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Reply to Drs Bloc, Rontes, Mercadal, and Delbos

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Reply to Drs Bloc, Rontes, Mercadal, and Delbos

Accepted for Publication: August 23, 2013.

To the Editor:

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

To address their first set of concerns, we respectfully disagree with their evaluation that, "the injection was not really standardized and the level of injection unclear." 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 this was the optimal protocol given the study's purpose as explained in the first paragraph of the manuscript: "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."[1] 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.[2] We disagree that prospectively specifying a specific trunk or root (presumably C7 root or the inferior trunk) 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. 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 also believe that the 2 studies used to support their proposition that, "it is well established that only a deep injection between C6 and C7, or a distal motor response (ie, wrist) provides a wide distribution around the brachial plexus block [sic] resulting in an extensive block from C5 to C8-T1"[2,3] do not conflict with the results of our study. These 2 previous investigations used "loss of cold sensation" tested "with an ice cube" and "pinprick" to determine "success."[3,4] In contrast, our study used tolerance to cutaneous electrical current with a threshold of 50 mA to define "success," since "50 to 60 mA is the current considered equivalent to a surgical incision."[5–7] Because the goal of our study was "to determine the proportion of cases in which deposition of local anesthetic using ultrasound guidance to target the deepest brachial plexus roots/trunks produces hand and forearm *anesthesia* within 30 minutes," the 50-mA threshold defining "success" was appropriate. If we had instead used loss of sensation tested with an ice cube or pinprick, our reported "success" rates would have doubtlessly been much higher, as found in the 2 noted previous investigations.[3,4] Our goal was not to determine how many nerves would be *affected* by the block but, rather, the proportion of subjects exhibiting *surgical anesthesia*.

Unfortunately, our colleagues' statement, "in most cases, the motor response was the shoulder or elbow flexion that corresponds to stimulation of the superior trunk" was not referenced, but, although we agree with this generalization for the anterolateral approach—as was used for both studies referenced elsewhere in their paragraph[3,4]—we have found that the *posterior* approach to the brachial plexus results in a far more complex and extensive evoked muscle motion pattern, often including deltoid contraction.[8]

Regarding our colleagues' opinion that "it would have been more appropriate to avoid sedation, as patient participation, strength, and cutaneous sensation could have been affected:" (1) it would have been unethical for us to withhold sedation that is currently considered standard-of-care and (2) if the analgesic fentanyl had been withheld, it would have presumably *further decreased* the observed block success rate given subjects' decreased tolerance of electrical current/discomfort. Of note, we did not have "difficulty of holding the electrodes" because these were adhered to subjects for consistency during the 30-minute evaluation period. Regarding the issue of electrode location, we concur that there are communications among the terminal nerves (as noted previously, there is high neuroanatomy variability), but we disagree that our primary results were compromised. Our hypothesis involved discovering the proportion of subjects developing a surgical block, defined as anesthetizing the nerves of the hand and forearm (tolerating current, >50 mA); the existence of communicating branches becomes irrelevant given that *all* the nerves had to be anesthetized to be considered a "successful" surgical block (communicating branches would not alter these results).

Addressing the final paragraph of our colleagues' letter, we concur that the variability in outcome measurement definitions doubtlessly influences results and conclusions (as noted previously regarding success measured with ice, pinprick, and electrical current thresholds). However, we must disagree with our colleagues' opinion that, "it would have been more appropriate to distinguish sensory and motor block to surgical block," given that our primary interest was determining whether a single bolus of local anesthetic injected adjacent to the deepest-visualized brachial plexus root or trunk reliably produces surgical anesthesia of the hand/forearm (and, therefore, could replace supraclavicular, infraclavicular, and axillary blocks); and, both sensory and motor blockade are appropriate/desired when a surgeon is performing a procedure on the hand/forearm. Nevertheless, we did provide data on the degree of sensory and/or motor block produced in each nerve distribution within the first

30 minutes after injection, which seems to provide the same information that our colleagues believe would have been "more appropriate."

Finally, we *do* agree with our colleagues' conclusion that, "this [study's] negative result is very interesting because it confirms that the interscalene block should not be used as a first indication for hand and forearm surgery…"

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Toward a Better Understanding of Brachial Plexus Anatomy for Shoulder, Forearm, and Hand Anesthesia

Accepted for Publication: July 23, 2013.

To the Editor:

I read with great interest the article by Madison et al[1] regarding ultrasound-guided injection of the most distal visible neural elements during interscalene block (ISB). I would like some clarification, however, on 2 issues.

The first regards a detail of brachial plexus anatomy used throughout the study. The authors pair the C7 nerve root with the inferior trunk, implying that it is a continuation of C7[1] when in fact it is formed by C8 and T1.[2] The correct description would pair C7 with the middle trunk.[2]

The second concerns the use of axillary nerve function as an accurate predictor of adequacy of surgical anesthesia after ISB. Loss of shoulder abduction was reported in 100% of cases, but 16% failed to exhibit surgical anesthesia. Contribution to the innervation of the shoulder joint and associated structures via the suprascapular nerve has been reported to approach 70%.[3] In contrast, the axillary nerve is responsible for supplying a much smaller proportion, along with relatively minor contributions from the lateral cutaneous, musculocutaneous, and subscapular nerves.[4] Did the authors consider using assessment of suprascapular nerve function (ie, loss of external rotation) as a potentially more accurate assessment of the density of ISB anesthesia?

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Reply to Dr Price

Accepted for Publication: August 23, 2013

To the Editor:

We would like to thank Dr Price for his thoughtful letter raising important questions regarding our prospective clinical trial.[1]

Regarding Dr Price's first issue, he is correct in that the deepest nerve root is T1. However, it was not our intention to "pair"—or make equivalent in any way—the C7 nerve root and the inferior trunk. Rather, each of these structures is the deepest visualized of their respective brachial plexus locations. The inferior trunk and C7 nerve root were correctly included in the caption of Figure 1, in which 3 neural elements were imaged. Because it is difficult to conclusively determine if these neural elements were nerve roots or trunks—and in the figure, we highlighted and then referred to the "deepest-visualized neural element"[1]—we labeled this neural element either the inferior trunk (deepest trunk) or the C7 root (third deepest nerve root).

To address Dr Price's second issue, we did not "consider using assessment of suprascapular nerve function (ie, loss of external rotation) as a potentially more accurate assessment of the density of ISB." Although assessment of suprascapular function could have been used as an end point for a successful surgical block (and the suprascapular nerve is certainly involved in postoperative shoulder pain), we felt that the more distal departure of the axillary nerve off the brachial plexus would allow it to better represent the brachial plexus aggregate. Therefore, surgical anesthesia of the shoulder, defined as "the inability to abduct at the shoulder joint within 30 minutes of local anesthetic deposition,"[1] was ensured to be a result of brachial plexus anesthesia rather than suprascapular anesthesia. As Dr Price noted, in 100% of cases, there was a loss of shoulder abduction indicating a 100% success rate for accurate deposition of the local anesthetic bolus (as defined by our protocol).

In regard to Dr Price's concern that although loss of shoulder abduction was found in 100% of cases but 16% of the subjects failed to achieve tolerance to 50 mA of current delivered cutaneously over the inferior deltoid muscle,

