



Comments on “The role of percutaneous neurolysis in lumbar disc herniation: systematic review and meta-analysis”

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TO THE EDITOR

I read the recently published article, “The role of percutaneous neurolysis in lumbar disc herniation: systematic review and meta-analysis” by Manchikanti et al. [1], with great interest. They performed a meta-analysis to evaluate the effectiveness of percutaneous epidural neurolysis or adhesiolysis in the management of chronic lumbar radiculopathy due to disc herniation by including 6 previous studies (1 randomized controlled trial and 5 retrospective single-arm studies) [2-7]. The standard mean differences in the change in pain and functional status after percutaneous epidural neurolysis or adhesiolysis ranged between -2 and -10, indicating high therapeutic effectiveness. Moreover, Manchikanti et al. concluded that the level of evidence for the use of percutaneous epidural neurolysis or adhesiolysis in the management of recalcitrant disc herniation or lumbar radiculopathy is “II.” However, I have some concerns regarding this study.

First, the authors have mentioned that they included studies that recruited patients with chronic disc herniation. The International Association for the Study of Pain has defined chronic pain as discomfort lasting longer

than 3 months, and even clinicians usually consider pain as chronic if it lasts for 3 months or more [8]. However, of the 6 included studies in the meta-analysis, only 2 studies [3,4] included patients with chronic lumbar radiculopathy (persistent pain for more than 3 months). The other 4 studies [2,5-7] included even those patients whose pain did not persist for over 3 months, or they did not include “chronic pain” in the inclusion criteria.

Second, 2 studies [3,7] included in the meta-analysis not only recruited patients with disc herniation, but also those with radicular pain induced by spinal stenosis. Because these 2 studies did not perform the sub-group analysis according to the disease types, the measured outcomes in patients with disc herniation and those with spinal stenosis were analyzed together. However, the study by Manchikanti et al. mentions that the inclusion criteria of these studies only included patients with chronic disc herniation. Furthermore, Manchikanti et al. have written the title and contents of the conclusion section indicating that these studies evaluated the effectiveness of the procedure only in patients with disc herniation. For accurate evaluation, the authors should have excluded the studies that included patients with spinal stenosis.

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In my opinion, a clarification of the aforementioned concerns is necessary in order to provide accurate information to the readers and increase the reliability of the meta-analysis.

CONFLICT OF INTEREST

No potential conflict of interest relevant to this article was reported.

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