regard, doubts may be raised about the propriety of interlaminear injections. However, lumbar transforaminal injections do not deserve to be tarred with the same brush, for the current data show that they are different. Let us not allow insurers to deny a treatment for which there is decent evidence of efficacy.

The review included another study of lumbar transforaminal injections, which was used to show favorable effect of epidural steroid over nonepidural steroid; yet in this study, no steroid was used. The study used etanercept, which is neither a steroid nor an analgesic, and which has been shown to be no more effective than saline. It is curious why the review was contaminated by inclusion of this study.

Competing Interests
None of the authors has a direct conflict of interest. Drs. Engel, Kennedy, and Bogduk serve as volunteer members of the International Spine Intervention Society (ISIS) Standards Division, and Drs. Kennedy, MacVicar, and Bogduk are volunteer members of the ISIS Board of Directors. Their views do not necessarily represent the views of ISIS.


References

Evaluation of Effect on Nonsteroid Epidural Injections

To the Editor:
‘Epidural Injections for Spinal Pain: A Systematic Review and Meta-analysis Evaluating the ‘Control’ Injections in Randomized Controlled Trials’ by Bicket et al. is an interesting and thought-provoking article. The authors have provided excellent information in reference to epidural injections of solutions other than steroids. Even though the authors have used an appropriate search strategy and quality assessment, some points of concern persist which may have influenced the conclusions.

As the authors have described, there is a wide variation in design and performance of the trials along with outcome assessments. Thus, it would have been appropriate if they compared local anesthetics versus local anesthetic and steroids, sodium chloride solution versus sodium chloride solution and steroid, sodium chloride solution versus steroid only, local anesthetic versus steroid only, epidural steroid versus intramuscular steroid, and so on in a manner which is much more reliable and understandable.

It seems the authors have missed at least three articles by Manchikanti et al. and also have used two duplicate articles providing inadequate analysis. The articles that were missed provided appropriate information similar to that provided by the other nine articles which could bolster the case for equal effectiveness between local anesthetics compared with local anesthetics and steroids.

In addition, the authors have developed an epidural steroid injection technical quality rating. Overall, the quality rating scale seems to be an excellent modality to assess the technical qualities. However, the authors have used some of the qualities which may be inappropriate in some cases such as excluding the patients with previous surgery (studies have been conducted only in postsurgery syndrome) and providing inclusion of patients with pain lasting less than 6 months, two points which are very unusual in chronic pain settings. In addition, the authors have provided only one point for radiographic guidance which should provide much higher importance for fluoroscopic usage and contrast injection, considering that a large number of injections are extra epidural and not target specific.

Hopefully, future studies will address some of the issues raised here.

Competing Interests
The authors declare no competing interests.

Laxmaiah Manchikanti, M.D., Frank J. E. Falco, M.D., Joshua A. Hirsch, M.D. University of Louisville, Louisville, Kentucky (L.M.). drlm@thepainmd.com

References
3. Manchikanti L, Cash KA, McManus CD, Pampati V, Benyamin RM: A preliminary report of a randomized double-blind,


(Accepted for publication January 21, 2014.)

In Reply:
We would like to thank Engel et al. and Manchikanti et al. for their astute comments regarding our recent systematic review and meta-analysis and will address their comments in order. Regarding the first statement by Engel et al. that we combined data from different approaches and regions, we acknowledge that it is true that cervical epidural steroid injections (ESIs) may be not be exactly the same as lumbar ESI, as is true for image-guided versus blind procedures, and for the various approaches to access the epidural space. By the same logic, one could also conclude that combining spinal stenosis with herniated disc, pooling subjects with psychosocial factors with those without comorbid psychopathology, not separating elderly from young patients, including both high- and low-volume injections together, and failing to separate different types of steroids are also flaws. But, if we had only included studies with homogeneous patient populations that used the same technique, the number of subjects in our meta-analysis would have been so small as to preclude any meaningful comparisons, and the generalizability would be negligible. This criticism also fails to consider that the main reason that patients fail to improve with ESI and other interventions is poor patient selection (i.e., greater disease burden, previous failed treatments, coexisting psychosocial factors), which outweighs by an order of magnitude the relative proportion that can be attributed to “technical failure.” To illustrate, a recent review article that stratified randomized trials by whether or not imaging was used found that a slightly higher proportion of studies in which the ESIs were done blindly had a positive result compared with those performed with image confirmation.

The comment that the study by Ghahreman et al., which we agree was an excellent study, was the only study to prospectively address the questioning being explored is incorrect. Two other studies, neither of which demonstrated a difference between the different control groups, also compared epidural nonsteroid injections (ENSIs) with nonepidural procedures. As for the authors’ assertion that “the results showing the efficacy of transforaminal injection of steroid is significantly greater than that of transforaminal injection of nonsteroid happen to contradict the conclusions of the review,” Engel et al. seem to reach the same false interpretation of our findings that the various lay press did. Our purpose was neither to prove, nor did our results show, that ENSIs are equally efficacious as ESIs, but rather that at the earliest available follow-up, ENSIs are superior to nonepidural injections. The authors also fail to appreciate that if well-conducted studies with more than 200 patients cannot reliably show a difference between ESI and a control treatment, then a study that allocates between 27 and 37 patients per group is incapable of detecting a difference between two ostensibly “control” treatments.

The authors correctly point out that there is some evidence that shows that transforaminal ESI may be more effective than other approaches. If this is the case, then one could logically deduce that transforaminal nonsteroid solutions would be also be more effective than interlaminar or caudal nonsteroid injections, which renders this point moot. This statement, which is probably true, also fails to note that the studies that compare transforaminal ESI with other epidural injections are all underpowered and seriously flawed (e.g.,

Table 1. Updated Effect Estimates for Positive Response to Injection

<table>
<thead>
<tr>
<th>Effect Estimate (comparison)</th>
<th>ESI vs. ENSI (Direct)</th>
<th>ENSI vs. NEI (Indirect)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Original</td>
<td>Reanalysis</td>
</tr>
<tr>
<td>Risk ratio (95% CI)</td>
<td>1.04 (0.96–1.13)</td>
<td>1.05 (0.97–1.13)</td>
</tr>
<tr>
<td>Risk difference (95% CI)</td>
<td>0.04 (–0.01 to 0.10)</td>
<td>0.05 (0.00–0.10)</td>
</tr>
<tr>
<td>Odds ratio (95% CI)</td>
<td>1.28 (0.98–1.67)</td>
<td>1.33 (1.03–1.73)</td>
</tr>
</tbody>
</table>

Data are given as effect estimate with 95% CI. ENSI = epidural nonsteroid injection; ESI = epidural steroid injection; NEI = Nonepidural injection.