

## Statistically Significant?

### To the Editor:

Bone et al.<sup>1</sup> report that axillary brachial plexus anesthesia is enhanced by coadministration of neostigmine. This question was addressed with a study in which 34 patients were deemed suitable for analysis. (Ten patients were excluded for various reasons.) Half of the patients were given mepivacaine alone and half were given mepivacaine combined with neostigmine.

Bone et al. conclude that "peripherally administered neostigmine improves postoperative analgesia in axillary brachial plexus block." Their data, though, do not fully support this assertion. The report is hard to interpret because no hypothesis was specified. This makes it difficult to determine which outcomes were specified a priori. However, 6 outcomes are reported in the paper. There does not appear to have been any statistical compensation for testing multiple similar outcomes.

One reported outcome, and arguably the most important, was the total amount of analgesics requested by the patients. There was no statistically significant difference between the treatment groups. A second outcome was the time from axillary brachial block until the first request for pain medication. Again, there was no difference. There was also no difference in the number of patients with unchanged pinprick perception at any time in the study, and no difference in motor function between the groups. These also might be considered important outcomes.

There was a significant difference in visual-analog pain scores at 24 hours, but that was only 1 of the 4 times tested. Scores in both cases were relatively low, being 15 and 32 mm in the 2 groups. The authors also report that "fewer patients in the neostigmine/mepivacaine group had supplemental analgesics during the first 24 postoperative hours ( $P < .05$ )." In fact, this difference (9 of 17 v 15 of 17 patients) is not even nearly statistically significant: the correct  $P$  value is .5.

The reported data thus seem most compatible with a conclusion nearly the opposite of the one the authors make. Coadministration of neostigmine appears to produce little block enhancement during axillary brachial plexus anesthesia.

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## Reply to Dr. Sessler

### To the Editor:

We thank you for the opportunity to comment on Dr. Sessler's letter concerning our study. In our study we have analyzed the effects of adding neostigmine to the local anesthetic in patients receiving axillary brachial plexus block for upper extremity surgery.<sup>1</sup> Our major findings were reduced visual analogue pain scores (VAS) in the group of patients with neostigmine and a lower number of patients that needed additional analgesics after surgery in the neostigmine group.

Dr. Sessler had some comments on the statistical analysis of our data. The first criticism was a lack of statistical compensation for testing multiple similar outcomes. However, it is very uncommon in medical literature to correct for multiple comparisons when completely different parameters were analyzed, as in our study—pinprick perception and time to motor block or number of patients with analgesics.

Twenty-four hours after surgery patients in the neostigmine group had lower VAS than patients in the control group (15 v 32 mm). This difference is not only statistically significant, but also seems clinically significant to us. This difference in VAS was seen only 24 hours after surgery. Possible reasons for this late effect of neostigmine were discussed in detail in our report. In another recently published article, there were no effects of adding neostigmine to axillary plexus blocks.<sup>2</sup> In contrast to our study, the observation period in that study was shorter so that the late effects of neostigmine could have been missed.

We have found that 9 of 17 patients in the neostigmine group and 15 of 17 patients in the control group needed supplemental analgesics after surgery. Dr. Sessler stated that this difference was not even nearly statistically significant and that the correct  $P$  value would have been .5. We have carefully recalculated these data using a different statistical program than during the first calculation. The result of our  $\chi^2$ -analysis is a  $P$  value of .0239 without a continuity correction, and a  $P$  value of .0598 with a continuity correction. The use of a continuity correction was recommended for data similar to our study.<sup>3</sup> Therefore, after recalculating our data, the differences in supplemental analgesics between the two groups were not statistically significant. We apologize for this mistake. Nevertheless, our data showed that the coadministration of neostigmine to axillary plexus block reduced VAS scores after surgery.

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## Lumbar Spine Pain Originating From Vertebral Osteophytes

### To the Editor:

We read with interest the article by Dr. Lamer concerning the injection of painful osteophytes with local anesthetic and corticosteroid.<sup>1</sup> The author did not address the reason for his successful block. Needle placement was done with fluoroscopic guidance with needle position at the anterolateral border of the vertebral body. The innervation to the anterior compartment of the spine is from the sympathetic and sinuvertebral nerves.<sup>2</sup> Therefore, we believe that the area injected by Dr. Lamer (the osteophyte) was a result of the block of the sympathetic chain and ventral ramus innervating the osteophytes since 3 to 4 mL of the local anesthetic-corticosteroid solution was injected.

Similar to Dr. Lamer, we have had 3 patients with multiple-level unilateral anterior lumbar osteophytes. Lumbar sympathetic blocks provided significant pain relief in each of these patients for approximately 3 weeks. The blocks were repeated in 2 of the 3 patients because of the return of a significant degree of pain. At the 3-month follow-ups of these patients, we observed some degree of persistent pain, which was not severe enough to warrant repeat injections. We recommend consideration of sympathetic block on the side of the osteophyte for pain believed to originate from multiple osteophytes.

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## Response to Drs. Ackerman and Ahmad

### To the Editor:

I wish to thank Drs. Ackerman and Ahmad for their interest in the vertebral osteophyte article.<sup>1</sup> In that report I briefly described the innervation of the lumbar spine structures. Drs. Ackerman and Ahmad correctly point out, as I did, that injecting lumbar spine osteophytes may block adjacent nociceptive afferent and/or sympathetic efferent fibers. The report also discussed that subsequent symptomatic relief from such an injection may be due to a direct effect of the injectant on the osteophyte, block of the somatic or sympathetic nerves, a systemic effect of the injectant, a placebo effect, or some combination of these mechanisms.

All of the patients discussed in the article failed to respond to previous block of somatic and sympathetic nerves (i.e., epidural block), which is why I am inclined to believe that the therapeutic result is more likely from a direct local affect of the injectant. Pain relief after injection of osteophytes in other areas of the body including the feet, hands, knees, and shoulders is well described in the orthopedic, rheumatology, and podiatry literature, and some studies have shown a preventative effect of corticosteroids on osteophyte formation.<sup>2-5</sup> It may be that a lumbar sympathetic block as described by Drs. Ackerman and Ahmad provides relief of lumbar osteophytic pain by spread of the injectant from the area of the lumbar sympathetic chain directly to the area of the osteophyte.

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