Neurosurgical Forum

LETTERS TO THE EDITOR

S-1 alar-iliac screw technique: nothing new under the sun

TO THE EDITOR: In a technical note published in 2013 in this very same journal, we first described the technique for sacral-alar iliac (SAI) screw fixation using an entry point in S-1 (the so-called “S-1 alar-iliac screws”; Figs. 1 and 2), along with several clinical examples in which this technique was successfully employed.² Five years later, without appropriate reference to the original publication, DePasse et al.¹ published another technical note presenting exactly the same technique (DePasse JM, Valdes M, Palumbo MA, et al: S-1 alar/iliac screw technique for spinopelvic fixation. *J Neurosurg Spine* 28:543–547, May 2018).

I am fully confident that the authors’ failure in properly acknowledging the original description of such a tech-

nique was unintentional and a mere product of the lack of a proper review of the literature on the issue.

Ultimately, the contribution from DePasse’s team amounts to a well-illustrated and educational article, perhaps just not as innovative as originally intended by the authors. In addition to re-demonstrating the feasibility of SAI screws with an entry point in S-1, this study also showed (although based on a single-specimen analysis) that such a technique is biomechanically very robust, ultimately constituting an interesting option (either by itself or in combination with S-2 alar-iliac screws as presented in our original article) for pelvic fixation in long posterior thoracolumbosacral constructs.

It is actually a great delight to observe that the technique we first described 5 years ago has found widespread favor in the eyes of the spine surgery community (likely not “because” of our humble past contribution, although it would have been polite as well as scientifically advisable

FIG. 1. A: Illustration of the entry points and ideal trajectory of the S-1 (upper arrow) and S-2 (lower arrow) sacral-alar iliac screws. **B and C:** Postoperative anteroposterior (AP) and lateral plain radiographs after a revision surgery for pseudarthrosis in a patient previously submitted to pelvic fixation using classic iliac wing screws; the revision surgery involved placement of combined S-1 and S-2 alar-iliac screws. Note the trajectory of both screws converging toward the upper limit of the sciatic notch on the AP film (B). Reprinted with permission from Mattei TA, Fasset DR: Combined S-1 and S-2 sacral alar-iliac screws as a salvage technique for pelvic fixation after pseudarthrosis and lumbosacropelvic instability. *J Neurosurg Spine* 19:321–330, 2013. Figure is available in color online only.

FIG. 2. Depiction of the posterior anatomy of the sacrum as well as important landmarks for the placement of the combined S-1 and S-2 SAI screws. The entry point for the S-2 SAI screw is located between the S-1 and S-2 foramina and at the level of the lateral sacral crest. The entry point for the S-1 SAI screw is also located at the level of the lateral sacral crest but between the S-1 foramen and the S-1 superior articular facet. Reprinted with permission from Mattei TA, Fasset DR: Combined S-1 and S-2 sacral alar-iliac screws as a salvage technique for pelvic fixation after pseudarthrosis and lumbosacropelvic instability. *J Neurosurg Spine* 19:321–330, 2013. Figure is available in color online only.

on the authors' part to have properly recognized its primacy), ultimately having been successfully used by several groups for the treatment of such challenging cases.

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Response

Dr. Mattei is absolutely right. As described in our paper, we developed this technique when treating a patient in whom we could not place S-2 alar-iliac screws, and then we found it a useful adjunct in select patients. When searching the literature for relevant previous publications,

we missed Dr. Mattei's well-written description of the technique due to the presence of dashes in the title that eliminate it from many PubMed searches, and we apologize for our oversight in failing to recognize his article as the first to describe this trajectory. We agree wholeheartedly with Dr. Mattei regarding the value of this surgical option, and we hope our article's biomechanical analysis and additional cases will increase confidence in its feasibility and contribute to its widespread utilization.

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Sagittal balance in adult spinal deformity

TO THE EDITOR: We read with great interest the article by Smith et al.⁹ (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), and the subsequent Letter to the Editor by Tessitore and Gautschi¹⁰ (Tessitore E, Gautschi OP: Adult spinal deformity surgery: is it always worthwhile? *J Neurosurg Spine* 26:408–409, March 2017).

Smith et al.⁹ world-renowned experts in the field, conducted a multicenter study with a large cohort of patients to prospectively assess the rates of complications associated with adult spinal deformity (ASD). The study concluded that spinal surgery for ASD is associated with an extremely high risk of perioperative and delayed complications (469 complications, 207 minor and 262 major, occurred in 203 of 291 patients; 1 revision surgery in 82 patients). Additionally, the study had only a 2-year follow-up. The risk of complications in a study with long-term follow-up might be considerably higher.

Tessitore and Gautschi,¹⁰ taking a cue from this study, highlighted that the majority of patients seeking medical advice for sagittal imbalance problems present with one or more factors associated with an increased complication rate (they are often elderly, obese, possess significant comorbidities, and have previously undergone back surgery). In the light of the extremely high risk of perioperative and delayed complications, they considered unavoidable the question: is surgery always worthwhile in the event of ASD problems? The best way to minimize the risk of complications or an unfavorable outcome seems to be meticulous patient selection.⁷

In an attempt to master the etiopathogenesis of ASD and sagittal imbalance, recent studies have suggested that trunk muscle strength is inversely correlated with the sagittal lumbar curve and that a positive relationship exists between trunk strength and sagittal balance.³ Trunk muscle strength can impact sagittal balance, particularly when

balance distortion is attributed to lumbar curve deviation.³ Even in the literature, it is currently accepted that the static and dynamic balance of the trunk, even in healthy subjects, gradually deteriorates with age,⁶ and that this correlates with the physiological decrease in trunk muscle strength and mass.⁵

These considerations appear to be even more important in the light of human body evolution. The modern human spine has adapted poorly to recent environmental conditions; humankind is the victim of a recent mismatch, which is exacerbated as sedentary behaviors increase. Since prior to the post-industrial era hominins were very active,⁸ low levels of physical activity and abnormal spinal loading may result in weak, unstable back tissues and an increased risk of pain and injury. Support for this comes from evidence that decreased back muscle strength and endurance strongly correlate with degenerative spine pathology.¹

In this scenario, we have furthermore to consider that the paravertebral muscle damage originating from conventional posterior instrumented spine surgery produces muscle atrophy and decreased muscle strength.^{2,4} The damage becomes more severe with operations that require long periods of muscle retraction.⁴ Over the years, this concern has led to a less invasive surgical approach being attempted in order to reduce the paravertebral muscle damage.²

In conclusion, trunk muscle mass and the strength it provides seem to be fundamental in maintaining sagittal balance. Is the current tendency to perform extremely aggressive spine surgery the suitable solution for ADS? We think that the answer may be found in the extremely high rates of complications.

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Response

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Cervical intramedullary tumor resection and kyphotic malalignment

TO THE EDITOR: We read with great interest the article by Nori et al.⁴ (Nori S, Iwanami A, Yasuda A, et al: Risk factor analysis of kyphotic malalignment after cervical intramedullary tumor resection in adults. *J Neurosurg Spine* 27:518–527, November 2017). They concluded that the main risk factors for cervical spine kyphosis after surgery for cervical intramedullary tumors in adults are atrophy of the deep extensor muscles (DEMs) after surgery and detachment of the DEMs from the C2 spinous process.^{3,5,6} We commend the authors for performing this study on a topic that is very much clinically relevant. We would like to highlight certain important issues related to the paper.

The authors concluded that the cervical spinal alignment became kyphotic after tumor excision in the upper tumor group. However, it would be worthwhile to know whether any of these patients deteriorated clinically with kyphotic malalignment. What is the average time period when patients developed kyphosis following surgery? Did any of these patients need surgical spinal stabilization? In our experience, rarely do patients with cervical intramedullary tumors who develop radiological kyphotic deformity present with clinically significant symptoms in the postoperative period. And even rarer is the need for surgical stabilization for these cases.

We think that extensor muscle atrophy results from the spinal cord damage, which occurs during intramedullary tumor excision as well. This will result in some change

in the cervical spinal alignment.^{1,2} Hence, it would not be right to ascribe all the changes in the cervical spinal alignment to laminectomy/laminoplasty/muscle dissection.

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Response

We thank Drs. Sreenivasan and Garg and colleagues for their comments and would like to take this opportunity to respond.

The purpose of our study was to investigate the changes in cervical spinal alignment after surgery for cervical intramedullary spinal cord tumors (CISTs) in adults. We did not focus on postoperative kyphotic deformity, which is usually defined as actual kyphosis of the cervical spine and requires cervical fixation surgery.^{4,5} Figure 3A clearly showed that the mean C2–7 angle in the upper tumor group was 15.3° preoperatively and 9.5° postoperatively. Although we observed the postoperative decrease in cervical lordosis (–5.8°), kyphotic deformity was not observed. As we mentioned in our paper, “no patient required deformity correction surgeries for cervical malalignment after tumor resection during the study period.” We agree

that adult patients with CISTs rarely develop postoperative kyphotic deformity accompanied by clinically significant symptoms or requiring cervical fixation surgery. However, kyphotic deformity is not the main subject of our study. Our main conclusion was that preservation of the DEMs, especially those attached to the C2 spinous process, was important for the prevention of kyphotic change of the cervical spine after CIST surgery. This conclusion is consistent with those in studies of laminectomy/laminoplasty for cervical spondylotic myelopathy.^{1,2}

As described in the *Methods*, we compared lateral cervical radiographs at the final follow-up at least 1 year after surgery with those obtained preoperatively. Thus, for most patients, kyphotic changes developed within 1 year after surgery. Since we did not monitor cervical alignment during the intervening year, we could not determine the average time after surgery when these kyphotic changes developed.

We did not conclude that “all” the changes in cervical spinal alignment were due to DEM dissection. Among our analyzed factors (atrophy of the DEMs, tumor location, detachment of the DEMs from the C2 spinous process, the C2–7 angle before surgery, age at surgery, tumor histology, patient sex, tumor size, number of laminae affected, and Japanese Orthopaedic Association [JOA] score), atrophy of the DEMs and detachment of the DEMs from the C2 spinous process were significantly associated with the risk of kyphotic change after surgery. We agree that extensor muscle atrophy can result from spinal cord damage, which can occur during tumor excision. However, there are four reasons that suggest that muscle dissection has a greater influence than spinal cord damage on changes in cervical alignment.

- 1) Tumor size was not related to the cervical kyphotic change. Gross-total resection was performed for all the patients in this study. It is natural to think that tumor size relates to spinal cord damage. However, tumor size was not related to the cervical alignment change.
- 2) The JOA score was not associated with cervical kyphotic change. The JOA score reflects spinal cord damage. However, we did not observe any relationship between JOA score and cervical alignment.
- 3) The postoperative change in the C2–7 angle differed between the upper tumor group (–5.8°) and the lower tumor group (+6.4°). Because there was no difference in tumor size between these two groups, we think that the surgical level of muscle dissection affected the cervical alignment change. If extensor muscle atrophy resulting from spinal cord damage were the primary cause of postoperative kyphotic change, it would not explain why the postoperative lordotic change was observed in only the lower tumor group. Similar alignment changes were also reported in a previous study.³
- 4) The postoperative change in the C2–7 angle differed between the upper tumor patients with detachment of the DEMs from the C2 spinous process (–8.4°) and those with non-detachment of the DEMs (+6.3°). As in item 3 above, the spinal cord damage theory would not explain the postoperative lordotic change observed in the non-detachment group.

The major limitation of our study was that it was retrospective and subject to selection bias. Another limitation was the small sample size. To confirm our results, further studies with larger patient populations will be required.

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