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Reply to Drs. Filip, Shankar, and Bigeleisen

Accepted for Publication: December 17, 2013.

To the Editor:

We thank our colleagues for their thoughtful letter raising important questions regarding our prospective clinical trial,¹ which found that only 15% of subjects who received a single-injection interscalene nerve block by a posterior approach exhibited a complete surgical block of the hand and forearm.²

To address their first set of concerns, we respectfully disagree with their evaluation that “the needle placement target is vague and variable”—the protocol specified that “the ultimate target for the needle tip was the point between the deepest and next-deepest brachial plexus roots/trunks.” We believe that this was the optimal protocol given the study’s purpose as explained in the first paragraph of the article, “Historically... interscalene nerve block... frequently failed to provide surgical anesthesia of the hand and forearm... using the anterolateral approach, which presumably targeted the most superficial nerve(s)...but not the terminal nerves derived from the deeper (posterior) roots/trunks.”² We aimed to test a theory—originally proposed by others—that targeting the deepest neural structures at the level of the roots/trunks would result in surgical anesthesia of the hand/forearm.³ We disagree that prospectively specifying a specific trunk or root would have altered our results; as our colleagues themselves note, neuroanatomy varies enormously and it is often impossible to conclusively determine what exact neural structure is imaged at any specific level. As described by Boezaart, “The trunks of the plexuses are transitional areas. The perineurium surrounding the fascicles splits away and forms perineurial sheath interdigitations or septae. There seems to be interindividual variation on the level at which the septae form, but functionally and practically from a regional anesthesia perspective, the trunks should be regarded as transitional areas between clearly defined fasciculi with rigid perineuria at the branches to the root area where perineuria are not present and all the perineuria have joined to form the dura.”³

Furthermore, we did not “use the terminology root and trunk interchangeably.”

Rather, we noted that, in our study, the level that the block was performed included the transition zone between roots and trunks. For example, the optimal ultrasound view frequently revealed 4 neural branches visualized within the ultrasound plane (reported in Table 2); it is impossible to conclusively know if the plane was at the level of the roots (C8/T1 variant) or trunks (inferior trunk variant). Therefore, the optimal protocol specified targeting the space between the deepest and next-deepest roots/trunks to test our hypothesis.

We believe our introductory statement and premise—incorrectly quoted by our colleagues in their letter—to be accurate: “with the introduction of ultrasound-guided peripheral nerve blocks, it is now relatively easy to target any desired portion of the brachial plexus.” Within the context of the rest of our Introduction section, the proposition is that any level of the brachial plexus may be imaged; and, not that every contributing element of the brachial plexus may be imaged at every level.

Regarding Figure 1, our colleagues propose that what we described as the “deepest-visualized neural element” and suggested was either C7 or the inferior trunk was, in fact, “the distal C5 and C6 plexus roots.” It is unclear to us how they determined this from a static image without tracing the brachial plexus. However, as evidence of the difficulty in identifying specific elements in a static image, our colleagues incorrectly identified local anesthetic in this figure. As they explained, “...in Fig. 1, they show the needle positioned and local anesthetic spread... depicted is a possible C6 subepineurial intraroot injection” when, in fact, there had been no local anesthetic injection whatsoever at the time of imaging. Our legend described “local anesthetic spread” as asterisks that we superimposed on the picture to illustrate the location we defined as spread “around the deepest-visualized neural element.”

We agree with our colleagues’ opinion that, “based on their nerve stimulation response data, in Table 1, only 14% of the responses seem to be consistent with a C7 root stimulation,” for the *anterolateral* interscalene approach.^{4,5} However, we have found that the *posterior* approach to the brachial plexus—as used in our study—results in a far more complex and extensive evoked muscle motion pattern, often including deltoid contraction.⁶ Our colleagues opine otherwise; however, the

article they cite to support their opinion⁶ actually describes an anterolateral—and not a posterior—approach to the brachial plexus for the treatment group receiving electrical current and an evoked muscle response. There is no evidence that we are aware of—including our colleagues’ cited reference⁷—that supports their conclusion regarding the *posterior* approach and specific evoked motor responses.

We, therefore, strongly disagree with Filip et al that the motor responses described in our results suggest that the insulated needle was incorrectly located at the time of stimulation. Consequently, we believe that our colleagues provide no evidence to support their conclusions that “the authors have proven in this study is nothing more than what ‘conventional wisdom’ has taught us over the years” and “due to the lack of data demonstrating correct targeting and identification of the C7 root level, a conclusion regarding forearm and hand anesthesia... is not technically sound.”

We also want to emphasize that there are nearly as many techniques to place a brachial plexus block as there are practitioners administering them, and, we specifically did not describe the block of our study as a “cervical paravertebral,” as our colleagues’ title and letter (first paragraph) suggest. (Our article’s title included “Ultrasound-Guided Root/Trunk (Interscalene) Block.”) As noted in the Discussion section of our original article, “the results of this investigation pertain specifically to the techniques used in this study—other approaches would have most likely altered our findings.”¹ As such, there very well might be a single brachial plexus block technique that reliably produces surgical anesthesia to the entire upper extremity, but, we believe—contrary to our colleagues’ opinion—that our concluding statement is supported by the data of our study: “this investigation did not find evidence to support the hypothesis that local anesthetic injected adjacent to the deepest brachial plexus roots/trunks using a posterior, ultrasound-guided, needle in-plane approach reliably results in surgical anesthesia of the hand and forearm.”²

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The authors declare no conflict of interest.

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Reducing and Washing Off Local Anesthetic for Continuous Interscalene Block

Accepted for Publication: November 26, 2013.

To the Editor:

We read with great interest the recent letter to the editor by De and Hayes¹ regarding the report by Pakala et al.,² which described persistent phrenic nerve paresis after interscalene block. We agree that this is a complex condition and warrants consideration when performing interscalene block, especially in patients with pre-existing cervical spine disease.

We concur that large volumes of local anesthetic pose an increased risk of nerve damage because of compression.

FIGURE 1. Chest x-ray (with patient's written consent) showing the positions of the right (white arrows) and left (black arrows) hemidiaphragms before (left) and 1 hour after (right) injection of normal saline through the left-sided interscalene catheter.

With regard to continuous interscalene blocks, traditional infusion rates of 5 to 6 mL/h may not only predispose to acute phrenic nerve paralysis but also induce local inflammatory consequences of myotoxicity. These consequences, as reported,³ can result in permanent diaphragm paresis, requiring surgery to address adhesions and related tissue damage. Indeed, we have noticed that shortness of breath typically occurs on the second day after the continuous infusion has been running for an extended period (eg, overnight). This suggests that we may be unnecessarily flooding the anterior scalene muscle with excess local anesthetic. A recent study showed that as little as 2 mL/h of local anesthetic is sufficient for analgesia and that, at this rate, pain relief can last for several hours.⁴

In our institution, we occasionally observe shortness of breath after administration of a continuous interscalene block. Since we first reported the successful use of a bolus of normal saline via the catheter to “wash off” local anesthetic after a block,⁵ we have used this approach to deliver a bolus of normal saline (10–30 mL) via the catheter in several more (>5) patients (Fig. 1). In each case, the patient reported an immediate improvement in breathing. Although the underlying mechanism and the best regimen for block reversal remains unclear,⁶ the saline bolus may cause a dilutional effect, a reduction in local pH, alteration of local sodium content, or even a placebo effect. We recently initiated a study to determine the optimal mechanism and regimen.

In short, this article reminds us that diaphragmatic paralysis subsequent to phrenic nerve blockade is a real phenomenon in clinical practice, particularly when high doses of local anesthetic are used. Our observations have prompted us to review the dosing regimen used for

catheterized interscalene analgesia. Subsequently, we use intermittent boluses of 5 mL every 2 to 3 hours rather than a constant infusion. Although it is too early to tell if this change will effect a reduction in the complication rate, it is at least a start. Combined with the ability to provide a rescue reversal of unwanted phrenic nerve blockade, this technique presents a potential method for improving the safety of continuous interscalene brachial plexus block.

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