a 22-gauge intravenous cannula is inserted carefully through the dura to lie in the intrathecal space (Fig. 2). A colored saline solution may then be injected or infused through the cannula. Placing the carcass in a slight, reverse Trendelenburg position facilitates filling of the lumbar intrathecal space. We have found that 10 to 15 mL of fluid is adequate to fill the thecal sac and provide good ultrasonographic images (Figs. 3 and 4).

Owing to the thickness of porcine skin, a 20-gauge or larger needle with a Quincke or Tuohy tip is recommended when practicing ultrasound-guided spinal and epidural needling techniques on the phantom. Needle tip placement in the epidural or intrathecal space can be readily visualized (Fig. 4 B; see Video, Supplemental Digital Content 1, http://links.lww.com/AAP/A25); however, we have found that aspiration of fluid from the lumbar intrathecal space is usually possible only if an additional 5 mL is simultaneously injected through the cannula at the cephalad end of the thecal sac.

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Investigation of Puncture Angles Needs Clinically Defined Insertion Points

Accepted for Publication: 10 August 2010

To the Editor:

With great interest, we read the article by Grasu et al investigating 289 magnetic resonance images (MRIs) of the thigh to give biometric data for the distal lateral approach to the sciatic nerve.

The results of the study confirm our own results published in 2005, in which we investigated similar biometric data for lateral sciatic approaches at different sites. In this study, we already defined some of the present parameters on 246 MRIs of the thigh.

In contrast to Grasu et al, we marked our potential needle insertion point in volunteers before the magnetic resonance imaging to get a realistic view of clinical practice. The reference for the puncture point was the border of the vastus lateralis muscle of the thigh.

Approximately 12 cm proximal of the joint line of the knee, we found a “sciatic nerve-to-femur” distance of 2.5 ± 0.4 cm and a “skin-to-nerve” distance of 5.2 ± 0.6 cm in 82 MRIs. The depth of the nerve was 39% of the diameter of the thigh.

A puncture angle of 10.9 ± 7.4 degrees was determined in our study, which was in contrast to the results of Grasu et al, who determined an angle of 30 ± 8 degrees. According to Grasu et al, we found a wide range of insertion angles—in our study, 5 to 29 degrees.

The explanation of the different angles determined in the 2 studies could be that Grasu et al measured the angle from a retrospectively assessed virtual needle insertion point defined by a horizontal, image-marked femur-to-skin line.
crossing the skin surface. Our prospectively determined and clinically used insertion point of the puncture needle is regularly more dorsal, so that the angle to the nerve gets smaller.

Also, our needle insertion point is 3 cm proximal compared with that of Grasu et al. Three cm to proximal, the vastus lateralis muscle proceeds to dorsal because of its anatomical configuration whereas the sciatic nerve holds its position in the depth (we found a similar sciatic to nerve distance over the whole femur distance). In confirmation of this, we found the smallest angle of 8 degrees in the middle of the thigh where the muscle border is at its most dorsal point. In conclusion, at the distal lateral approach to the sciatic nerve, the puncture angle increases with approximating the puncture point to the joint line of the knee, using the border of the vastus lateralis muscle of the thigh as reference.

One must define a clinically used needle insertion point to get exact information about the needed insertion angle in clinical practice.

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Specialist’s Knowledge of Local Anesthetic Systemic Toxicity

Accepted for Publication: 23 July 2010

To the Editor:

We read with great interest the excellent series of articles on local anesthetic systemic toxicity (LAST) that were featured in the March-April issue. Awareness of this much feared complication is high among anesthetists, particularly those who regularly perform regional anesthesia. However, there are physicians in other specialties who regularly use local anesthetic in clinical practice such as plastic surgeons and dermatologists. All practitioners using local anesthetic are responsible for knowing the maximum dosage, recognizing the signs and symptoms of toxicity, and being able to manage toxicity should it arise. Because many cases of local anesthetic toxicity arise not from excessive doses but from inadvertent intravascular injection, it is conceivable that even practitioners using lower volumes of local anesthetic solution may provoke toxicity. With many dermatologists working in a predominantly nonacute setting, more specialist assistance may not be immediately available from anesthetic or acute physician colleagues; the need for recognition of the early signs of toxicity and the prompt institution of early management is essential.

We recently conducted a survey of dermatologists in the United Kingdom and Ireland to determine the knowledge of LAST with some interesting results. In total, 92 dermatologists (66% of whom were consultants) responded to our online survey. We found that 61% of respondents knew the maximum safe dose of lignocaine in milligrams per kilogram. We presented 2 clinical vignettes of patients of different age and weight presenting for excision of skin lesions and asked respondents to choose the maximum volume of lignocaine that could be safely used to anesthetize the area. Just more than half (53%) chose the correct answer. Significantly, 47% chose volumes of lignocaine that exceeded the maximum safe dose of local anesthetic. Knowledge of symptoms of local anesthetic toxicity was high among dermatologists in our survey, but just 19 (21%) of 92 were aware of the place of lipid emulsion in the management of LAST.

Our results show that dermatologists are cognizant of the signs and symptoms of LAST, but a worrying percentage are not sure of the correct maximum safe doses of lignocaine that can be used for skin infiltration. We applaud the authors on a most informative review series, which will be of use to a wide audience as demonstrated by the results of our survey.

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Reply to Drs. Walsh, Moran and Walsh

Accepted for Publication: 26 July 2010

To the Editor:

On behalf of the American Society of Regional Anesthesia and Pain Medicine’s (ASRA) practice advisory panel on local anesthetic systemic toxicity (LAST), I wish to thank Drs. Walsh and Walsh for sharing their insights and the results of their survey of United Kingdom and Irish dermatologists’ knowledge regarding LAST. Their correspondence makes several important points that are worthy of reemphasis. First, as the survey confirms, local anesthetics are widely used by a variety of medical specialists who do not always possess complete knowledge of potential complications or contemporary therapeutic options. Like some members of the anesthesiology community itself, nonanesthesiologists can be unaware that weight and body mass index are not reliable metrics from which to calculate local anesthetic maximum dose or that propensity for LAST varies by age and medical comorbidity.

When the ASRA panel began this project, it solicited input from a number of US organizations that represent physicians who routinely use local anesthetics in potentially toxic doses. Although the American Society of Plastic Surgeons was on that list, we unfortunately neglected to solicit input from the American Academy of Dermatology. The summary article from the Practice Advisory1 has been shared subsequently with all of these professional
societies, with permission given to distribute the 2-page summary checklist to their membership. Both the summary article and the checklist are available for free download at www.asra.com/publications. Anesthesiologists worldwide are encouraged to share this information (or similar guidelines from the Association of Anaesthetists of Great Britain and Ireland) with their nonanesthesiologist colleagues.

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An Unsubstantiated Condemnation of Intraneural Injection

Accepted for Publication: August 6, 2010.

To the Editor:

We have read the editorial by Drs. Neal and Wedel in the July–August 2010 edition of Regional Anesthesia and Pain Medicine. We disagree with some of Drs. Neal’s and Wedel’s interpretation of the case reports and their final recommendations. In the case report of Reis et al, the injection was most likely extraneural. We propose this because the nerve block took 15 mins to set up after the injection of 30 mL of ropivacaine, 0.6%. In our experience (>1000 intraneural supraclavicular injections), the injection of 30 mL of 0.6% ropivacaine in the supraclavicular fossa results in a very rapid onset of sensory and motor block. In this setting, most patients will have a surgical block by the time the needle is removed from the patient’s skin and the block will be completely set in 98% of patients after 5 mins.

The results in the report of Cohen and Gray are not surprising. Virtually all texts (ours included) advise practitioners of the serious risks and sequelae of intraneural injection at the level of the nerve root or during interscalene block. Moreover, the needle used in this case (Cook EchoTip, Bloomington, Ind) is not designed for nerve blocks and has a bevel that is designed for vascular access or the biopsy/puncture of cysts. In the video of Cohen and Gray, the nerve plexus does not move as the needle enters the plexus. This is distinctly different from our experience and is likely related to the type of bevel on the needle. In more than 1000 supravacicular intraneural injections, we cannot recall being able to puncture the plexus with a blunt needle designed for nerve block without indenting or moving the nerve/plexus. Our experience using ultrasound guidance and needles that are not designed for nerve block is similar to Cohen and Gray’s. Sharp beveled needles can penetrate the nerve without indenting or moving the nerve and in this setting lead to a high incidence of transient nerve praxias. In this scenario, it may be the choice of needle bevel (lack of a tactile pop or visualization of nerve movement) that did not alert the users to intraneural needle placement. Finally, Cohen and Gray interpret their patient’s outcome as injury to the plexus and subsequent recovery. Although this interpretation may be accurate, it is also possible that the injection of a large dose of local anesthetic around or into a fascicle may provide a long-acting reservoir of local anesthetic, which takes weeks to be metabolized or reabsorbed from around or within the perineurium.

The analogy between neuraxial anesthesia and peripheral nerve block is germane here. Despite case reports of permanent nerve injury after spinal anesthesia, no one has called for its extermination, even though epidural anesthesia (which is performed outside the protective barrier of the axons) accomplishes most of the tasks of spinal anesthesia. Most practitioners would agree that spinal anesthesia is more reliable at producing a surgical block than epidural anesthesia; so too, intraneural injection is more reliable at producing surgical anesthesia than extraneural injection in our hands.

Brachial plexus block can be accomplished with as little as 4 mL of local anesthetic. Is it possible that 30 to 40 mL of local anesthetic deposited outside a nerve or plexus is as dangerous or more dangerous than 3 to 4 mL of local anesthetic deposited within the nerve or plexus but outside the perineurium? No one would criticize the use of 20 to 30 mL of local anesthetic for an epidural block, but certainly this dose would be inappropriate for a subarachnoid block and possibly toxic to the nerve tissue. At this time, it is difficult to determine if intraneural injection of peripheral nerves is more or less dangerous than extraneural injection. The use of multiple methods to prevent intraneural injection is speculative, and in a study, it seemed to contribute to additional intraneural injections. To improve safety, it seems more prudent to recommend limiting the type and dose of local anesthetic (as well as additives) used and to recommend the use of needles designed for nerve block. We believe that a scholarly debate at our national meeting with both sides of the issue presented would help to enlighten our readers.

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1. Neal JM, Wedel DJ. Ultrasound guidance and peripheral nerve injury, is our vision as sharp as we think it is? Reg Anesth Pain Med. 2010;35:335–337.
Neurologic Deficit in Conjunction with Intraneural Injection: More Questions than Answers

To the Editor:

In the July-August issue of the journal, Cohen and Gray present an interesting case of transient neurologic deficit in conjunction with a sonographically observed and recorded intraneural injection. The authors present a set of impeccable high-quality images as well as a video documenting the involved anatomical structures before and after injection. Then they proceed at discussing the possible causes for the observed neurologic deficit, alluding to the observed intraneural injection as the causative event.1

I would like to present the readers with an intriguing fact. On careful review of the excellent quality digital data provided, it is clear to me that “the injected brachial plexus component” is indeed the middle trunk/distal part of the C7 root of the brachial plexus. The clinical deficits reported by Cohen and Gray, however, are consistent with a C5 and C6 or upper trunk brachial plexopathy.

This discrepancy raises more questions than I can discuss in this limited space. Is the plexus postfixed? Are there other reasons for the axons to the musculocutaneous nerve to be carried by the middle trunk? Could the neuropraxia be secondary to surgical causes? It is unfortunate that the authors have not obtained an electrophysiologic study, which is standard practice after nerve injury. That could have helped elucidate the causative event of this case. Hence, unlike the authors, it is difficult for me to conjure a cause-effect relationship between a middle trunk intraneural injection and upper trunk clinical plexopathy. Although intraneural injection is controversial and not standard practice, Cohen and Gray’s case report, otherwise exquisitely well written and illustrated, is far from providing strong evidence that intraneural injection can result in neurological injury.2

If anything, it could be used as evidence of the contrary.

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2. Neal JM, Wedel DJ. Ultrasound guidance and peripheral nerve injury, is our vision as sharp as we think it is? Reg Anesth Pain Med. 2010;35:335–337.

Reply to Drs. Bigeleisen, Chelly, and Filip

To the Editor:

The authors thank Drs. Bigeleisen and Chelly for their detailed comments on the recent set of publications in Regional Anesthesia and Pain Medicine, which address adverse clinical outcomes after intraneural injections as visualized with ultrasound imaging (Video, Supplemental Digital Content 1, http://links.lww.com/AAP/A24). Indeed, previous reports written by these authors have contributed significantly to our understanding of intraneural injections.

We practice extraneural injections for regional anesthesia because of the overall efficacy and safety of these procedures.3 In the broad scope of this practice, we continue to recognize small incidental intraneural injections on rare occasions (in the range of 1%–2% of peripheral nerve blocks). The incidence of neurologic complaints and injury after regional blocks depends on the method of follow-up and timing.4 However, our present case clearly stands out as an adverse neurologic outcome. All discussion of this case must be placed in the context of more than 10 thousand unremarkable blocks we

FIGURE 1. Tentative assignments of the sixth (A) and seventh (B) cervical transverse processes (T) based on bony morphology. The sixth cervical transverse process typically has a “U” shape because of the relatively equal size of its anterior and posterior tubercles. The anterior tubercle of the seventh cervical transverse process is characteristically poorly developed. The C5 and C6 ventral rami are identified in panel A (arrows), and in panel B, the C5, C6, and C7 ventral rami are identified (arrows) in short-axis view. Note that the seventh ventral ramus is bifascicular in echotexture as it exits the transverse process, as is the sixth ventral ramus (B). The intraneural injection occurred within neural elements derived from the identified seventh ventral ramus. The skin surface is at the top of the images, L = posterolateral, with images 3 cm in depth.

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have performed with ultrasound imaging during the last decade.

Regarding the clinical management, an interscalene block was chosen because the patient was highly satisfied with the previous block performed a week before his second shoulder surgery. His only reservation was that pain relief only lasted 4 hrs after surgery, and now chronic shoulder pain symptoms had developed. The pharmacologic adjuncts (tetracaine and bupivacaine added to bupivacaine) were specifically chosen because of the need for prolonged pain relief when an indwelling catheter was considered contraindicated because of the proximity of shoulder infection. Particularly impressive was the absence of reported paresthesia by the unsedated patient.

Once a block needle tip is intra-neural, a myriad of factors can potentially influence neurologic outcome after injection including (1) bevel angle (sharp or blunt, with evidence supporting either one as more safe), (2) bevel orientation (longitudinal or transverse to nerve fibers), (3) injection pressure and volume, (4) the specific drugs injected, (5) whether nerve blood supply is damaged, (6) the anatomic location and regenerative capacity of the nerves, and (7) the exact needle tip location (extrafascicular versus intrafascicular, with the difference probably beyond the resolution of current ultrasound technology).

The relative importance of these factors will be difficult to sort out in experimental models let alone the clinical arena. We are hard pressed to find similar compelling evidence that these factors influence neurologic outcome after extraneural injections. Also germane to this discussion are the potential advantages of intraneural injections in block onset kinetics and reduced volume of local anesthetic. For the reasons listed above, we agree that this debate will continue but hope that progress and resolution can be made for patient safety and efficacy of our interventions.

The distinction between true nerve injury and prolonged conduction block from residual drug effect is presumably made on the basis of intraneural drug concentrations. Although certain radiopaque dyes (since discontinued from clinical use due to lack of resorption) can persist after injection into peripheral nerves, we know of no evidence that the same is true for any local anesthetic. Regardless of this formal distinction, our patient did not have full motor function of his upper extremity for 6 weeks, thereby compromising rehabilitation. The overall bevel needle angle of the block needle in question (20.3 ± 0.4 degrees, mean [SD]; n = 3 block needle samples, measured with photomicroscopy) is within the range of those specifically designed and marketed for regional anesthesia purposes. There are many determinants of nerve mobility that may influence the incidence of nerve impairment in clinical practice, including the free-running course of the nerve, whether unyielding surfaces such as bone lie nearby, nerve stretching from patient positioning, and the amount of transducer compression. Whereas some anatomic locations may be more predisposed than others to nerve injury, it is well established that peripheral nerve connective tissue content and fascicular architecture varies considerably. Whether ultrasound imaging can accurately detect those fascicular echotextures at higher risk for needle injection injury remains an open question (the present case argues that it can not).

The authors thank Dr. Filip for his insights and comments on our report. He raises the central question of whether the neurologic findings were anatomically consistent with recorded sonography. As Dr. Filip himself has recently commented, there are inherent limitations to assigning cervical contributions to the brachial plexus. The criterion standard is to use fluoroscopy to establish cervical levels relative to the occiput, atlas, and axis. Ultrasound imaging can also be used to examine cervical transverse process morphology and trace their respective ventral rami to the peripheral plexus for tentative assignments. Neither of these were fully done in the present case. However, the recorded sliding scans suggest the intraneural injection occurred at the middle trunk (derived from a continuation of the ventral ramus of C7; Fig. 1). Even with correct assignment, an additional layer of complexity is to consider that the levels, functions, and peripheral nerve derivation are all subject to anatomic variation and that collateral damage of adjacent nerves may occur from needle injection injury (either within or outside the plane of imaging).

A prominent physical finding was our patient’s near-complete loss of flexion at the elbow. The musculocutaneous...
nerve usually provides motor branches to all 3 flexor muscles (brachialis, coracobrachialis, and biceps brachii). This nerve contains contributions from the fifth, sixth, and seventh cervical rami. The function of flexion at the elbow is also partially served by the radial nerve (which receives contributions from all ventral rami of the brachial plexus) because it frequently provides a small motor branch to the brachialis. Sensory and motor function of the axillary nerve seemed intact on postoperative day 1, suggesting that at least part of the superior trunk was spared from injury.

One anatomic variation was clearly identified on preprocedural scans (Fig. 2). An artery passed through the brachial plexus, with the intraneural injection occurring inferior to this crossing point. The dorsal scapular artery consistently divides the middle and inferior trunks of the brachial plexus. However, we suspect that the identified artery in our case separates the superior and middle trunks of the plexus, in agreement with the designation of middle trunk injection. Electrodiagnostic studies were not done because of the gradual improvement in symptoms. Whereas these studies are commonly performed for evaluation and may have helped elucidate our patient’s neurologic deficits, we are not aware of formal standards in this setting.

Ultrasound and other imaging technologies have limited ability to ascertain the needle tip, nerve borders, and injection distribution (as does nerve stimulation). We reported the illustrations and accompanying video footage of the present case to help expedite this detection for readers and hopefully further improve the safety of peripheral nerve blocks guided by ultrasound.

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Intraneural Injections

Accepted for Publication: 13 September 2010

To the Editor:

We respectfully disagree with Drs. Bigeleisen and Chelly’s boldly assertive stance on benefits and apparent safety of intraneural injections. The dismissal of the dangers of intraneural
injections is at odds with the plethora of literature on the potential risk for devastating neurologic complications of intraneural, infrasacicular injections and cannot be supported by non-peer-reviewed publications, such as book chapters. The analogy between spinal anesthesia and intraneural injection with peripheral nerve blocks is not germane as the former comprises an injection into the subarachnoid space that is filled with cerebrospinal fluid, whereas the latter is a violation of the densely packed peripheral nerves. Intraneural needle placement is always accompanied by an adverse nerve response, such as intraneural inflammation. Fapojuwo et al have recently starkly refreshed our memory of the potential for severe nerve injury with intraneural injection with their report of 160 cases of debilitating sciatic neuropathy after guileful intramuscular injections.

Drs. Bigeleisen and Chelly argue that in the case by Reiss et al, an intraneural injection did not occur because the block took 15 mins to reach full onset. We disagree; injection into 1 trunk does not necessarily mean lead to an unusually rapid onset of the block in the entire plexus.

O’Donnell and Iohom, Latzke et al, and Eichenberger et al have all reported that miniscule volumes of local anesthetics (≤1 mL/nerve) result in rapid onset of nerve blockade when deposited under ultrasound guidance outside the peripheral nerves. If so, what can be gained with an intentional intraneural injection, except an unnecessary risk?

We agree with Drs. Bigeleisen and Chelly that the discussion on this and other emerging anecdotal observations with ultrasound-guided blocks should regularly take place at a national leadership level. Until a consensus on safety of intraneural injections is reached, our position is that such should not be recommended as standard practice.

Reply to Drs. Bigeleisen, Chelly, and Filip

Accepted for Publication: September 21, 2010

To the Editor:

We thank Drs. Bigeleisen and Chelly and Dr. Filip for their criticisms regarding our editorial, which was written as a commentary on case reports by Cohen and Gray and Reiss et al that described peripheral nerve injury in the setting of ultrasound-guided regional anesthesia. The central theme of both letters to the editor is that the case reports and editorial represent “an unsubstantiated condemnation of intraneural injection.” We believe this debate to be an important one and appreciate the unique perspectives offered by Dr. Bigeleisen and his colleagues.

The primary purpose of our editorial was to highlight the reports of peripheral nerve injury occurring despite the use of ultrasound-guided regional anesthesia, which together with an evidence-based review challenges the hope of some anesthesiologists that ultrasound guidance will prevent nerve injury. Nevertheless, the editorial did agree with the basic assertions of Cohen and Gray and Reiss et al that intentional intraneural injection is a practice with an unknown, but potentially high, risk-to-benefit ratio. The accompanying responses in this issue of Regional Anesthesia and Pain Medicine by Cohen and Gray and by Reiss et al defend their patient management and conclusions regarding causation. One issue not directly addressed by these respondents is the assertion by Bigeleisen and Chelly that limiting the type and dose of local anesthetic may be more prudent than avoiding intraneural needle placement when the goal is improved safety. Based on animal data that clearly link nerve injury severity to local anesthetic concentration, we partially agree their recommendation regarding local anesthetic. However, the landmark animal studies of Selander et al suggest that even miniscule volumes of local anesthetic (0.01–0.5 mL) are capable of causing nerve injury in the setting of increased intraneural pressure or mechanically damaged nerves; thus, we remain unconvinced that limiting local anesthetic volume is relevant once a nerve has sustained needle injury. We have little to add to our editorial stance other than to emphasize that our interpretation of the existing peer-reviewed literature of anesthesia-related peripheral nerve injury is that intraneural injection is probably best avoided, even while acknowledging that recent ultrasound-based studies demonstrate unintentional intraneural injection (most presumably are extraneous) occurs much more frequently during peripheral nerve stimulation, and perhaps ultrasound guidance, than suspected previously.

We agree with our critics that the full risks and benefits of intentional intraneural injection are currently unknown. We applaud and support Drs. Bigeleisen and Chelly’s call for open scholarly debate on this topic. In closing, we have chosen to interpret the existing literature conservatively while awaiting definitive answers to these important questions. Ultimately, only future peer-reviewed research will allow us to confidently understand this issue.

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Recognizing Dangerous Intraneural Injection: Is It the Musician or the Instrument?

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We read with interest the enlightening editorial by Neal and Wedel¹ regarding the challenges of real-time ultrasound (US) in detecting inadvertent intraneural injection and preventing nerve injury. These limitations are echoed in the American Society of Regional Anesthesia and Pain Medicine-sponsored evidence-based review of US-guided regional anesthesia recently published in this journal, which concluded that a superior safety profile with respect to nerve injury cannot—and likely will never—be demonstrated for US compared with traditional nerve localization techniques. One explanation offered to support this conclusion was that the “characteristics of US machines vary [and], acoustic resolution is limited.”² Indeed, there have now been various reports³⁻⁵ of unintentional intraneural injection despite the vigilant use of US guidance, some of which resulted in varying degrees of nerve damage. However, what is common to each of these reports is the retrospective recognition of intraneural injection following subsequent review of the stored images. Although the authors of each of these 3 reports are experts in US-guided regional anesthesia, they nonetheless apparently failed to recognize intraneural needle tip placement and intraneural injection during US-guided block performance. It thus begs the question: Is it the musician or the instrument? We propose that the limitation lies with the former, in that we as providers still do not reliably know what changes in nerve sonoanatomy constitute an intraneural injection and, more specifically and importantly, a dangerous intraneural injection. It seems probable that the sonocharacteristics of intraneural injections vary depending on the type and location of the nerve³⁻⁵; much like the motor responses to nerve stimulation,⁶,⁷ and arguably the propensity for nerve injury,⁸ vary depending on nerve type. Inevitably, pathognomonic sonographic features of intraneural injections and hazardous patterns of local anesthetic spread will declare themselves as we continue to gain experience with US technology, but at present, we may be shortsighted to blame our own limitations on the quality of our instrument.

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