

dural incision. Renal blood flow was similarly trended using laser-Doppler probes placed surgically against the renal capsule. Red cell flux monitoring was plotted against cerebral and renal perfusion pressure respectively as a definitive standard pressure autoregulation curve. The aim of the study was to test the accuracy of separately measured metrics of dynamic vascular reactivity derived from near-infrared spectroscopy: the hemoglobin volume index describing cerebral vascular reactivity and the reno-vascular reactivity index to describe vascular reactivity in the kidney. Although not the primary aim of that study, it was observed in some animals that *renal blood flow fell during hemorrhagic shock before any change in arterial blood pressure occurred*. Although, cardiac output was not directly monitored, these findings of reduced renal blood flow despite no change in blood pressure can be explained only by a reduced cardiac output. This was demonstrated by the example data shown in the figures of the article. Thus, these findings support our contention that renal blood flow is dependent on both blood flow (cardiac output or cardiopulmonary bypass) and blood pressure as we state in our editorial.

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Competing Interests

The authors declare no competing interests.

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Not All Injections Are the Same

To the Editor:

In metaphorical terms, a problem arises when good apples are pooled with bad apples; they all get tarred with the same brush. This principle applies when all studies pertaining to spinal injections of steroids are pooled, as if they are all equal. Subsequently, the lay press publicizes sweeping conclusions such as “injecting any liquid, even plain saline solution, works just as well.”¹ Such statements bring all injections into disrepute.

Admirably, Bicket *et al.*² used an ingenious statistical exercise to explore the conjecture that epidural injections of other agents are not fair controls as epidural injections of steroids. However, in their exploration, they pooled data on cervical and lumbar injections, on image-guided injections and blind injections, and on interlaminar, caudal, and transforaminal injections; they even included studies that did not involve steroids. Given that these various targets and techniques differ with respect to pathology, anatomy, technical accuracy, and evidence base, such pooling might not be legitimate, and at least clouds the true picture.

Prominent among the studies analyzed is that of Ghahreman *et al.*³ which, indeed, the authors rank as rigorous. In the statistical analysis, this study stands out as an outlier; but it is also different in other respects. It is one of the few studies included in the review that used transforaminal injections, and it is the only study that actually addressed prospectively the very question being explored by the meta-analysis. In that regard, its results happen to contradict the conclusions of the review. It showed that the efficacy of transforaminal injection of steroid is significantly greater than that of transforaminal injection of nonsteroid. The authors of the review have referred to a conclusion that detecting a difference between treatment and control groups would not be practical but have not stated that this conclusion related specifically to long-term (12 months) outcomes.

It would have been more courteous, and more informative, had the authors stratified their analysis by region and by technique. Their conclusions might still apply to classical, blind epidural injections, but they would not apply to lumbar transforaminal injections. Lumbar transforaminal injection of steroids is significantly more often effective than transforaminal injection of either local anesthetic or saline, and intramuscular injection of either steroids or saline, and by the same magnitude in all cases.

It may be that these data could be overturned by future studies, but at present, they are the only direct data on this procedure. Those data defy the sweeping generalizations of the review, which are sensationalized by the lay press, and which serve the purpose of those who wish to deny reimbursement for epidural injections.

Meta-analysis of circumstantial evidence does not constitute proof; is not a substitute for well-designed controlled trials that address the issue. It serves only to raise an intriguing proposition worthy of studies that prospectively test it. In this

regard, doubts may be raised about the propriety of interlaminar 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 steroid⁴; 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.⁵ 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.

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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.*¹ 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.*^{2–4} and also have used two duplicate articles^{5–8} 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.

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