Pedicle screw–based dynamic stabilization and adjacent-segment disease

TO THE EDITOR: We read with great interest the article by Dr. Han and colleagues (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.

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. For an optimal chance of reducing ASD, the pedicle screws are thus suggested to be inserted via the Wiltse approach without any facet violation. In the literature, the main concerns related to PDS were screw loosening and facet arthrosis. Although most reports demonstrated satisfactory clinical outcomes with PDS, whether it protects against ASD remains debatable. 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 have required 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 intramuscular 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 those 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

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patients can also occur from long-bone fractures and fat embolism, and results of neck CTA performed for detection of VAI would be negative.

Some of the numbers in the tables do not add up. In the Results section, the authors state that 35 patients received antiplatelet treatment, but Table 2 lists only 32.

A total of 26 of the 51 VAIIs in the current study were Grade 3 or Grade 4 injuries. This is significantly higher than found in most published literature. We wonder if the authors have an opinion on this. Follow-up imaging was performed in 28 of the 51 patients. Could the authors specify if follow-up was performed in low-grade or high-grade injuries, what the results were, and if it changed management?

To comment on the true value of CTA, it would also be important to assess the 329 high-risk and 374 low-risk fractures that were not screened with CTA and to compare their outcomes. We agree that cost-effectiveness of screening for blunt cerebrovascular injury needs further studies, especially given the significant heterogeneity in the literature.3

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Disclosures
The authors report no conflict of interest.

Response
Thank you for allowing us to respond to the concerns regarding our manuscript voiced by Wu et al. To start, this was a retrospective review of patients with and without CTA screening in the setting of cervical spine fractures. The premise of the study was to evaluate high- and low-risk fracture types based on the Denver criteria, and to evaluate how we were using CTA at our institution.

Many of their concerns regard the subgroup analysis of our posterior circulation strokes. To clarify, we identified 10 posterior circulation strokes in the entire 1435-patient cohort. Posterior circulation strokes were identified by formal chart review and by MRI or CT studies of the head. Of these, 8 had CTA, which we state in Table 4. As stated in our Methods, formal radiology reports were used for identifying VAI in this study, and this is a limitation to it. The 4 of the 10 strokes occurring in patients with VAI that the authors are referencing happened in a delayed fashion and were not identified at the initial time of presentation. Moreover, the atrial fibrillation strokes were identified at presentation and had intracranial pathology on initial CT imaging that was consistent with embolic stroke, which was confirmed on formal chart review by physician documentation as the most likely etiology of the stroke. Of the 51 patients in our cohort with VAI, the correct number of patients receiving antiplatelet therapy is 35, and this is an error in Table 2.

Charts for all patients within this cohort were run through the ICD-9 codes for stroke and identified within 30 days of the index injury. There are certainly limitations to a retrospective chart review, such as physician discretion about the ordering of advanced imaging, patient follow-up at or presentation to another hospital facility, or advanced imaging being obtained at outside facilities. Certainly these are limitations that pertain to all retrospective chart reviews that report follow-up information, and we mention most of these in our discussion.

Within this current study, Grade 3 and Grade 4 VAIIs made up 50% of our total, which is significantly higher than in other published literature, and follow-up imaging was heterogeneous, with some patients receiving digital subtraction angiography while others just had CTA. Many did not receive imaging and were treated empirically at the discretion of the ordering physician. Certainly this is a limitation to the study, and further investigation is warranted into the management of patients after VAI in the setting of trauma. The number of high-grade injuries could be related to the bias within our study concerning who received CTA in the first place. A large portion of our cohort did not receive CTA screening. We believe that a larger multicenter study reviewing the incidence of low- versus high-grade VAI as well as further interobserver reliability studies need to be performed.

Certainly, we all agree that prospective studies on the cost-effectiveness of screening for blunt cerebrovascular injury need to be performed.
Adult spinal deformity surgery: is it always worthwhile?

TO THE EDITOR: We read with great interest the article by Smith et al.13 (Smith JS, Klineberg E, Lafage V, et al: Prospective multicenter assessment of perioperative and minimum 2-year postoperative complication rates associated with adult spinal deformity surgery. J Neurosurg Spine 25:1–14, July 2016). The authors conducted a study of 346 patients to prospectively assess the rates of complications associated with adult spinal deformity (ASD) surgery with a minimum 2-year follow-up based on a multicenter study design; they also stratified complication rates by age and provided a general assessment of factors potentially related to complication occurrence. Overall, 469 complications (207 minor and 262 major) occurred in 203 (69.8%) of 291 patients for whom a minimum of 2 years of follow-up was achieved. Altogether, at least one revision surgery was required in 82 patients (28.2%) and higher complication rates were associated with older age, greater body mass index, increased comorbidities, previous spine surgery with fusion, and 3-column osteotomies.

With great pleasure and contentment, we welcome this paper written by world-renowned experts in the field. It has to be acknowledged that spine surgery for ASD is associated with an extremely high risk rate for potential perioperative and delayed complications. Moreover, a large percentage of affected patients are not “poster patients” and are often aged, obese, possess significant comorbidities, and have previously undergone back surgery. At least in our daily clinical routine, the majority of patients seeking medical advice for sagittal imbalance problems present with one or more factors associated with an increased complication rate or a putative unfavorable outcome. Therefore, the questions inevitably posed are as follows: Is surgery always worthwhile and what should we tell our patients when consulting on ASD problems?

Surgery for ASD has become extremely popular following the “French revolution” concerning the concept of sagittal balance.6 According to this concept, the pelvic parameters (e.g., pelvic tilt, pelvic incidence, and sacral slope) should be taken into account in the treatment planning and subsequent care of the patients including the restoration of sagittal balance. Several studies have shown that restoring sagittal balance is related to a better postoperative health-related quality of life (HRQOL).7,11,15 Furthermore, it has been widely reported that surgeries not respecting the basic rules of sagittal balance are prone to failure. Of course, the industry embraces surgeries that involve multiple operated vertebral levels, which may have an economic impact that cannot be quantified, without taking into account direct and indirect costs of ASD revision surgery (a caveat: according to the paper by Smith et al., there was at least one revision surgery in 28% of patients). Unfortunately, we have the impression that the spine industry is pushing in this direction by popularizing the concept of sagittal balance and supporting training courses staffed with renowned experts on sagittal balance correction surgery. More recently, the idea that if surgeons are not respecting the sagittal balance rules then they are not doing a good job is dangerously taking hold in the surgical community.7,8,12

Data on complications of ASD surgery are often underestimated and underreported. Recently, Kim et al. found an overall incidence of neurological complications in ASD surgery of 17.6%. The incidence of surgical neurological complications in their series was 13.7%. There was a higher risk of neurological complications in revision cases and in interbody fusion cases.5 Booth et al. reported 11 complications in 7 patients out of 28 patients who underwent surgery for fixed sagittal imbalance, corresponding to an overall 25% complication rate.2 Glassman et al. reported major complications in approximately 10% of cases, adversely affecting outcome as shown by a deterioration in SF-12 general health scores 1 year after surgery.4 These complications not only have a negative impact on pain intensity, functional impairment, and HRQOL but also have a substantial economic impact associated with the management of ASD patients. Yeramaneni et al. reviewed 17 articles that included a total of 355,354 patients with 11,148 reported complications.14 Infection was the most commonly reported complication with an average treatment cost ranging from $15,817 to $38,701. Hospital costs for patients with deep vein thrombosis, pulmonary thromboembolism, and surgical site infection were 2.3–3.1 times greater than for patients without those complications.

The best way to minimize the risk of complications or an unfavorable outcome is a meticulous patient selection. A careful and exhaustive assessment of general conditions of those patients is therefore of paramount importance. Specifically, cardiac and pulmonary function need to be assessed, coagulation pattern checked, and eventually skin microflora identified. Ideally, a model that predicts absolute risk of medical complications after ASD surgery, rather than relative risk or odds ratio values, would greatly ameliorate the discussion of safety in complex spine surgery. The SpineSage tool, for example, is a predictive model based on data derived from the Spine End Results Registry (SERR), which is a prospectively collected data registry for all patients who underwent spine surgery at Harborview Medical Center and University of Washington Medical Center from 2003 to 2004.9 This tool allows the determination of the likelihood of a medical complication, such as cardiac failure and infection, or a surgical complication, such as incidental durotomy, according to the patient’s risk factors and the invasiveness of surgery. We therefore suggest the implementation of such a tool in the ASD surgery selection process. Of course, further validation studies are mandatory to test the tool’s applicability and feasibility in daily clinical routine.

More recently, experts in the field have reported that the application of minimally invasive surgical (MIS) techniques might reduce invasiveness and complication occurrence in ASD surgery.1,10 However, MIS techniques are not suitable for every ASD case, and in general it is indicated mostly for cases involving nonfixed sagittal imbalance. Nevertheless, the definite role of MIS techniques in ASD surgery is still unknown. Bach et al. performed a systematic review of MIS techniques in ASD.1 Invariably, MIS techniques led to a clinical improvement according to visual analog scale leg/back pain scores, Oswestry Disabil-
ity Index, treatment intensity scale, and HRQOL measured with the SF-36. Reported fusion rates ranged from 71.4% to 100% 1 year after surgery. Interestingly, however, only 4 of 10 studies reporting radiographic results on deformity correction found the procedures to be effective in correcting deformity. There were 115 complications reported among the 258 patients (45%), including 37 neurological complications (14%). These data clearly show that MIS techniques are associated with a probably lower, but still high, risk of complication and failure in ASD surgery.

We personally think that sagittal balance is an appealing concept, but patients may pay an excruciatingly high price to obtain good clinical results. Additionally, we only have results after a 2-year follow-up, with 69.8% of patients affected by some degree of complications. As the mean age of the patients in the study by Smith et al. was 56.2 years, the projected life expectancy of these patients may be 20–30 years or even more. Correspondingly, the risk of complications in the long-term follow-up might be considerably higher. Again, the authors should be commended for their honest multicenter report showing a somewhat higher complication rate than previously reported. As a conclusion and in order to be congruent with our Hippocratic Oath, we should tell our patients the truth: “Primum non nocere” and “do no further harm” concepts should always be kept in mind before suggesting ASD surgery to our patients.

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Disclosures
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Response
No response was received from the authors of the original article.

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