Letter to the Editor

Is There Really No Benefit of Vertebroplasty for Osteoporotic Vertebral Fractures? A Meta-analysis

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To the Editor

We read with interest the article by Shi et al. [9] published in your journal that addresses the controversial topic of percutaneous vertebroplasty (PVP) using meta-analysis of selected literature. The authors' conclusion [9] appears to contradict Recommendation 8 of the American Academy of Orthopaedic Surgeons (AAOS) "Clinical Practice Guideline (CPG) on the Treatment of Symptomatic Osteoporotic Spinal Compression Fractures" [2]. We would like to address the differences between their analysis and that done by the AAOS CPG Workgroup.

Although the meta-analysis process was similar in both reviews, some methodologic aspects differ. AAOS established a priori content based inclusion and exclusion criteria for study selection [2]. It is unclear how Shi et al. determined which articles to include. The AAOS Workgroup excluded three studies that they included [1, 4, 8] for poor study design description and for not being the best available evidence. A study is not the best available evidence if there are at least two studies of higher quality that

measure the same outcomes; high quality studies are unlikely to be overturned by future evidence. Three studies in the current review [5, 7, 11] were published after the AAOS CPG was completed, but would not have changed our recommendation. Only three studies were mutually included [3, 6, 10].

Both reviews show no difference between sham treatments and PVP [2, 9]. There is disagreement in outcomes between nonoperative therapy and PVP, a comparison not made in randomized controlled studies because of inherent bias. The AAOS considered the magnitude of treatment effects by using statistical significance in the context of minimal clinically important improvements (MCII). The MCII is the smallest change that is important to patients; it controls for statistically significant treatment effects that are too small to matter. Shi et al. appear to incorporate only statistics. They rely heavily on the results of PVP that measure subjective outcomes and, in their conclusion, diminish the validity of comparing PVP with nonoperative treatment [9].

The meta-analysis by Shi et al. offers a different conclusion on the benefits of PVP than does the AAOS guideline. The methodology used by the AAOS is substantially more rigorous and transparent. Additional studies used in the Shi et al. analysis either do not meet our inclusion criteria standards or their weakened quality would exclude them in the AAOS analysis. We believe the AAOS's use of MCII is preferred over the use of straight statistical comparisons used by Shi et al. PVP versus sham surgery is the gold standard of comparisons, and these results are not contradicted.

It is understandable that physicians would want to validate a procedure that they believe works. For this to occur, high-quality studies using appropriate outcome measures will have to show a difference greater than the MCII.

(Re: Shi MM, Cai XZ, Lin T, Wang W, Yan SG. Is there really no benefit of vertebroplasty for osteoporotic vertebral fractures? A meta-analysis. *Clin Orthop Relat Res*. 2012;470:2785–2799.)

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