Sagittal plane analysis

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How to cite

FAUNDEZ, Antonio, LE HUEC, Jean Charles. Sagittal plane analysis. In: Journal of neurosurgery. Spine, 2013, vol. 19, n° 6, p. 795–796. doi: 10.3171/2011.10.SPINE11306

This publication URL: https://archive-ouverte.unige.ch/unige:90390
Publication DOI: 10.3171/2011.10.SPINE11306

Neurosurgical forum

Letters to the editor

Intraarticular spacers

To The Editor: We found the article by Goel et al.[2] (Goel A, Shah A, Jadhav M, et al: Distraction of facets with intraarticular spacers as treatment for lumbar canal stenosis: report on a preliminary experience with 21 cases. Clinical article. *J Neurosurg Spine* [epub ahead of print September 16, 2011. DOI: 10.3171/2011.8.SPINE11249]) very interesting. The study described a preliminary experience with 21 patients with lumbar stenosis symptoms who underwent 1- or 2-level distraction of the facet joints with an indirect widening of the spinal canal and intervertebral root dimensions. The authors reported that there was a decrease in patients' symptoms, as assessed using the Oswestry Disability Index and the visual analog scale at 6 months of follow-up.

Although the authors advocated that this new surgical technique is valid and safe, some questions should be raised. In the modern era of spinal surgery, clinical outcome is associated with an adequate restoration of the sagittal balance.[1,4] Distraction of the facet joints in the lumbar spine would result in segmental kyphosis, adversely affecting the sagittal balance and consequently the long-term outcome. Another important fact is that a new medical procedure should be at least as efficient as the available therapeutic modalities. This new technique is very restrictive with respect to surgical indications (patients without clear instability, without anterior compression due to disc herniation, and with just 1 or 2 levels of stenosis). It also requires 8 weeks of bed rest/limited activities versus early ambulation obtained with the traditional decompressive procedures.[3] Long-term bed rest is associated with complications such as deep venous thrombosis, pneumonia, and muscular atrophy, in addition to its economical and psychological impact in very active patients. The indirect decompression of the nerve roots can also be temporary, with early new stenosis. Moreover, the patients who met the inclusion criteria could have been treated with standard laminectomy or foraminotomy, without the need for the use of spacers, which increases surgery costs.

In light of all these points, new studies comparing the safety and efficacy of the distraction of lumbar facet joints with conventional surgical decompressive procedures are necessary before we include this surgical technique in the armamentarium to treat lumbar spine stenosis.

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Disclosure

The author reports no conflict of interest.

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Response: We appreciate Dr. Joaquim's interest in our article and in the proposed technique of facet distraction.

Facet distraction using spacers is an attempt to reverse the telescoping or vertical instability that occurs secondary to age-related spinal degeneration or to spinal degeneration related to muscle disuse or abuse. Our study on the subject concludes that facet articulation is a primary site of spinal degeneration and that overriding of facets is an initial pathological phenomenon that ultimately results in spinal canal stenosis.[1–4] Ligament hypertrophy, disc space reduction, osteophyte formation, and similar conditions that lead to reduction in the circumferential diameter of the spinal and neural canals are secondary to the primary vertical instability of the facet joint and are potentially reversible. The angulation of the facets and their articulation are architecturally unique for each spinal level and are designed to provide for stability and mobility. Distraction of facets is an attempt to restore the height and lordosis of the spinal segment and maintain the sagittal balance. The procedure assists in strengthening or restoring the sagittal balance rather than adversely affecting it. Although, in our article on distraction for single- or multilevel cervical spondylosis, we mentioned that preoperative kyphosis of the spinal segment may preclude surgery, we now feel that even such cases can be appropriate for the procedure of distraction.[4] In the presence of preoperative manifest or demonstrable instability of the spinal segment, it appears that spacers by themselves may not be able to provide the necessary stabilization, and in such cases additional instrumented fixation appears mandatory. Our technique aims at arthrodesis of the spinal segment. Once arthrodesis occurs, restenosis of the spinal segment may only be a remote possibility, akin to restenosis after corpectomy and bone graft–fusion surgery.

We are convinced that our technique of facet distraction has a promising future in the treatment of lumbar canal stenosis. The simplicity of the technique can permit even percutaneous surgery. Avoidance of the need for removal of any bone, ligament, and disc are also positive

This article contains some figures that are displayed in color online but in black-and-white in the print edition.

features. The procedure can be conducted for a single- or multilevel canal stenosis. Laminectomy or foraminotomy can be done in a relatively straightforward manner in cases of failure of distraction arthrodesis of facets. In general, our policy is that whenever fusion of the spinal segment is contemplated, postoperative rest for a period of 8 weeks is advocated for provision of an optimum environment for bone fusion. We feel that such a period of rest can be avoided if a rigid external arthrosis can be used.

Surgeons have been successfully resorting to laminectomy and laminotomy for decades. The principal of facet distraction as proposed in our article is essentially similar to that involved in the currently popular methods of inter–spinous process and inter-laminar distraction. Bone- and soft tissue–saving techniques can certainly be considered in the treatment of degenerative canal stenosis.

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Please include this information when citing this paper: published online October 11, 2013; DOI: 10.3171/2012.8.SPINE12689a.

To The Editor: We read with interest the article by Goel et al.[10] (Goel A, Shah A, Jadhav M, Nama S: Distraction of facets with intraarticular spacers as treatment for lumbar canal stenosis: report on a preliminary experience with 21 cases. Clinical article. *J Neurosurg Spine* [epub ahead of print September 16, 2011; DOI: 10.3171/2011.8.SPINE11249]).

In Greek mythology, Panacea (Πανάκεια, named after the goddess of the "universal remedy") was supposed to be an unknown miraculous substance that would be able to cure all diseases. It was one of the three great quests of medieval alchemists together with the "elixir of life" (a magic potion that would grant eternal life and everlasting youth) and the "philosopher's stone" (a mythical substance that would enable the transmutation of common metals into gold).

Although contemporary medicine is generally thought to have been freed from mystical approaches, the intriguing phenomenon of searching for a universal and perfectly efficient therapeutic tool (which is probably related to an unconscious and deep primitive human urge to find the ever-desired antidote against mankind's greatest enemies, illness and death) manifests itself as a recurrent pattern in the medical literature. In fact, the most common reaction to any new therapeutic alternative (both in medicine and surgery) that presents a real advance to a specific field usually follows a very characteristic pattern: First, the new alternative is considered worthless and the product of a vain and unnecessary search for novelty; then its value is clearly overestimated and it is considered the possible "universal remedy" for a broad range of otherwise unrelated pathologies (a phase which in surgery has been described by the well-known humorous hammer/nail analogy); ultimately, after being both passionately rejected and excitedly accepted, when the long-term outcomes of the new treatment become more clear (usually several years later), its real value is finally appreciated and its proper role in the therapeutic armamentarium becomes well established.

In relation to the scientific basis of therapeutics, clinical medicine is classically understood as having been built upon the solid ground of a "pathophysiology-based" pharmaceutics. In a similar fashion, modern surgery has been founded on a clear and objective understanding of healthy and pathological anatomy. Although in recent decades the advances of evidence-based medicine have shown us that logical predictions (based upon our best knowledge about the pathophysiology of a certain disease) are not sufficient to prove the efficacy of a specific treatment, this does not mean that pathophysiology and anatomy have no role in providing the basic scientific support for new medical therapies and surgical techniques. Pathophysiology and anatomy may, in fact, not be sufficient to prove the efficacy of a new treatment, but they should certainly be necessary for their proposal. In other words, the statistical significance of the outcome measures of a new treatment that is not grounded in a solid anatomical and pathophysiological understanding of the disease for which it has been proposed should be rather interpreted as a result of a myriad of biases that may affect data collection, statistical analysis, and outcome measurements rather than as a proof of efficacy of the treatment.[20]

We believe such remarks are very relevant for those who read the interesting study in which Goel et al.[10] describe the results of their new surgical technique for the treatment of lumbar canal stenosis.

In this paper, the authors present a series of 21 patients with lumbar canal stenosis who were treated with an alternative surgical technique (the so-called Goel's intra-articular spacers), which basically consists of a posterior approach to the lumbar spine with wide opening of the inter-articular joints, denuding of the articular capsules, distraction of the facets, and forced impaction of intra-articular spacers (Goel's cages) in an attempt to achieve, at the same time, fusion as well as distraction and opening of the neural foramina. The clinical results presented by the authors demonstrate an improvement in the visual analog scale (VAS) for back pain and in the Oswestry Disability Index (ODI) scores at a mean follow-up of 17 months. The authors also present the expected radiographic results of such technique: an increase in facet, foraminal, interspinous, and disc space heights. With a basis on such data the authors conclude that this new technique of lumbar facet joint distraction through cage implantation should be considered an effective alternative for the surgical treatment of single- or multilevel lumbar canal stenosis.

After a careful analysis of the issue, some remarks are worthwhile. First, the use of VAS (back pain) and ODI as the only measures for determining clinical improvement in patients with lumbar canal stenosis is far from the ideal. As is well known by all neurosurgeons, lumbar spinal stenosis is a clinical syndrome that concurs with symptoms of neurogenic claudication, back pain, and radicular pain in variable degrees. Although VAS scores for back pain are very useful to evaluate the clinical improvement in back pain after a fusion procedure (which the proposed technique attempts to perform), there is insufficient data on the presented results to prove that such technique results in significant improvement in radicular symptoms (which could have been measured by the VAS for leg pain, for example) or in neurogenic claudication. The only additional score used in the study (the ODI score) does not differentiate between symptoms related to axial and radicular pain or between the disability related to pain and the functional impairment of neurogenic claudication. Other measures (such as the Zurich Claudication Questionnaire and the Neurogenic Claudication Outcome Score) have already been extensively used in clinical studies with patients with lumbar canal stenosis as specific tools for evaluation of neurogenic claudication.[25] Moreover, as previously discussed, in the current era of comparative effectiveness research it is no longer acceptable to defend effectiveness of a surgical procedure based solely on statistical significance.[20] Rather, outcomes should be analyzed in terms of the actual clinical and functional impact achieved by the proposed treatments. In fact, for all the scores that were used in Goel's study (as well as those which were not but should have been) there have already been studies that determined their minimum clinically important difference (MCID) and substantial clinical benefit (SCB). Such cutoff values cannot be ignored in any serious attempt to evaluate the clinical impact of a new therapeutic strategy as they play an essential role in providing objective parameters to which the clinical outcomes can be compared.[21] Moreover, other functional tests (such as the Treadmill Test or Self-Paced Walking Test) have been successfully used as surrogate markers for the evaluation of walking capacity and walking performance in patients with lumbar canal stenosis, constituting additional tools that should be used in order to objectively rate the degree of improvement provided by any new therapeutic approaches intended to be similar or superior to the conventional treatment of lumbar canal stenosis.[5]

Moreover, according to the authors, the intra-articular spacer technique was proposed as an indirect method for lumbar canal decompression, by reducing ligamentum flavum buckling as well as disc protrusion through distraction of the affected level. Although the authors state, "On MR images, there was clear evidence of reduction of the posterior disc bulge and ligamentum flavum indentation into the spinal canal, resulting in an overall increase in the spinal canal dimensions," there is no radiographic proof of such "statistically significant" decompression of the lumbar canal in the postoperative imaging follow-up as measured by an increase in the dural sac cross-sectional area, the current "gold standard" for evaluating central canal stenosis. In fact, Fig. 2 (in the original article) demonstrates very questionable imaging results described by the authors as "marginal reduction in the indentations opposite the L3–4 and L4–5 disc space." Therefore, at least from the radiographic standpoint, although it could be stated that the proposed technique may significantly increase the distance of the posterior elements of adjacent spinal segments at the treated levels (likely resulting in an indirect foraminal decompression and maybe in some relief of the radicular symptoms, which, as already mentioned, the study did not evaluate), there is no proof that this new technique is effective in increasing the cross-sectional area of the lumbar canal and alleviating neurogenic claudication, one of the most important sources of disability in patients with lumbar canal stenosis.

According to the presented clinical outcomes (as measured by the employed clinical scores) as well as the radiographic results, it would be reasonable for the authors to defend the proposed procedure as potentially effective for fusion of the desired level (maybe as an alternative to pedicle screw fixation). Stating that such a technique effectively decompresses the spinal canal (with results similar or superior to conventional surgical techniques) is clearly an attempt to reach much farther than the presented data allow.

Nevertheless, in our opinion, the main drawback of this new proposed technique is that its principal therapeutic mechanism (distraction of the posterior spinal elements) presents a clear theoretical conflict with what is currently known to be the ideal local biomechanical status of the lumbar spine. Although the authors demonstrated that such technique may result in distraction of both the posterior and anterior elements of the spine (by showing an increase in the interforaminal and interspinous distance—as well as, to a lesser degree, in the anterior height of the intervertebral disc), it is crucial to understand that from a biomechanical standpoint it is impossible to obtain a proportional distraction of both posterior and anterior elements with parallel forces applied through a short moment arm posterior to the internal axis of rotation (IAR) (such as the one that results from the implantation of the intra-articular spacers). Therefore, due to the simple fact that the momentum arm is posterior to the IAR, it is inevitable that any distraction provided by the intra-articular spacers will necessarily induce at least some degree of local kyphosis.[2] In fact, the only forces able to distract the anterior spinal elements in the same proportion as the posterior ones (and, therefore, preserve or increase segmental lordosis) would be those applied by two forces acting in parallel both anterior and posterior to the IAR (such as those obtained with 360° fusion, for example), or through the use of specific applied moment arm cantilever beam constructs (such as Schanz screws) that enable parallel distraction without rotation over the IAR.[23]

In relation to the biomechanical effects of posterior approaches to the spine, it has already been demonstrated that the simple resection of the posterior elements (as well as the consequent damage to the paravertebral muscles induced by the lateral dissection) are enough to induce a loss in lordosis that has been estimated to be up to 8° after a 2-level (L4–S1) posterolateral transpedicular fusion.[24]

In the case of application of parallel distractive forces posterior to the IAR (such as the ones that occur with the implantation of the intra-articular spacers) the expected loss of lordosis would, of course, be much greater. As important data regarding pre- and postoperative segmental lordosis have not been provided by Goel et al. (an important drawback of their study), we asked some colleagues from the mathematics department of our institution if it would be possible to perform an estimation of the biomechanical effects of the newly proposed technique in terms of segmental lordosis by considering the pre- and postoperative morphometric values provided by the authors regarding the interforaminal distance, the interarticular distance, and the anterior and posterior disc height. After an initial analysis it was concluded that some additional variables that have not been provided by Goel et al. (such as the average for the L-4 and L-5 anterior body height, posterior body height, body length, and foraminal width) would be required for building a consistent biomechanical model of the spine. Therefore, we searched the literature in order to obtain such morphometric data from previous anatomical studies of the adult lumbar spine (Table 1, preoperative values).[7] On the basis of such values, it was then possible to build a consistent geometric model that simulated the behavior of the lumbar spine after implantation of the intra-articular spacers.

According to the proposed "trigonometric model of concentric circles," the polygonal chains linking the anterior and posterior borders of the L4/S1 bodies were approximated to segments of parallel rings of concentric circles (Fig. 1).

Mathematically we have:

$$\widehat{AE} \approx AL4 + AL4/L5h + AL5h + AL5/S1h$$

$$\widehat{BD} \approx PL4 + PL4/L5h + PL5h + PL5/S1h$$

Although this model had to take into account the degree of error regarding the approximation of the polygonal chain to perfect arcs of circle (which was estimated to be insignificant, in light of the order of magnitude of the angles which were being calculated), for didactic purposes the employed methodology could be easily explained in the following manner (for a complete mathematical proof of the model see Fig. 3).

The classic formula for calculating the length of an arc of a circle is $L = r \times \theta\pi/180°$, where L is the length of arc, θ is the central angle of the arc in degrees, and r is the radius of the arc.

According to the values for the distances between the spinal elements before and after the implant of the interarticular spacers (Table 1, postoperative values) it was possible to calculate the change in the length of the arcs of the external and internal segments of circles passing adjacent to the anterior and posterior longitudinal ligaments, respectively, and, therefore, estimate the new central angle of the segment of circles (θ′).

TABLE 1: Morphometric preoperative values as obtained from the original paper and classic studies and postoperative values as calculated by using the trigonometric model of the concentric circles*

Variable	Preop	Postop
SL	33°	???
L4L	34.9	34.9
L5L	33.9	33.9
S1L	34.6	34.6
Fh4	10.14	**12.52**
Fh5	10.14	**12.52**
Fw4	1.68	1.68
Fw5	1.74	1.74
AL4/L5h	5.26	**6.71**
PL4/L5h	4.09	**5.11**
AL5/S1h	5.26	**6.71**
PL5/S1h	4.09	**5.11**
FL4/L5	2.8	**5.75**
FL5/S1	2.8	**5.75**
AL4h	27.4	27.4
AL5h	28.3	28.3
PL4h	27.1	27.1
PL5h	25.7	25.7

* With the exception of the value for segmental lordosis (SL), all values are in millimeters. The preoperative values are based on the paper by Goel et al.[10] or, for the variables for which values were not provided by Goel et al., averages from classic morphometric anatomical studies of adult lumbar spines. The postoperative biomechanical effects of the implant of the interarticular spacers were calculated according to the trigonometric model of the concentric circles. The values shown in bold type are those that presented a significant change after implantation of the intraarticular spacers. See Fig. 1 for definitions of abbreviations.

According to the performed calculations, the implantation of intra-articular spacers bilaterally at both the L4–5 and L5–S1 levels would result in a new central angle (θ′) (corresponding to the segmental lordosis of L4–S1) that would be $\theta' = 0.553426 \times \theta$, in which θ is the initial angle (that is, the preoperative segmental lordosis) and θ′ is the final angle (the postoperative segmental lordosis)

According to previous radiological studies on the morphometric parameters of the lumbar spine in adults, the average segmental lumbar lordosis has been estimated to be approximately 19.5° ± 5.2° at the L4–5 level and 13.4° ± 6°at the L5–S1 level, with a total L4–S1 lordosis of 32.9° ± 11.2°.[11]

Applying these values to the above formula where the original angle (θ) would be 32.9°, the final angle after the implantation of the intra-articular spacers (θ′) would be: θ′ = 18.21° (Fig. 2).

In summary, according to the mathematical model built upon the data provided by Goel et al., the final biomechanical effects of the implantation of intra-articular spacers bilaterally inside the L4–5 and L5–S1 facet joints would be an unacceptable final reduction in the segmen-

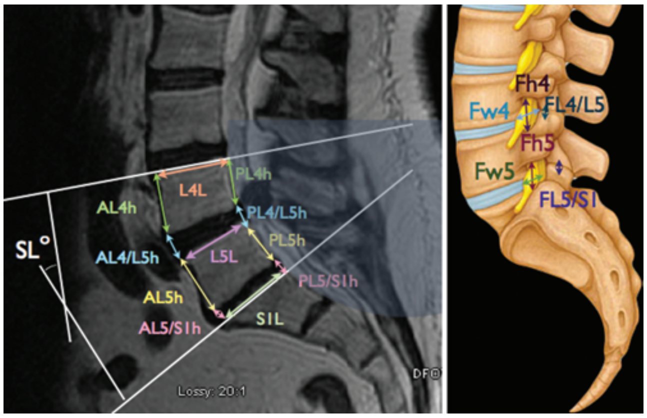

Fig. 1. Definition of variables used in the proposed "trigonometric model of concentric circles." The values for the variables in bold type (which were not provided by Goel at al.[10] but were necessary for completing the geometric modeling of the anatomical relationships among the spinal elements) were based on averages for adult spines obtained from the morphometric anatomical studies of Gilad and Nissan.[7] As can be observed, both the polygonal chain linking the anterior margin of the spine adjacent to the anterior longitudinal ligament and the polygonal chain linking the posterior margin of the spine adjacent to the posterior longitudinal ligament can be geometrically approximated to parallel segments of arcs of circles with an extremely low distortion in the measured values. **AL4h** = anterior height of L-4 vertebral body (VB); **AL5h** = anterior height of the L-5 VB; **AL4/L5h** = anterior height of the L4/L5 disc space; **AL5/S1h** = anterior height of the L5–S1 disc space; **Fh4** = height of L-4 foramen; **Fh5** = height of L-5 foramen; **Fw4** = width of L-4 foramen; **Fw5** = width of L-5 foramen; **FL4/L5** = inter-facet distance between the L4/L5 facets ("facet height" in original study's nomenclature); **FL5/S1** = inter-facet distance between the L5/S1 facets ("facet height" in original study's nomenclature); **L4L** = AP length of the L-4 superior endplate; **L5L** = AP length of the L-5 superior endplate; **PL4h** = posterior height of L-4 vertebral body; **PL5h** = posterior height of the L-5 VB; **PL4/L5h** = posterior height of the L4–5 disc space; **PL5/S1h** = posterior height of the L5–S1 disc space; **SL** = segmental lordosis; **S1L** = AP length of the S-1 superior endplate.

tal lordosis of 14.69°. As already mentioned, although such values have not been reported in the study (maybe purposely, as the authors probably already expected such deleterious effect of the technique upon the segmental lumbar lordosis), such postoperative effects can be easily noted in the postoperative MR images and CT scans presented in Fig. 2 of the original article, which resemble classic examples of flat-back syndrome.

In relation to the clinical effects of postoperative changes in the biomechanics of the lumbar spine, there is already an extensive body of literature demonstrating the harmful effects of loss of lordosis upon the natural history of degenerative pathologies, as well the clinical improvement which results from surgical restoration of lordosis, not only for central canal stenosis but also in the context of degenerative disc disease and spondylolisthesis.[1,12,13,16–18]

For example it has already been demonstrated in an animal model that fusion in kyphosis accelerates the degenerative changes in adjacent facet joints.[22] Moreover, in a cadaveric study that evaluated the degree of lordosis of a fused segment and its effects on adjacent-segment motion, it has been demonstrated that a hypolordotic alignment of the fused segments causes the greatest amount

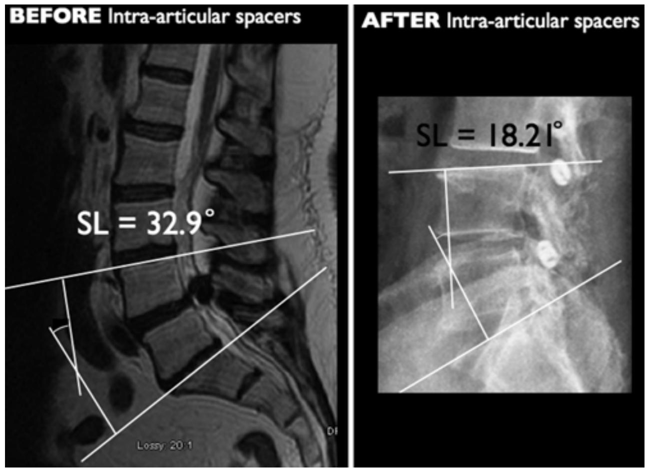

Fig. 2. Lumbar segmental lordosis at L4–S1. **Left:** Average L4–S1 segmental lordosis (LS) according to morphometric studies in adult spines. (Value of 32.9° based on Harrison et al.[11]) **Right:** Estimated biomechanical effects on lumbar lordosis of the implantation of intra-articular spacers bilaterally at 2 levels, according to the data provided by Goel et al. and as calculated on the basis of the proposed trigonometric model of the concentric circles.

of flexion-extension motion at the superior adjacent segment.[1] Additionally it has already been demonstrated that such biomechanical instability in adjacent levels, which is induced by segmental kyphosis, leads to increased rates of adjacent-segment disease.[15] Similarly, it has been demonstrated that in children and adolescents who undergo surgery for spondylolisthesis, correction of the kyphosis of the affected segment is one of the main factors that predict successful long-term fusion.[6]

Regarding the relationship between restoration of segmental lordosis and clinical outcomes after fusion, a retrospective study of patients submitted to double-level posterior lumbar interbody fusion for degenerative disc disease demonstrated a positive correlation between an increase in the lordotic angle and an increase in the Japanese Orthopaedic Association (JOA) score for back pain.[12] Another

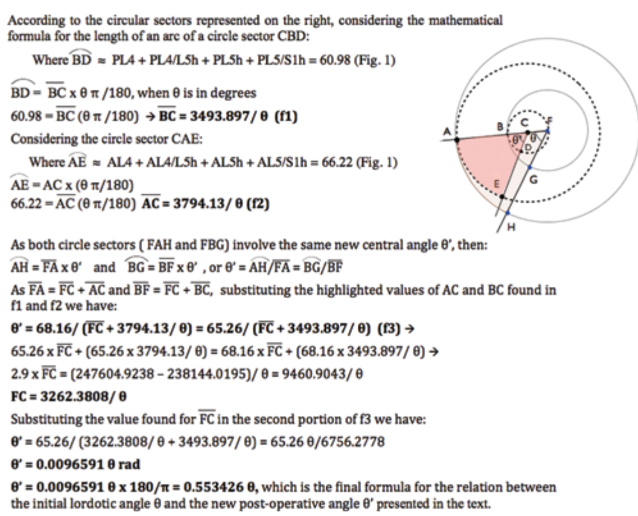

Fig. 3. Geometrical demonstration of the final equation for relating preoperative (θ) and postoperative lordotic angles (θ′) in the proposed trigonometric model of the concentric circles.

retrospective analysis, which evaluated the outcomes of a motion-preserving stabilization system for treatment of Grade 1 degenerative spondylolisthesis, demonstrated that "total lumbar lordosis" and "segmental lumbar lordosis" were significantly higher in the group with optimal clinical improvement in comparison with the group with suboptimal clinical outcomes. Such results suggest that improvement in the sagittal spinal alignment through the increase of segmental lumbar lordosis is one of the most influential factors for back pain relief and functional improvement in the context of degenerative spondylolisthesis.[4] Additionally, retrospective analysis of 45 cases involving patients who had undergone a transforaminal lumbar interbody fusion (TLIF) procedure for single-level degenerative disc disease demonstrated that a lower degree of postoperative lordosis was associated with worse back and leg pain as assessed by VAS scores. Moreover patients with persistent leg pain at the final follow-up visit had significantly less lumbar lordosis than patients without leg pain.[17] Similarly, another study, which evaluated the effects of sagittal balance upon clinical outcomes after posterolateral fusion for degenerative spondylolisthesis, found a positive correlation between postoperative lordosis and postoperative recovery rates.[22] All these results suggest that improving the sagittal alignment by restoring the segmental lordosis is one of the most influential factors for back pain relief and functional improvement in the context of degenerative pathologies of the lumbar spine.[4]

In order for the intra-articular spacer technique to be considered at least noninferior to other surgical techniques for treatment of lumbar canal stenosis and for fusion, it must be shown that it provides a major advantage in relation to current standard techniques in order to compensate for its deleterious effects on the local segmental lordosis. In fact, most previous clinical series that presented good long-term outcomes after fusion have also demonstrated a significant increase in lumbar lordodis. For example a previous series of patients submitted to anterior lumbar interbody fusion (ALIF) demonstrated a mean increase in overall lumbar lordosis of 6.2°.[13] Another series of patients submitted to TLIF demonstrated an average increase in lumbar lordosis of 15.2° per level.[14]

Therefore, any technique that proposes to distract the posterior elements of the lumbar spine without a proportional distraction of the anterior ones (necessarily inducing at least some degree of kyphosis) presents, from the outset, a serious biomechanical conceptual problem. Moreover, with the current amount of data supporting the correlation between segmental lumbar lordosis and clinical postoperative outcomes, in our opinion, an alternative surgical technique that, instead of preserving or increasing lordosis (such as the current standard procedures relying on interbody fusion), clearly induces kyphosis is very likely to lead to unsatisfactory long-term clinical outcomes.

Even though we believe in the role of minimally invasive procedures in spine surgery, we have recently demonstrated that the use of posterior-elements distraction devices as an attempt to mitigate major interventional procedures, while ignoring the distortions in important global biomechanical parameters such as sagittal and coronal balance (as in patients with adult degenerative scoliosis), is not able to provide a sustained clinical improvement in the setting of multilevel lumbar canal stenosis, ultimately resulting not only in higher long-term costs but also in unacceptable rates of complications and revision surgeries.[19]

As this commentary is focused on the objective results presented by the study, we will avoid (and leave such judgment to the readers) any criticisms regarding the issue of possible conflict of interest in the research of a group that has already applied to several spinal pathologies, without any restriction, a new surgical technique employing intra-articular spacers that, by the way, bear the main author's last name.

In summary, regarding such therapeutic innovation, it is clear that the Indian group led by the renowned spine and skull base surgeon are certainly in the "enthusiastic stage" of the use of intra-articular spacers in spine surgery. Expecting that a treatment that, according to the authors' reports, has already been successfully applied for the treatment of craniocervical instability[9] may provide the same good results in otherwise completely different pathologies of the spine (such as cervical spine myelopathy[8] and lumbar canal stenosis[10]) is completing ignoring the anatomical particularities of and striking differences between the biomechanics of the craniovertebral junction, subaxial cervical spine, and lumbosacral spine. As already mentioned, although a proper biomechanical, pathophysiological, and anatomical rationale may not be enough to ensure the efficacy of a new proposed treatment, they are certainly necessary for their acceptance as part of the therapeutic armamentarium for a specific pathology, especially in the scenario of spinal canal stenosis, in which microsurgical decompression—with or without fusion—possesses the solid status of a well-established gold-standard treatment with very good long-term clinical outcomes.

In other words, the application of intra-articular spacers seemed to be relatively reasonable in the craniocervical junction,[9] it was quite questionable in the cervical spine,[8] and it is entirely unacceptable in the lumbar spine,[10] where lordosis is the "apple of the eye" with respect to local biomechanics. As a surgical and scientific community open to innovation we should acknowledge that there may be a role for the technique of intra-articular distraction in spine surgery, but certainly not for all pathologies and at all segments of the spine. Personally, in the absence of a solid pathophysiological and biomechanical basis, we do not believe that intra-articular spacers are the hidden universal solution for all pathologies in spine surgery, even if in the near future the authors present another questionable-methodology series demonstrating apparent short-term good functional results after the use of their "universal" intra-articular spacer for the treatment of thoracolumbar osteoporotic fractures, thoracic disc herniations, or any other spinal pathology.

In order to demonstrate that these are not isolated and meaningless warnings for an unreal danger, it is worth remembering that an alert regarding a similar imminent danger has already been sounded in the past by older, well-known and diligent watchmen.[3]

In summary, unless the technique of intra-articular spacers becomes solidly supported by biomechanical studies that take into account the pathophysiological particularities of each spinal pathology it is supposed to be treating (as well as each specific segment of the spine for which it is being applied), rather than being presented as a revolutionary universal technique, it should be deemed as one more manifestation of the old-Greek goddess Panacea, who, from time to time, shows up in the spine surgery literature, exhibiting her seductive contours and deluding the simple and the naïve.

At this point, it is worth remembering the ancient words of wisdom of King Solomon of Israel: "Say unto knowledge, Thou art my sister; and call understanding thy kinswoman. That they may keep thee from the strange woman, from the stranger which flattereth with her words." *Proverbs 7:4-5*

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Disclosure

The authors report no conflict of interest.

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Response: The thoughtful and scholarly critique by Drs. Mattei and Abdounur of our work on the intra-articular spacers is welcome, for it is helpful in providing us greater clarity toward evaluation of our approach. Not a single surgery on the spine can be deemed perfect. We must recall Prof. Ian Aird, surgeon-writer of the United Kingdom, who aphorized that "Each surgery for peptic ulcer is an experiment." Likewise, each spine surgery is an experiment toward evolving increasingly better techniques. So must be the intraarticular spacers we have proposed.

Muscle disuse– or abuse–related weakness and telescoping of the spinal segments due to "vertical" instability primarily related to standing human posture is the key issue in spinal degeneration. Facets are in different configurations but essentially have the same function in the entire spine. Facet distraction works as much in the other parts of the spine as it works in the craniovertebral junction. It may be that external traction or internal distraction as proposed by us is the treatment or panacea for degenerative diseases at all spinal levels. The hammer and nail analogy may just be true in cases of degenerative spine disease.

The issue of spinal degeneration has been under discussion for decades. Drs. Mattei and Abdounur will agree that the last word in the pathogenesis and treatment of spinal degeneration has not yet been said and continued opinions on the subject are necessary for the philosophy of management to evolve. Some of our recent publications depict our current concepts in the management of degenerative spinal disorders.[3–7] Essentially, on the basis of our experience, we believe that disc degeneration and disc space reduction are not the primary issues in spondylotic disease; rather it is muscle weakness–related facet listhesis or vertical instability that is the prime initiator in the entire pathogenesis. We had emphasized the role of instability in the process of spinal degeneration and proposed that only stabilization of the spine may be the treatment for spinal degeneration.[5] I am relieved to read that Mattei and Abdounur have acknowledged the effectiveness of facet spacers in providing stability to the spine. We proposed that bone need not be removed, osteophytes need not be resected, and ligaments need not be manipulated.[3–7] We have demonstrated, more effectively in the cervical spine, the reversal of all pathological spinal events related to degeneration by a single maneuver of facet distraction. We are convinced that the phenomenon is the same in the lumbar spine. However, radiological demonstration of reversal of spondylotic changes is not as clear in the lumbar spine as it is in the cervical spine.

We have, in a way, adopted a rule-of-thumb approach. Distracting articular facets de-telescopes the spine to undo the buckling consequent to the spine's telescoping. In our ongoing study we shall bear in mind all the points that Drs. Mattei and Abdounur have raised. That should, in the long run, prove or disprove the therapeutic benefit of spacing. Dr. Mattei's previously published articles suggest that he has been critical regarding the techniques that involve distraction of the posterior spinal elements.[12,13] However, the techniques that involve distraction of posterior spinal elements have continued to gain wide acceptance and are in extensive clinical use throughout the world. The concept of external traction has been used effectively for backache related to lumbar and cervical degeneration. Traction as a form of treatment is currently in use in a wide range of limb fractures and deformities. Distraction of the spinal elements also appears to be the key issue for success in cases where disc space fusion is performed or an artificial disc is introduced.

The advantage of facet distraction as a form of treatment for degenerative spinal stenosis is in its simplicity and effectiveness. The ease of the procedure can be gauged by the fact that it is possible even with percutaneous methods. The facet weakness can be appreciated during the surgery and distraction can be carried out, even when the radiological images are not confirmatory.

The strong bone quality of the articular lateral mass can be exploited effectively in providing stability to the spine and restoring the alignment and curvature of the spine. Although fixation of the lumbar spine by pedicle screws is a rather straightforward and effective method of stabilizing the segments, cervical fixation by means of facet or pedicle screws is neither easy nor always safe for the vertebral artery or for the segmental nerves. The inherent segmental elasticity holds the spacer within the articular cavity by itself, and it provides strong immobilization as it is placed at the site of fulcrum of all movements in the spine and presents a reliable ground for fusion. We had labeled a similar technique used for stabilization of the craniovertebral junction as "atlantoaxial joint jamming."[2] When multilevel fixation is attempted by any procedure, alterations in the spinal curvature can be expected. To make the spine flat when 4-level lumbar fixation is being done, as in the case demonstrated in our publication, might be the goal of surgery or intentional, and it is certainly far from being unusual, abnormal, or pathological.

Although we do not dispute the mathematical analysis, for a better and more clinically relevant understanding of the effects of using facet distraction disc-spacers across the opposing articular surfaces of a facet joint, we attempted to simulate the procedure in an experimentally validated finite element model of a ligamentous spinal unit (say L4–5). Such a model simulates the orientation of the facets, disc, ligaments, and other spinal elements in an anatomically correct manner. The approach allows one to assign experimentally determined appropriate material properties to these structures. The model enables one to simulate the surgical procedure to place a device such as the facet distraction disk-spacers. I asked Vijay K. Goel, Ph.D. (Co-Director, Engineering Center for Orthopaedic Research Excellence, University of Toledo, Toledo, Ohio) to provide the analysis using his experimentally validated finite element model of the spine.[1,8–11] The simulation and results are presented in Figs. 1 and 2.

The intact model was modified as follows:

- The spinous process of the superior level (L-4) was lifted to open up the gap between the L4–5 facets for insertion of the spacers.
- The articular facet ligaments/cartilage were completely removed.
- The spacers were inserted bilaterally, the lateral face of the spacer was fixed to the lateral face of the facet joint, and a contact was simulated between the medial face of the spacer and the associated contact area at the facet joint.
- The superior spinous process was then released until the contact between the spacers and the facet joint was established.

The resulting changes in the intervertebral foramen height, area of the canal, and the anterior and posterior

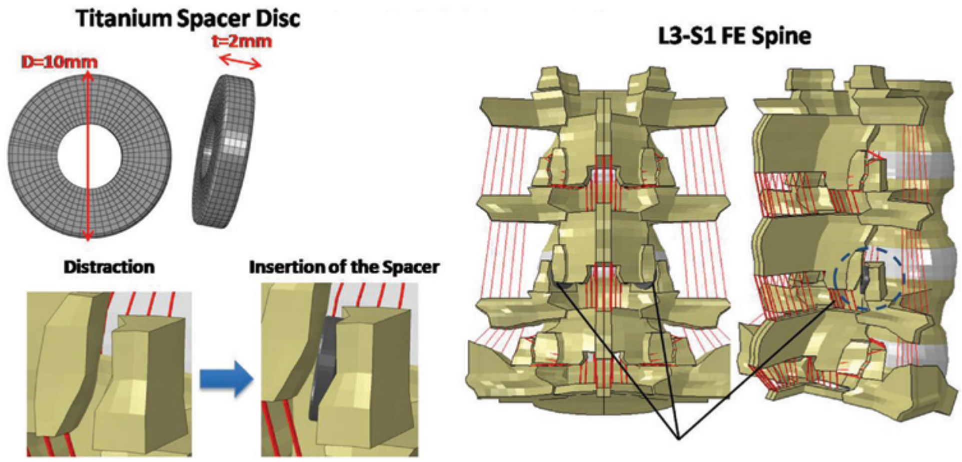

Fig. 1. The ligamentous finite element (FE) model of the L3–S1 segment showing various spinal structures. The placement of the interarticular spacer is also shown.

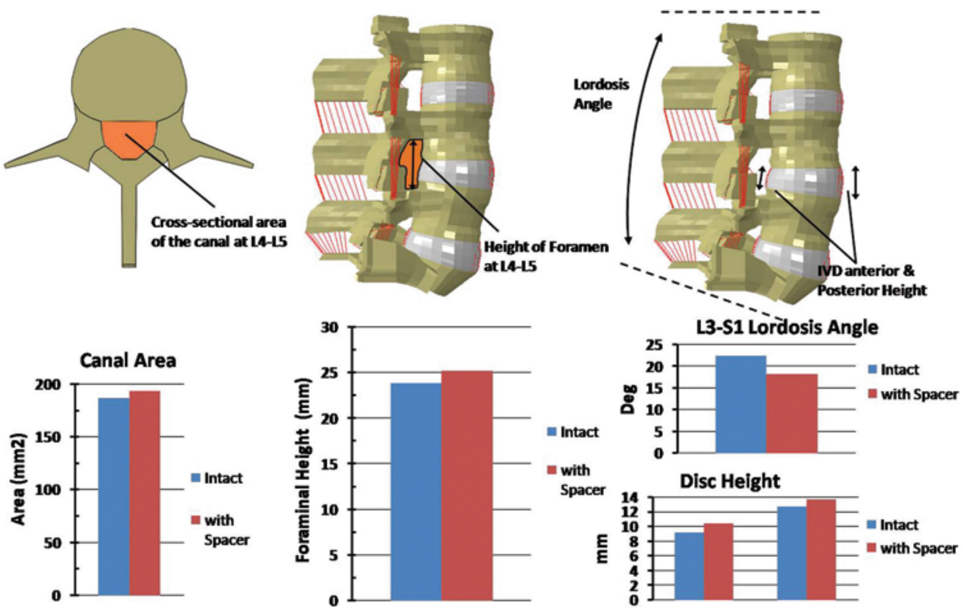

Fig. 2. The parameters computed are shown. The lordosis angle decreased while all of the other parameters increased following "disc" placement. IVD = intervertebral disc.

disc heights are shown in the figures. The disc height increased from 9.2 mm to 10.4 mm at the posterior side and from 12.7 mm to 13.7 mm at the anterior side. The lordosis angle across the L3–S1 segment decreased from 22.5° to 18.2°. Accordingly, the height of the intervertebral foramen increased from 23.9 mm to 25.1 mm. Likewise, the area of the spinal canal increased from 186.7 mm^2 to 193.2 mm^2, while the lordosis angle decreased (Fig. 2).

The outcome of treatment speaks for its effectiveness. The authors of the letter are not satisfied with the usability of VAS and ODI as indices in lumbar canal stenosis. It certainly seems to be a personal opinion or bias. There are a large number of articles on the subject of lumbar canal stenosis in the literature that base their analysis on these scores. These scores are considered to be standard parameters and are in universal use. The authors have mentioned some additional parameters to assess claudication pain and radiating pain. However, the superiority of these parameters over the standard and accepted monitoring scores can only be debated.

I have no hesitation in stating that facet distraction has the potential of revolutionizing spinal stabilization techniques.[4]

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Please include this information when citing this paper: published online October 11, 2013; DOI: 10.3171/2012.8.SPINE12689b.

Surgical seroma

To The Editor: We read with great interest the article by Yew et al.[8] published in the *Journal of Neurosurgery: Spine* (Yew A, Kimball J, Lu DC: Surgical seroma formation following posterior cervical laminectomy and fusion without rhBMP-2. Case report. *J Neurosurg Spine 19:*297–300, September 2013). The authors are to be congratulated for bringing attention to this issue. Following the approval of recombinant human bone morphogenetic protein–2 (rhBMP-2) by the Food and Drug Adminsitration for anterior lumbar interbody fusion in 2002, there has been significant off-label usage of this product in spine surgery to enhance fusion rates. Adverse effects have been reported with its use for anterior cervical fusion,[2] including swelling, dysphagia, and hematoma and seroma formation. Seroma formation with use of rhBMP-2 has also been reported following posterior cervical procedures.[1,3]

The authors present a case of seroma formation after

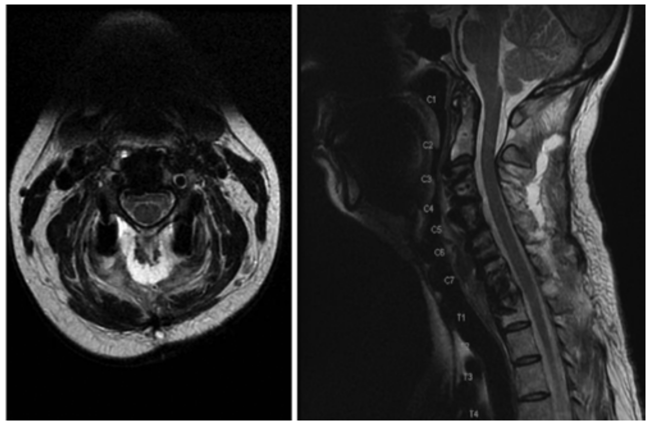

Fig. 1. Case 1. Postoperative axial **(left)** and sagittal **(right)** T2-weighted MR image of the cervical spine demonstrating a T2 hyperintense fluid collection overlying the C2–6 levels consistent with a seroma.

posterior cervical laminectomy and fusion in the absence of rhBMP-2 usage. We applaud the authors' timely diagnosis and treatment of their patient as well as the review of this condition, which obviously has clinical implications. We are a bit surprised that non–rhBMP-2 related surgical seroma formation following posterior cervical spine surgery is considered to be so novel. We have seen this complication many times over the years and would like to share with the readers our experience with two recent patients with surgical seroma formation after posterior cervical spine fusion and instrumentation in the absence of rhBMP use (Figs. 1 and 2). Both of our patients had surgical drains placed, with use of the drains subsequently discontinued after diminished output, as reported by Yew et al.[8] The first patient was a 37-year-old woman with pseudarthrosis following prior C3–T1 anterior fusion who underwent posterior C3–T1 instrumentation and fusion with allograft without rhBMP-2; she presented with persistent neck and scapular pain 2 weeks after surgery and was found to have a surgical seroma. Due to concern for pain and occult infection, the seroma was drained. Her symptoms resolved after surgical evacuation of the seroma. The second patient was a 60-year-old woman with a prior traumatic cervical cord injury and resultant quadriparesis and a history of pseudarthrosis following prior C5–7 anterior fusion and instrumenta-

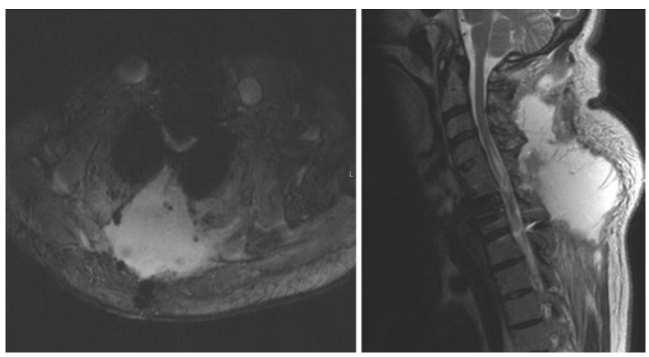

Fig. 2. Case 2. Postoperative axial **(left)** and sagittal **(right)** T2-weighted MR image of the cervical spine showing a significant fluid collection overlying the C2–T1 levels consistent with a seroma.

tion, who underwent a C5–T1 posterior cervical fusion and instrumentation with use of cortico-cancellous allograft. She presented with persistent pain and a bulging mass under the posterior cervical incision. Her pain also improved with surgical evacuation of the seroma.

Although uncommon, seroma formation following surgical procedures is very well known. "Morel-Lavallée seroma" can form following trauma or surgical procedures and is an entity first described in 1863.[6] The pathogenesis of postsurgical seroma formation has been attributed to accumulation of blood, lymph, and necrotic fat in the dead space(s) created from the surgical procedure.[6,7,9] Granulation tissue can form around the fluid collection to form a fibrous capsule which further impairs absorption of the seroma. Females are estimated to be 12 times more likely to have this type of seroma formation than are males.[5,6,9] The increased incidence may be due to the looser connective tissue in which fat is distributed, with is more prone to surgical trauma.[4] Though unproven, it is our belief that seroma formation after posterior cervical approaches may be more common after placement of instrumentation because the prominence of the instrumentation increases the potential dead space. Meticulous layered closures and postoperative wound drainage may help to reduce the incidence of seromas after such procedures. Symptomatic patients may present with persistent/recurrent pain, neurological deficits, or incisional drainage that usually prompts surgical evacuation and revision of the wound.

Surgical seroma is an important entity that spinal surgeons need to recognize with or without rhBMP-2 use. Drainage of clinically relevant seromas can be expected to result in prompt resolution of symptoms.

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Disclosure

Dr. Traynelis reports a consultant relationship with, stock ownership in, and receiving study support from Medtronic and institutional fellowship support from Globus.

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

Please include this information when citing this paper: published online September 27, 2013; DOI: 10.3171/2013.7.SPINE13655.

Osteoporosis model

To The Editor: We read the paper by Lee and colleagues[2] (Lee CY, Chan SH, Lai HY, et al: A method to develop an in vitro osteoporosis model of porcine vertebrae: histological and biomechanical study. Laboratory investigation. *J Neurosurg Spine 14:*789–798, June 2011) with a great interest. The authors presented an in vitro model for porcine osteoporotic vertebrae in their article. They indicated that their model is that of an "osteoporotic" vertebra; however, we do not agree with their characterization. The vertebrae created with the proposed method are not "osteoporotic" vertebrae but, rather, are demineralized vertebrae. In order to truly mimic osteoporosis, the collagen content, and in fact the structure of the trabeculae itself, would require removal. In the authors' model, unlike osteoporosis, the structure of the trabeculae remains after decalcification. There are fundamental differences between demineralized vertebrae, which have a reduction in bone mineral density (BMD) after removal of the mineral content, and true osteoporotic vertebrae, in which measured BMD changes represent primarily a physical removal of bone stock. That is, in osteoporosis the volume of bone matrix is reduced, and the BMD changes are a secondary measure of a primary architectural change. This difference is an important nuance, and it has been throughly discussed in our previous study,[1] which was probably overlooked by the authors. In our 2008 article, we described in vitro acid demineralization of the vertebrae with a stronger acid than the EDTA used by Lee et al. We perfused the acid solution into an entire vertebra using an infusion pump. In this way, the acid solution could be delivered to nearly all the trabecular bone in a vertebra and demineralization time could be decreased within hours.

Consequently, the principal methodology of the model proposed by Lee et al. is well known. However, the histology and morphology associated with the vertebral model presented in the article is more consistent with osteomalacia than osteoporosis, the biomechanical properties that exist in osteopenic or osteoporotic vertebrae.

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Disclosure

The authors report no conflict of interest.

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Response: According to our studies, decalcified porcine lumbar vertebrae (Fig. 1B and D) treated for 2 months have significantly less mineralized bone in the trabecular area, with more space interspersed throughout, suggesting less density overall compared with the normal vertebrae shown in Fig. 4A and B and Tables 3 and 4 of our original paper. This observation is in agreement with dual energy x-ray absorptiometry (DEXA) and micro-CT studies of porcine lumbar vertebrae that demonstrated reduced BMD and reduced trabecular and cortical thickness of the decalcified vertebrae (also shown in Fig. 3 of the original paper).

We fully agree with Drs. Akbay and Akalan that our decalcified porcine lumbar vertebrae are a type of demineralized vertebrae similar to those shown in other studies.[2,3]

Although different decalcified vertebra methods have been described, none has shown evidence of decreased collagen content.[1–3] In some of the reports, destruction was required of part of the vertebral body to facilitate the decalcification process, making the biomechanical study more unreliable.[3] However, in our study, those sections of porcine lumbar vertebra with 2-month decalcification stained with Masson trichrome and Alcian blue (pH 2.5) stain showed less epiphyseal plate density and less collagen content than the normal vertebrae (Fig. 1). The decalcified vertebrae at 2 months (Fig. 1B and D) had less overall blue staining, which indicates a lower-density epiphyseal plate with less collagen present compared with the normal vertebrae (Fig. 1A and C). The method that we described for the in vitro decalcified porcine vertebra model not only causes loss of trabeculae in vertebrae but also causes a loss of collagen content.

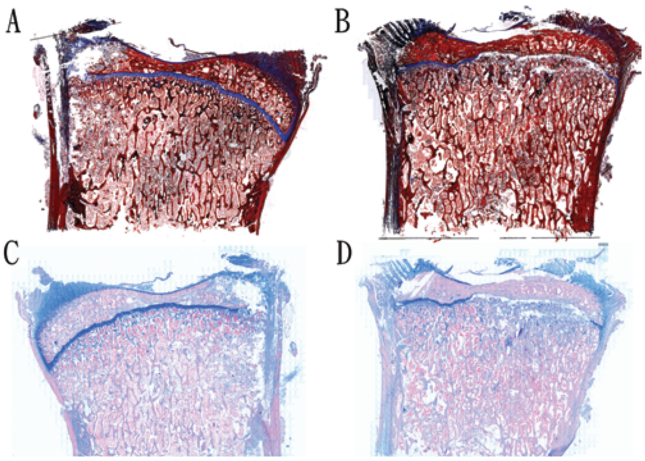

Fig. 1. Photomicrographs of porcine lumbar vertebrae. Normal and decalcified lumbar vertebrae were sectioned and stained with Masson trichrome and Alcian blue (pH 2.5) stain to detect collagen *(blue)* in the epiphyseal plate of normal **(A and C)** and decalcified **(B and D)** lumbar vertebrae. Collagen staining in the epiphyseal plate of decalcified lumbar vertebrae was less intense and less dense suggesting an overall decrease in the amount of collagen in the growth plate.

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Please include this information when citing this paper: published online October 11, 2013; DOI: 10.3171/2011.8.SPINE11521.

Sagittal plane analysis

To The Editor: We would like to comment on the article by Lafage and colleagues[1] (Lafage V, Schwab F, Vira S, et al: Does vertebral level of pedicle subtraction osteotomy correlate with degree of spinopelvic parameter correction? Clinical article. *J Neurosurg Spine 14:*184–191, February 2011) published in the *Journal of Neurosurgery: Spine*.

The authors aim to answer the question whether a lumbar pedicle subtraction osteotomy (PSO) performed more distally in the spine results in a more pronounced alteration of the sagittal balance. To answer the question, they retrospectively reviewed the radiological results of PSOs performed at different levels of the lumbar spine in 70 patients. They measured several spinopelvic parameters on full standing radiographs: amplitude of anatomical spinal curves; pelvic incidence (PI) and pelvic tilt (PT); and 2 variables reflecting global spinal sagittal balance—the sagittal vertical axis (SVA) and the T-1 spinopelvic inclination. The osteotomies were performed by different surgeons and at different levels of the lumbar spine: L-1 in 6 patients, L2- in 15, L-3 in 29, and L-4 in 20 patients.

We can summarize their findings as follows: the mean segmental correction angles obtained were not significantly different with regard to the anatomical level of PSO (range 22°–25°). Statistically significant improvements from preoperative to postoperative were shown in all patients for all the other variables measured. More specifically, the SVA and the T-1 spinopelvic inclination were significantly decreased postoperatively, an indication that the postoperative global sagittal spinal balance was improved.

The authors conclude that a PSO, when properly planned and executed, significantly improves the sagittal balance of patients. They, however, also conclude something that is a bit surprising: the decrease in the SVA was not correlated to the level of PSO. In other words, a PSO performed more proximally in the lumbar spine is, according to them, as efficient in restoring the global balance as one performed more distally.

We think that several aspects of the authors' interpretations should be discussed.[2–4] First, when analyzing spinal sagittal balance, it is more suitable to define spinal curves from a biomechanical point of view rather than an anatomical one. The transition between lumbar lordosis and thoracic kyphosis is called the inflexion point and it is anatomically variable, depending on specific spinopelvic parameters.[3] Second, the authors found no correlation between SVA decrease and level of PSO. They did, however, find a statistically significant correlation between a more distal PSO and a greater decrease in PT.

In a sagittaly unbalanced spine, a postoperative decrease of PT is a sign of improved sagittal balance. However, this improvement in sagittal balance is not adequately reflected in SVA alone, which only measures a forward displacement of the C-7 plumbline, in centimeters. A variable that integrates both the forward displacement of the C-7 plumbline and the spatial position of the pelvis (reflected in the PT) would better describe the global sagittal balance of the spine. This variable exists and has been called the spinosacral angle (SSA).[3] The SSA is measured at the intersection of a line traced from the center of the C-7 vertebral body to the posterior angle of the S-1 endplate, and a second line that is tangent to the S-1 endplate (reflecting the sacral slope) (Fig. 1). The mean SSA angle in the asymptomatic population is 131° ± 8°.[2] It decreases when the spine is sagittaly out of balance and increases when the spine is better balanced again after a corrective surgery. For this reason, the SSA is a better indicator of restored postoperative sagittal balance than the SVA alone. In their study, Lafage and colleagues found a greater PT decrease when the PSO was done more distally in the lumbar spine. If they had measured the SSA, they could have shown that a more distal osteotomy does in fact result in a better sagittal balance correction than a more proximal one.

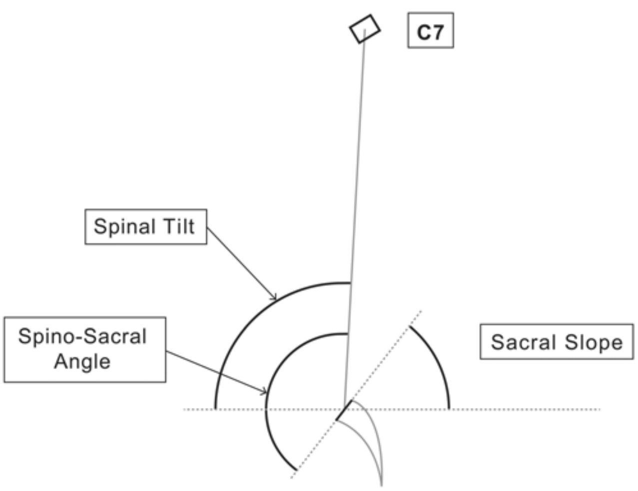

Fig. 1. Three important sagittal balance angles: the spinal tilt, the sacral slope, and the SSA, which is the most holistic measure of global sagittal balance.

Last, another detail is missing in the material section: there are no data about the flexibility of the upper curve at the thoracic or thoracolumbar junction. This is important to evaluate preoperatively because compensation phenomena in the upper curve can modify the final balance and change the SVA measure. The SSA, however, is more consistent as it is an intrinsic parameter of the global sagittal balance including spinal and pelvic parameters.

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Disclosure

The authors report no conflict of interest.

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Response: First, we would like to thank the authors of this letter for spending time reviewing and commenting on our article. This letter highlights the complexity of sagittal plane analysis and the confusion among clinicians and scientists trying to address spinopelvic alignment. While we fully acknowledge some of the limitations inherent in every published manuscript, we appreciate the opportunity of addressing point by point the issues raised in this letter.

The results presented in our study may seem surprising as they differ from the common belief that a more caudal wedge resection would lead to a greater correction of the SVA. These beliefs were recently reinforced by the publication of a trigonometric method to calculate the angle of correction needed to achieve neutral alignment for PSO procedures.[5] One shortcoming of this method is that the contribution of the pelvis to sagittal alignment is neglected. Our study demonstrated that the level of the osteotomy correlates with a greater correction of pelvic retroversion (that is, reduction of PT) while no correlation was found between the osteotomy level and the SVA correction. These data-driven results highlight the necessity to take into account the retroversion of the pelvis (a compensatory mechanism) during surgical planning as the resection will lead to a decrease of both the SVA and the PT.

Concerning the measurement of the lumbar lordosis, we are in total agreement with the authors of the letter. Ideally, lumbar lordosis (and thoracic kyphosis) should be anatomically defined by inflexion points. The importance of the maximal lumbar lordosis over the L1–S1 lordosis was recently highlighted in our studies predicting postoperative spinopelvic alignment.[3,4] Using principal component analysis and multilinear modeling, our results demonstrate that maximal lordosis and kyphosis (along with pelvic incidence) were predictors of SVA and PT (r^2 = 0.78 and 0.86). One shortcoming of using maximal lordosis instead of L1–S1 lordosis relates to the increased complexity in reporting the results (one would need to also report the number of vertebrae included in each curvature) and the difficulty in reporting longitudinal changes in lumbar lordosis when considering different end vertebrae at different time points.

Knowledgeably, the authors of the letter are suggesting the integration of global alignment and pelvic position into a single parameter (SSA). While the definition of this parameter could be improved (by taking into account directly the PT instead of the sacral slope, a parameter highly dependent on the pelvic morphology in asymptomatic individuals), it is nevertheless a good first approach in defining a global parameter. We respectfully disagree with the authors and their suggestion that the use of the SSA would have been necessary to demonstrate a correction of the spinopelvic alignment. While using the SSA would offer additional perspectives on alignment change, our results already demonstrate a change in the sagittal plane affecting the trunk as well as the pelvis (that is, correction of the SVA and pelvic retroversion).

A global understanding of realignment must span a framework encompassing lumbar and thoracic osteotomies. It has emerged that in both instances reciprocal changes can occur in unfused segments[1] (sometimes with a negative impact[2] on spinopelvic alignment).

Seemingly suggested by the authors, one solution to appreciate reciprocal changes would be to estimate the sagittal flexibility of the unfused thoracic segments based upon flexion-extension radiographs. Due to the nature of this retrospective study, and the lack of flexibility radiographs in clinical practice, we were unable to integrate this parameter. Unfortunately, no radiographic parameters (including the SSA) would be able to overcome this limitation.

Summarizing the interesting exchange related to our article, it is imperative that surgeons recognize the complexity of sagittal plane stance in patients. The interplay of pelvic position with truncal position is a key element to be recognized in realignment procedures. Our work demonstrates that while osteotomies affect both the trunk and pelvic version, the lumbar level of a PSO does not directly correlate with change in SVA.

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Please include this information when citing this paper: published online October 11, 2013; DOI: 10.3171/2011.10.SPINE11306.