Neurosurgical Forum
LETTERS TO THE EDITOR

Alternative C-1 screw placement technique

TO THE EDITOR: We read with great interest the article by Moisi et al.¹ and agree with their conclusions regarding the safety of this screw trajectory compared with the traditional Harms technique (Moisi M, Fisahn C, Tkachenko L, et al: Posterior arch C-1 screw technique: a cadaveric comparison study. *J Neurosurg Spine* 26:679–683, June 2017). In their cadaveric study, the authors demonstrated fewer medial breaches with the posterior arch approach than with the Harms technique when a group of spine fellows placed the screws under fluoroscopic guidance. The technique does not require entry into the rich venous plexus surrounding the C-2 ganglion or sacrifice of the ganglion.

This has been our experience clinically and was described by us in detail in a series of 26 patients in 2010.² The C-2 ganglion was preserved in all cases, and there were no cases of vertebral artery injury, durotomy, hardware malposition, or fracture. The anatomical landmarks for screw placement were also clearly described in our paper.

Confirmation that the posterior arch technique can provide a safer alternative for the placement of C-1 screws in a laboratory setting is certainly a noteworthy finding. However, we are disappointed that the authors failed to acknowledge our previous contribution, which is the initial description of this alternative technique and with which we have obtained excellent clinical results. We continue to employ the alternative C-1 screw placement technique and have had no case requiring sacrifice of the C-2 ganglion or injury to the vertebral artery.

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2. Thomas JA, Tredway T, Fessler RG, Sandhu FA: An alternative method of placement of C1 screws: technical report and case series. *J Neurosurg Spine* 12:337–341, 2010

Disclosures

The authors report no conflict of interest.

Response

We would like to thank Drs. Sandhu and Fessler for their comments on our cadaveric comparison study. The C-1 vertebra is indeed a unique anatomical entity. In preparation for our work, we found a number of authors who, over the years, have suggested different forms of segmental C-1 fixation in order to provide a stable environment for bony healing. Of course, formulating a safe and reproducible method for segmental fixation of this challenging vertebral segment is in our communal interest as spine surgeons.

The goal of our study was to compare the safety of 2 basic posterior atlas fixation techniques, one attributed to 2 different authors—Harms and Goel—and another that has in some variation been described by a number of authors over the years, beginning in 2009 with Senoglu and Gumusalan.^{1–5,7} To avoid conflict of attribution, we have simply used an anatomical term, “posterior arch lateral mass” (PALM) in the naming of our technique and use this technique as an alternative to conventional lateral mass screw placement. We acknowledge and thank Drs. Sandhu and Fessler for their helpful clarification as to their previous clinical work using the PALM technique⁶ and hope our readers will find our comparison study of these 2 techniques helpful in enhancing their proficiency in both.

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Treatment of thoracolumbar burst fractures: extended follow-up of a randomized clinical trial comparing orthosis versus no orthosis

TO THE EDITOR: I read with interest the recent article by Urquhart et al.⁴ (Urquhart JC, Alrehaili OA, Fisher CG, et al: Treatment of thoracolumbar burst fractures: extended follow-up of a randomized clinical trial comparing orthosis versus no orthosis. *J Neurosurg Spine* 27:42–47, July 2017). The authors report that there was no loss of spinal alignment, no increased risk of neurological deterioration, nor any worsening of pain and functional outcome between patients with thoracolumbosacral orthosis (TLSO) and those with no orthosis (NO) over the 2-year follow-up period in this study. Overall results appear not to demonstrate any superiority of the TLSO group over the NO group, even with a theoretically higher risk of developing deficits as a result of the early ambulation of patients in the NO group. Avoidance of early ambulation to prevent further axial loading within the cast has been the recommendation for patients being treated conservatively for thoracolumbar burst fractures.³ This raises the question as to whether the use of an orthosis in the management of this group of spine patients really has any benefit, especially in view of its own challenges, with extra cost to the patient, long duration of discomfort from the splinting effect, and even development of ulcers over pressure points within the TLSO cast, and so on. The work of Urquhart et al., in a way, represents an important and significant contribution to our understanding of the pathophysiology of burst fractures in the thoracolumbar segment of the spine, and thus should be taken into consideration in the creation of future guidelines for managing these patients.^{1,2} Unfortunately, however, the patient sample size in this study is quite small. Additionally, the authors did not clearly define exactly how immediate the mobilization by the physiotherapist was for the early ambulation of patients in the NO group. A larger cohort and randomized controlled study will probably be required to objectively validate this interesting finding.

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Disclosures

The author reports no conflict of interest.

Response

Thank you for inviting a response to the letter written by Dr. Chiazor U. Onyia, and to Dr. Onyia for his thoughtful comments regarding our study. We especially appreciate Dr. Onyia's insight into the potential significance of the study: "The work of Urquhart et al., in a way, represents an important and significant contribution to our understanding of the pathophysiology of burst fractures in the thoracolumbar segment of the spine, and thus should be taken into consideration in the creation of future guidelines for managing these patients." We have always believed that after our initial randomized study and this follow-up, the approach to the treatment of these fractures needs to change.¹ Perhaps even more poignant is the impact these studies could have in regions of the world in which bracing and surgery are not available and prolonged bed rest is the only option.

We agree with Dr. Onyia's assertion that not using the TLSO for the treatment of a thoracolumbar burst fracture is a reasonable option, particularly when considering the associated downsides of cost, inconvenience, and potential morbidity. Our work (Urquhart et al.) demonstrates that the treatment outcome of a thoracolumbar burst fracture is equal, irrespective of the use of the TLSO for our study-defined population. The population described in this paper is an extended follow-up of patients from a single center that participated in the original multicenter trial.¹ Dr. Onyia has appropriately drawn attention to the relatively smaller number of patients included in our extended follow-up study. The original multicenter cohort study published in 2014, however, was sufficiently powered and concluded there was a similar equivalence in outcome between the TLSO and NO cohorts for as long as 2 years.¹ Therefore, we do not believe that another randomized control trial is necessary before this treatment approach can be incorporated into treatment guidelines.

With respect to the question as to how quickly patients were mobilized in the NO cohort, there was no specified

time regarding physiotherapist-supervised mobilization. Physiotherapists were instructed to mobilize patients in a neutral spine as soon as tolerated, and in many instances the patients were mobilized the same day of their admission to hospital.

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Symptomatic degenerative lumbar disease and obesity

TO THE EDITOR: We read with great interest the article by Joseph et al.² (Joseph JR, Farooqui Z, Smith BW, et al: Does clinical improvement of symptomatic degenerative lumbar disease impact obesity? *J Neurosurg Spine* 26:705–708, June 2017). We commend the authors for presenting the results of their study on a topic that is clinically relevant. Indeed, many patients ascribe their inability to lose weight to back pain. One would therefore expect patients to lose weight after successful spine surgery. However, the relationship between obesity and back pain is not as simple as has been concluded by many other studies.^{1,3,4}

We think that the present study used extremely simple methodology to test a “hypothesis.” The authors’ results could not be generalized at all for numerous reasons: lack of a priori hypothesis, small sample size (did not describe adequate sample size), did not control for diet and exercise (pre- and postsurgery), did not assess the motivation of patients to lose weight, did not control for medications used (which may lead to weight changes), did not control for comorbid medical/surgical/psychiatric conditions (e.g., depression in the setting of chronic pain could lead to decreased motivation to lose weight), and did not control for socioeconomic variables (availability of a gym nearby, money to enroll in a gym, and so on).

We think that motivation for getting spine surgery for pain relief, and to be able to become physically active to lose weight are two mutually exclusive things. All of the patients were obese before surgery, which could mean that most of them were never motivated to exercise and/or use diet modification to lose weight, and so losing it after sur-

gery would have been difficult. It is difficult to say that they became obese due to inactivity resulting from back pain. Similarly, one cannot expect that they should lose weight after becoming physically active after pain relief.

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Disclosures

The authors report no conflict of interest.

Response

We appreciate the comments by Dr. Garg et al. and acknowledge their points about the generalizability of the results found in our study. However, most of their concerns are reflective of the retrospective nature of the study, which was meant to be an observational investigation to determine if obese patients tended to lose weight after symptomatic improvement of degenerative lumbar disease. Consequently, we were unable to control for a variety of the patient factors mentioned by Dr. Garg et al. We do disagree that pain relief and weight loss are mutually exclusive. Significant pain resulting in disability can be a real barrier to weight loss due to decreased activity level. This study was intended to be a “real world” analysis of a common scenario in which many patients report that they are unable to lose weight due to the limitations brought on by their back or leg pain. Whether a structured weight loss program for the motivated patient who symptomatically improves after spine surgery can result in meaningful weight loss was not evaluated in this study, but it warrants further investigation.

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