LETTERS TO THE EDITOR

Pedicle screw–based dynamic stabilization and adjacent-segment disease

TO THE EDITOR: We read with great interest the article by Dr. Han and colleagues4 (Han Y, Sun J, Luo C, et al: Comparison of pedicle screw–based dynamic stabilization and fusion surgery in the treatment of radiographic adjacent-segment degeneration: a retrospective analysis of single L5–S1 degenerative spondylosis covering 4 years. J Neurosurg Spine 25:706–712, December 2016). The authors conducted a head-to-head comparison of 31 cases in which patients underwent pedicle screw–based dynamic stabilization (PDS) with 31 cases in which patients underwent standard posterior lumbar interbody fusion (PLIF) for L5–S1 spondylosis. They concluded that the incidence of adjacent-segment disease (ASD) 4 years after surgery was similar in patients treated with PDS and those treated with PLIF. Also, the authors found pre-existing disc degeneration at L4–5 to be an independent risk factor for ASD detected radiographically later on. Thus, the authors made the statement that PDS is feasible for L5–S1 spondylosis and pre-existing ASD does not necessitate any additional treatment, only observation, in the absence of clinical symptoms or signs.

We would like to respectfully point out 2 caveats pertaining to this study. First, the necessity of instrumentation for L5–S1 spondylosis is controversial, particularly given that the authors mentioned in their Methods that there was no instability. Unlike percutaneous screws, a conventional open approach for the placement of pedicle screws at L-5 inevitably involves dissection of the facet capsule and surrounding musculature of L4–5, which could predispose patients to acceleration of pre-existing ASD. Second, the authors used a midline approach rather than the Wiltse approach, which is usually recommended for candidates for PDS. Patients who have undergone surgery using the Wiltse approach reportedly have decreased risks of wound infection, reoperation, and ASD.5

The rationale for the use of PDS systems is preservation of segmental motion and protection from disc degeneration at both the indexed and adjacent segments.2,7,9 For an optimal chance of reducing ASD, the pedicle screws are thus suggested to be inserted via the Wiltse approach without any facet violation.5 In the literature, the main concerns related to PDS were screw loosening and facet arthrosis.1,5,6,10 Although most reports demonstrated satisfactory clinical outcomes with PDS, whether it protects against ASD remains debatable.1,3,10 Further investigations—particularly investigations involving larger numbers of patients—are required to clarify the risk factors of ASD, as well as the best candidates for PDS systems. Nevertheless, the authors are commended for sharing their experience with worldwide readers of the Journal of Neurosurgery: Spine. Their study demonstrates promising results for the application of dynamic stabilization.

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Disclosures
The authors report no conflict of interest.

Response
We are pleased to respond to the letter from Professor Cheng and colleagues.

There were 2 reasons why we did not include simple decompression in our study. The first reason was that a complete decompression would require en bloc laminectomy, which can jeopardize lumbar spine stability, and spinal instability is one of the risk factors for lower back pain and segmental degeneration. The other reason was that the limited access and visualization of simple decompression may lead to nerve injury and incomplete decompression. Nerve injury may be related to inadequate intracanal exposure or excessive nerve retraction, which can be minimized by wide decompressive laminectomies. Furthermore, patients may experience recurrence of signs and symptoms of disc herniation or canal stenosis due to incomplete decompression. For these reasons, we adopted PLIF or PDS to treat the 62 patients who met the inclusion criteria. Definitely, simple decompression and discectomy were used in some cases that were excluded from this study.

The Wiltse approach exploits the avascular intermuscular plane between the multifidus and longissimus, leading to better tissue conservation, less pressure during retraction, and less bleeding. We did use the Wiltse approach in several cases, but the number was too small to allow for inclusion of these cases in our study.

Thank you for your interest in our paper.

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Screening via CT angiogram and cervical spine fractures

TO THE EDITOR: We would like to thank the authors Lockwood et al.2 for their study, which is a valuable effort to look at the effectiveness of CT angiography (CTA) to diagnose vertebral artery injury (VAI) in patients with blunt trauma (Lockwood MM, Smith GA, Tanenbaum J, et al: Screening via CT angiogram after traumatic cervical spine fractures: narrowing imaging to improve cost effectiveness. Experience of a Level I trauma center. J Neurosurg Spine 24:490–495, March 2016). However, we would like to raise a few concerns about the study.

Denver screening criteria were found to correlate with VAI, but not with posterior circulation stroke. The authors stated that 10 patients with cervical spine fractures had posterior circulation strokes—we are assuming these are only in 732 of the 1435 patients who were screened with CTA. Six of the 10 patients with posterior circulation strokes did not have VAI, according to the authors. However, as per Table 4, 2 of these patients did not have CTA. Also, the authors did not mention if the CT angiograms were retrospectively reviewed to see if the VAI was normal on those images. Previous studies have found a significant false-negative rate with CTA for VAI.4 Because the study started with charts from 2002, it might include many patients who underwent scanning with earlier-generation CTA units, which have been reported to have lower sensitivity.1 Did any of the patients in the study have digital subtraction angiography as part of their initial workup after CTA?

We would like to ask the authors to clarify if all the patients in the study were routinely followed up to assess stroke outcome. The authors stated that ICD-9 codes were used to identify patients who suffered a stroke within 30 days of injury, but it was unclear if the code by itself would cover the entire study cohort. The authors also stated that all strokes were confirmed on MRI or CT scans—was CT scanning performed routinely or only when patients were having acute neurological symptoms? If routine surveillance was not performed in all patients, the incidence of stroke could be even higher.

Four of the 10 strokes occurred in patients with VAI who either were not treated or despite treatment. The authors stated that 2 died from polytrauma injuries and 2 were discharged neurologically intact. Could the authors specify if the strokes happened at the time of presentation or before treatment was initiated?

In 3 of the 6 patients with stroke who did not have VAI, their strokes are ascribed to atrial fibrillation—was it a new diagnosis after trauma? If not, was anticoagulation stopped due to concern about bleeding with polytrauma? If it was a new diagnosis, did these patients receive adequate anticoagulation therapy? Embolic stroke in trauma