Letters to the Editor

NEUROSURGICAL FORUM

Alternative approach in Type II hangman fractures

TO THE EDITOR: We read with great interest the article by Buchholz et al.¹ (Buchholz AL, Morgan SL, Robinson LC, et al: Minimally invasive percutaneous screw fixation of traumatic spondylolisthesis of the the axis. *J Neurosurg Spine* 22:459–465, May 2015).

In this article, the authors presented 5 patients with Levine–Edwards Type II hangman's fractures that were treated with minimally invasive percutaneous screw fixation. Initial conservative treatment had failed in 3 patients, was impossible due to neck lacerations in 1 patient, or was likely to fail because of noncompliance with wearing the rigid collar in 1 patient.

We agree with the authors that in these cases there is a good indication for transpedicular screw fixation. Until now, we have performed a similar procedure in 3 patients with Type II hangman's fracture. However, in contrast to the procedure described by Buchholz et al., we placed the screws with neuronavigation (O-arm, Medtronic) using a small (6-cm) midline incision (Fig. 1). In our opinion, this procedure has the advantage that the screws can be placed minimally invasively with direct view of the entry point and anatomical landmarks. The frame for the navigation was placed on the spinal process of C-3 to prevent distortion of the navigation, which might occur if the frame is placed on the spinal process of C-2. A small skin retractor is used (Fig. 1B) to achieve maximal mobility of the skin and muscles during the screw placement. In our opinion, this procedure is safer than that described by Buchholz et al., because the screw can be placed with direct vision of the entry point in the pedicle and the associated

FIG. 1. A: Navigated placement of a transpedicular C-2 screw in a case of Type II hangman's fracture using a small midline incision with direction vision of the screw entry and anatomical landmarks. B: Intraoperative trajectory on the monitor of the navigation system. Figure is available in color online only.

anatomical landmarks. This approach will also result in better control of a possible intraoperative neurovascular complication. Moreover, the size of the incision is more or less the same as the total of 2 lateral stab incisions, as is the total duration of surgery and postoperative time in the hospital.

In addition, we want to advocate the use of lag screws to reduce the fracture displacement. Buchholz et al. mentioned the use, but did not specify in how many cases and why in these cases lag screws were used. We used lag screws in all 3 of our cases, which resulted in early ossification, 3 months after the surgery. An example is provided in Fig. 2.

Finally, we would advise that preoperative MRI be performed in all cases of hangman's fracture to document 1) potential rupture of the C2–3 intervertebral disc (even in

FIG. 2. Transverse CT scans obtained in a 47-year-old woman who was referred to us in 2013, 1 week after a car crash. A: Preoperative image showing a 3-mm fracture displacement. B: Postoperative image demonstrating reduction of the fracture site. C: Follow-up image after 3 months showing complete ossification across the fracture site.

the absence of angulation), 2) possible dissection of the vertebral artery, and 3) the presence and degree of posterior ligamentous injury. In case of rupture of the C2–3 intervertebral disc, there is a high risk for secondary angulation. If so, we would favor C2–3 anterior cervical discectomy with fusion. In cases of unilateral dissection of the vertebral artery we would also perform C2–3 fixation (either anterior or posterior), because of potential contralateral injury in transpedicular screw placement. In case of posterior ligamentous injury, the patient is advised to wear a hard collar for 3 months. Follow-up should be at least 1 year to exclude late-onset C2–3 instability by dynamic radiography.

Although Buchholz et al. presented an interesting case series of minimally invasive percutaneous transpedicular screw placement for Type II hangman's fractures, we do not agree that the advantages of percutaneous placement outweigh the risk of potential neurovascular injury. We presented an alternative minimally invasive procedure, which has the advantage of direct vision of the screw entry point and associated anatomical landmarks.

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DISCLOSURES

Dr. Arts reports the following: consultant for Biomet, InSpine, and Silony; support of non-study-related clinical or research effort from Amedica and Biomet; and patent holder with EIT.

Response

We thank Drs. de Ruiter and Arts for their interest in our article and for raising several important points of discussion. Initially, we began screw fixation of hangman's fractures through the use of open midline incisions, much in the way these authors describe; however, in an effort to reduce the blood loss and postoperative pain associated with this approach, we transitioned to percutaneous screw placement.

We agree that there are risks of neurovascular injury during any posterior cervical spine procedure, open or minimally invasive, and careful attention must be placed on anatomy and technique. Neither their group nor ours reported vertebral artery injury in the small series, but the possibility exists regardless. We disagree with several points raised by Drs. De Ruiter and Arts. First, we doubt that being able to directly visualize the bony surface through the described minimal open incision significantly decreases these neurovascular risks or gives the surgeon a better chance of fixing an injury. Repairing a laceration of the vertebral artery would require a larger exposure than either group describes in order to expose the vertebral artery in or near the foramen transversarium for proximal and distal control as well as suture repair. In addition, it

is a matter of debate whether open or minimally invasive techniques have a higher risk of neurovascular injury. Finally, a 6-cm incision is significantly larger than the 2 stab incisions we describe. We do, however, agree with the utility of MRI in the evaluation of hangman's fractures as well as the usefulness of lag screws and technique.

The use of neuronavigation has been advantageous in allowing surgeons to more accurately place screws with less dissection through both open and percutaneous approaches. The goal of our paper was not to show superiority of a procedure but rather a new approach to an old problem. The patients presented by Drs. de Ruiter and Arts were treated using less invasive open procedures made possible by advancements in neuronavigation, and we are glad to see continued progress in minimizing surgical interventions.

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Treatment of subaxial cervical facet dislocations

TO THE EDITOR: It is with great interest that I read the article by Park et al.⁵ (Park JH, Roh SW, Rhim SC: A single-stage posterior approach with open reduction and pedicle screw fixation in subaxial cervical facet dislocations. *J Neurosurg Spine* 23:35–41, July 2015). This is interesting clinical research, and I believe the technique mentioned in the article can be an alternative to other techniques in some cases. However, I have some concerns.

First, cervical pedicle screw insertion is a technically demanding procedure that not every surgeon can perform because of the small diameter of the pedicle, and the risk of artery and nerve injury is greater than it is when performing mass screw placement.^{1,4} In the authors' cases, lateral mass screw conversion occurred when placing 5 pedicle screws, and lateral pedicle wall perforation occurred when placing 5 screws (total unsatisfactory rate 11.9%, 10 of 84). Thus, this technique may be limited in its application. As we all know, lateral mass screw placement is safer than cervical pedicle screw fixation and anterior decompression is more effective than posterior decompression, and combining the two requires 2 hours, which is similar to the operative duration reported by the authors (mean 133.3 ± 33.42 minutes). So, why not replace a more difficult and risky procedure with the safer and simpler posterior lateral mass screw fixation and anterior cervical discectomy and fusion?² Of course the cost of the latter operation will be much higher.

Second, bilateral facet joints were destroyed during posterior decompression in 3 patients, and the load was totally subjected to the pedicle screws and rods without

anterior support. Thus, the failure rate of instrumentation may be a concern despite its rigidity.³

Third, there were 14 patients without cervical disc herniation. Why not try skull traction after hospitalization or manipulative reduction after general anesthesia? Good preoperative realignment can be properly achieved in the majority of cervical dislocations,⁶ and according to my experience, manipulative reduction after general anesthesia is successful in about 50% of cases.

Fourth, is the 30-mm length of the pedicle screw too long? I would think that the long pedicle screws might disturb the insertion of vertebral screws if the patient needed a further anterior surgery. I usually choose a 20-mm-long pedicle screw.

In summary, based on the encouraging early clinical outcomes, I am looking forward to further follow-up results from the authors.

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Acknowledgments

Drs. Liangbi Xiang and Jun Liu contributed to this letter.

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Disclosures

The author reports no conflict of interest.

Response

We wish to thank Dr. Wang for his interest in our clinical research article.

First, Dr. Wang suggested that the posterior-only approach using a risky and unpopular cervical pedicle screw fixation hardly replaces the anterior-posterior or posterior-anterior approach. We agree with his opinion, although

this approach has the advantage of avoiding a sometimes unnecessary anterior approach. We also believe that lateral mass screw conversion is only one of the safety processes and not an unsatisfactory result.⁴

Second, a longer follow-up period seems to be necessary to show the long-term durability.

Third, as to conservative measures, we discussed this issue in the article.

Fourth, we believe that the biomechanical superiority of a longer screw could obviate a need for an anterior plate despite additional anterior surgery with only a cage, which would be necessary in single-level cervical surgery.^{1–4}

Once again, we appreciate Dr. Wang's interest in our article.

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Autograft-derived spinal cord mass

TO THE EDITOR: In regard to the case report by Dlouhy et al.,¹ we are greatly saddened to learn of this adverse event (Dlouhy BJ, Awe O, Rao RC, et al: Autograft-derived spinal cord mass following olfactory mucosal cell transplantation in a spinal cord injury patient: case report. **J Neurosurg Spine** 21:618–622, October 2014). However, as far as we know, this is the only treatment (scar removal, autograft, and rehabilitation) for chronic complete spinal cord injury that has resulted in significant functional improvement. It should be noted that the incidence of this particular complication appears to be very low (< 1%) among the more than 140 patients who have undergone

this treatment in Portugal. Additional patients have received this treatment in Japan where the procedure is appropriately reimbursed.

Although this adverse event is distressing, in our 2010 study we were heartened by the finding of numerous axons in the autograft tissue, which in combination with rigorous physical therapy and scar removal may explain the remarkable recovery observed in many patients.⁴ Patient 13 (C-6 complete American Spinal Injury Association [ASIA] Impairment Scale [AIS] Grade A tetraplegia) had the olfactory mucosa autograft (OMA) procedure 24 months after injury. Before and after the procedure, the patient received rehabilitative brain-initiated overground nonrobotic/non-weight supported training (BIONT) at Centro Giusti in Florence, Italy. Before the OMA procedure, he was unable to operate a manual wheelchair. Now he can walk a short distance using a walker with only low-boot orthotics. His condition has improved to the AIS Grade C level (Video 1).

VIDEO 1. Case 13 (C-6 complete AIS A tetraplegic) from the 2010 publication⁴ after he had the olfactory mucosa autograft procedure and he received BIONT (brain-initiated overground nonrobotic/non-weight supported training) rehabilitation at Centro Giusti in Florence, Italy. Copyright Carlo Alberto Arcangeli. Published with permission. Click here to view.

Several other individuals receiving BIONT have demonstrated similar improvement.⁴ More than 80% of the patients receiving OMA and long-term BIONT have shown considerable improvement. However, because this type of therapy frequently lacks long-term insurance coverage and thus few centers adopt this method of treatment, only about 30% of the individuals who could benefit from the therapy demonstrate improvement.

We agree with the authors of this case study that all stem cell treatments may have the risk of adverse ectopic cellular overgrowth. We must also note that the state of the art has certainly advanced since our initial trial began more than 10 years ago. This study was discontinued following the death of Dr. Lima, but efforts are being made to modify the protocol so that this unexpected adverse event can be avoided in any future investigations. Furthermore, efforts are also underway in the United States and Portugal to obtain approval for the use of a purified population of stem cells from olfactory mucosa that would hopefully avoid this particular problem.

This benign growth may have resulted from an atypical distribution of respiratory epithelium. Before initiating our preliminary trial, the distribution of olfactory mucosa in the nasal cavity was studied at necropsy in numerous individuals so that a site that contained only olfactory epithelium could be selected.^{2,3} Although sections of the tissue harvested for autograft were examined histologically, random selection may not have been sufficient to detect contamination with respiratory epithelium. This may explain the aberrant tissue findings in the authors' case report.

We will make every effort to contact all of the OMA patients to advise them of this potential problem. Unfortunately, the surgical team for the index procedure was not notified of this adverse event when it was identified more than 6 months ago even though one of the authors is located in a neighboring university. We only became

aware of this problem when a reporter from the United Kingdom notified us less than a week before the online publication of this case study. It is hoped that at the time of the subsequent surgery, the patient was advised to contact the investigators in the original study so that the medical records could be examined to determine if there were any unusual circumstances that may have contributed to this adverse event.

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Disclosures

The authors report no conflict of interest.

Supplemental Information

Videos

Video 1. <https://vimeo.com/140829111>.

Response

We appreciate the reply by Dr. Escada and colleagues. We acknowledge that stem cell therapy and cell transplantation hold immense medical promise and will very likely lead to newer and better treatments for a variety of neurological disorders. However, its use in humans is experimental and invasive and may result in substantial side effects, complications, and unknown long-term consequences.¹ Therefore, patients require continual lifelong follow-up. This is especially true with spinal cord cell transplantation. Without a complete assessment of all patients who have undergone olfactory mucosal cell transplantation for spinal cord injury (SCI), the incidence of complications, including ectopic cellular growth, remains unknown. We understand the difficulty of continuing clinical studies and managing patients in cases in which the principal investigator of the experimental treatment is no longer alive. Therefore, we are pleased to read that the authors will be assessing all patients who have undergone this treatment and will continue to monitor these patients in the future.

Going forward, we encourage an assessment of the functional status of all patients who have undergone olfactory mucosal cell transplantation for SCI. In addition, MRI of the spine should be conducted to assess for both symptomatic and asymptomatic ectopic cellular growth, as seen in the patient in our report. We look forward to seeing these data in a peer-reviewed publication.

As we stated in our report, nerve twigs—not axons—were identified in the autograft mass at the SCI site. We stated that it was unclear what these nerve twigs represented. They were small nerve fibers and not the regeneration of lesioned axons in the spinal cord. Magnetic resonance imaging of the spine with diffusion tensor imaging (DTI) would help to examine the recovery of spinal cord white matter tracts and support the authors' claims that olfactory mucosal cell transplantation repaired axonal growth and provided functional recovery. Additionally, Lima et al.⁵ stated that a small experimental animal trial using autologous whole olfactory mucosal transplants in a spinal transection model was conducted prior to their use in humans; however, this work is not published. The publication of these animal studies may help to elucidate the mechanisms by which olfactory mucosa facilitates spinal cord regeneration or function.

Although the authors state that patients who have undergone olfactory mucosal cell transplantation after SCI had significant functional improvement, the original published study⁴ included only 20 patients and was not randomized or controlled to include a comparison SCI group without olfactory mucosal transplantation that also underwent the same rigorous rehabilitation therapy (BIONT). A case control study, cohort study, or a randomized controlled clinical trial would have provided much greater evidence of the treatment's effectiveness.

Even positive results from randomized clinical trials of SCI treatments are typically viewed with caution. Double-blind, prospective randomized studies after SCI with GM1 ganglioside³ and methylprednisolone² remain controversial 20 years after their publication. We are currently enrolling SCI patients in studies using investigational medical treatments at the time of injury. Some of the patients who have received the investigational treatments have done well. However, we can hardly attribute the recovery of function to this treatment in a preliminary and randomized study of only 70 patients who have not been analyzed to compare outcomes between experimental and control groups. The same is true with the use of olfactory mucosal cell transplantation in 20 SCI patients without a comparison control group.⁴ Caution is advocated in promoting controversial and experimental treatments for SCI, when both patient and physician are desperately seeking a cure.

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A report of spinal subdural abscess provides incomplete and inaccurate information

TO THE EDITOR: The report by Kraeutler et al.⁵ describing spinal subdural abscess following epidural steroid injection provides incomplete information, leaving the reader with multiple questions (Kraeutler MJ, Bozzay JD, Walker MP, et al: Spinal subdural abscess following epidural steroid injection. *J Neurosurg Spine* 22:90–93, January 2015). The patient in this case underwent transforaminal steroid injection 24 days prior to his presentation; however, the only information available in this regard is that the procedure was performed under fluoroscopy by an orthopedic spine specialist without complication. No antibiotics were given, and the authors failed to describe sterile precautions, including the wearing of a mask and sterile gloves, or the type of steroid injected (that is, whether it was methylprednisolone belonging to the contaminated group). Neither did the authors describe the level of the procedure performed or the side. The data showed that the patient had an L5–S1 disc bulge with persistent left neural foraminal narrowing and possible left S-1 nerve root impingement, which was consistent with prior findings. However, clinical examination revealed that strength in his right lower extremity had decreased with intact sensation, whereas it was only mildly decreased in his left lower extremity. Thus, questions arise regarding not only the sterile precautions, but also the side of the procedure, the lack of correlation between MRI findings, and the type of steroid utilized.

In addition, it appears that the MRI study that missed the diagnosis of spinal subdural abscess was performed without administering contrast agent. The authors' *Discussion* includes the Coumans and Walcott² report of an

epidural abscess following an epidural injection at T-7 that had been administered 11 weeks earlier. In this regard, Kraeutler and colleagues missed that this report specifically stresses the rapid progression of lumbar subdural empyema following an acromial bursal injection, which had been administered 2 days before the patient's presentation. Further, the patient in that report² also underwent a facet injection at T6–7. The case appears to be an instance of steroid and interventional overuse and again lacks information on sterile precautions. This practice does not correlate with Interventional Pain Management guidelines.⁶ In addition, multiple cases of spinal subdural abscess, though rare, have secondary causes related to hematogenous spread from remote infection, contiguous spread from decubitus ulcers, and epidural catheterizations, but are mainly attributable to intravenous drug use.^{4,6}

Tan et al.¹⁷ have responded to this case report with comments against epidural injections and in favor of surgery. However, they,¹⁷ like Kraeutler et al.,⁵ have relied heavily on a single flawed randomized trial with inappropriate references^{3,8} and have omitted a multitude of studies performed with what we believe is a more relevant design.^{9–11,13}

Transforaminal epidural injections are associated with significant complications, including paraplegia, especially when they are performed utilizing the safe triangle approach with particulate steroids.^{1,15,16} These complications can be avoided by using an infraneural approach. Tan et al.¹⁷ described only epidural injections performed in central spinal stenosis.³ Further, the case report by Kraeutler et al. described the treatment performed for disc herniation rather than spinal stenosis. Consequently, it may be more appropriate to consider literature related to the effectiveness of epidural injections in disc herniation rather than spinal stenosis only.^{7,12,14} of alternative approaches for transforaminal epidural injections,^{1,15,16} of other routes of administration including caudal and lumbar interlaminar routes,^{13,14} and finally of local anesthetic alone without steroids.^{12–14}

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Disclosures

Dr. Hirsch is a consultant for Medtronic and has taught courses for Carefusion. Dr. Manchikanti is a consultant for Semnur Pharmaceuticals Inc.

Response

We thank Dr. Manchikanti et al. for their response to our case report on a spinal subdural abscess occurring after an epidural steroid injection. We would like to re-

spond by answering some of their questions resulting from our report. Prior to the patient's initial presentation, he underwent a left S-1 transforaminal epidural steroid injection through the first dorsal sacral foramen. Sterile precautions were observed including wearing a mask and sterile gloves and cleaning the skin with Chloraprep (2% chlorhexidine gluconate and 70% isopropyl alcohol). Kenalog (triamcinolone acetonide), which does not belong to the methylprednisolone acetate group associated with fungal infections, was the steroid used for this injection.² However, there have been 2 remote cases of a spinal epidural abscess occurring after epidural steroid injection of triamcinolone.^{1,3} Both of the patients in these case reports had a history of diabetes mellitus, which was notably absent in our patient.

We appreciate the suggestion by Dr. Manchikanti and colleagues to use a Kambin triangle approach. This technique has shown positive outcomes with no significant complications for patients with lumbar spinal stenosis⁴ and lumbar radicular pain,⁵ although it is still unclear whether this approach offers benefits specifically to patients with disc herniation. Future studies should focus on comparing outcomes and complication rates between supranatural and Kambin triangle approaches for patients with disc herniations.

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