Neurosurgical Forum

LETTERS TO THE EDITOR

Bone cylinder plug and coil technique for accurate pedicle localization in thoracic spine surgery

TO THE EDITOR: We read with interest the article by Castle-Kirsbaum et al.¹ (Castle-Kirsbaum M, Maingard J, Goldschlager T, et al. Preoperative coil localization for spinal surgery: technical note. *J Neurosurg Spine*. 2020;32(3):483–487). The authors present their technique for preoperative localization of the index pedicle for subsequent thoracic spine procedures. This technique relies solely on the identification of a pushable microcoil, which has been inserted into the periosteum of the index pedicle using a prior CT-guided, percutaneous procedure.

We invite the authors to review our article describing a new technique for preoperative pedicle identification that we named the “bone cylinder plug and coil technique.”² In our description,² the procedure is accomplished on an outpatient basis, with the patient prone and under local anesthesia. The index level is identified using biplanar fluoroscopy. The incision site is indicated using a sterile skin marker, and the skin is punctured over the correct level. The local paraspinal muscles and subperiosteal space are then infiltrated with 10 ml of 1% lidocaine. An 11-gauge, 125-mm-long trocar is then inserted into the posterior pedicle. A 13-gauge trepan/trephine is introduced at a 5-mm depth into the middle portion of the pedicle, allowing for the removal of a cylindrical fragment including both cortical and cancellous bone. Two embolization microcoils measuring 2–4 mm are then inserted into the bone defect and secured in place with the bone cylindrical fragment. Accurate positioning of the metallic coils is verified on a CT scan of the thoracolumbar spine, including the sacrum, allowing us to count each vertebra.

The main difference between our technique and the one described by Castle-Kirsbaum et al. is that our procedure involves removal of a cylindrical bone fragment from the pedicle and its replacement after coil insertion into the pedicle. After their insertion, the coils are locked in place, which minimizes the risk of coil migration and thus wrong-level surgery. It differs from the techniques described by Madaelil et al.³ and Binning and Schmidt,⁴ in which the authors do not secure the intraosseous markers inside the pedicles, thus increasing the risk of marker migration. Macki et al. also secured a gold fiducial, but as

they only utilized bone wax, their procedure is not secure and thus is unreliable.⁵

We believe that our original technical description of pedicle localization in thoracic spine surgery deserved to be acknowledged by the authors.

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Disclosures

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Response

We thank Maduri and colleagues for their interest in our article and for bringing our attention to their recently published technique.¹ They describe a method of preoperative localization using fluoroscopically guided trephination of a thoracic vertebra. They percutaneously remove from the thoracic pedicle a cylinder of cortical and

trabecular bone 2 mm in diameter and 5 mm in depth and subsequently deploy two microcoils into the bony defect, which is resealed with the bone fragment. The location of the coils is confirmed with postprocedural fluoroscopy and CT imaging.

We believe our technique has several important advantages worthy of discussion. First, our technique utilizes a preprocedure scout CT, with the microcoils placed under CT guidance. With this method, consistent anatomical landmarks can be used to localize the level of interest prior to puncturing the skin. Maduri and colleagues trephine the pedicle under fluoroscopic guidance alone, which renders their technique fallible to the same errors of traditional intraoperative localization described in our article.

Secondly, our technique involves placement of a microcoil under the periosteum of the vertebrae, thus rendering it fixed to the level of interest. Maduri and colleagues perform a more invasive technique, removing cortical and trabecular bone from a fluoroscopically identified level. This potentially increases the risk of inadvertent damage to adjacent structures, especially in individuals with poor bone quality. It may produce a localized hematoma from the highly vascular trabecular bone as well as induce a more pronounced local inflammatory reaction, which can interfere with the upcoming surgery.

Finally, Maduri and colleagues utilize a postprocedure CT to verify the correct level of coil placement. Although this mitigates the risk of wrong-level surgery, it means that incorrect fluoroscopic placement of the coils is only realized after percutaneous trephination of the pedicle and the risks that follow. Our technique of preprocedure CT and CT-guided microcoil placement using fixed landmarks eliminates this possibility.

We again thank Maduri and colleagues for making us aware of their technique and facilitating further discussion on the important topic of methods for reducing wrong-site surgery. Maduri and colleagues' paper was not referenced in our original article as it was published after the completion of our literature review.

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Klippel-Feil syndrome

TO THE EDITOR: I read the article by Patel et al.¹ with interest (Patel K, Evans H, Sommaruga S, et al. Characteristics and management of pain in patients with Klippel-Feil syndrome: analysis of a global patient-reported registry. *J Neurosurg Spine*. 2020;32(4):578–583). I wish to make some comments, because the article pertains to a subject of interest to my colleagues and me.

The authors have essentially evaluated the symptom of pain in patients with Klippel-Feil syndrome (KFS) and have identified that this symptom is quite common and that the outcome following any kind of surgical intervention is worse than in patients in whom no surgery was done. Moreover, following surgery the patients had “significantly” more comorbidities and neurological symptoms when compared to patients who were not surgically treated. Without contesting any of the statements made by the authors, we wish to update the readers regarding our current views on the subject, which are based on more than 3 decades of experience and several articles published in the PubMed/Medline database.

We have identified that short neck, torticollis, low posterior hairline, bone fusions, platybasia, bifid arches of atlas, and a host of other musculoskeletal alterations that are grouped under the umbrella term of basilar invagination are not primary issues related to embryological disorders or even to any kind of genetic mutation, but are secondary manifestations of chronic or longstanding atlantoaxial dislocation.^{1–8} We have related the symptom of pain and neurological symptoms to the presence of an unstable spine. More importantly, we have observed that all the symptoms and musculoskeletal and neural alterations have the potential for reversal following atlantoaxial fixation.⁷

We have observed that in the event of chronic atlantoaxial instability all the secondary musculoskeletal and neural alterations are “protective” in their function.^{5–7} There is vertical reduction and transverse increase in both the cranial and spinal dimension.³ Vertical reduction in cervical spine length results in short neck and torticollis, vertical reduction in cervicodorsal spine results in kyphoscoliosis, and vertical reduction in cranial height results in “short head.”

Atlantoaxial fixation in the presence of short head and cervical vertebral fusions is a relatively complex surgical procedure. Unless perfectly and successfully executed, there is a potential for worsening of all symptoms.⁸ Our longstanding experience with the subject has provided us an advantageous position and the confidence to state that surgery in such cases is mandatory and has the real potential of giving a new life to these unfortunate patients.

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Response

We thank Dr. Goel for his interest in our article. We would like to clarify one of the findings in our study regarding the experience of pain in patients with KFS who received surgical treatment. Contrary to what was mentioned in the letter to the editor by Dr. Goel, we did not conclude that patients who received surgical intervention had inferior outcomes compared to patients without surgical treatment.

Our analysis was based on a survey that was completed at a single time point. As a result, for patients who received surgical intervention, we did not have access to survey data before and after the procedure, and therefore cannot comment on any changes in levels of pain following surgery. Rather, we showed a significant correlation between receipt of surgical intervention and the number of comorbidities and neurological symptoms, probably because patients with more severe presentations pursued surgical correction. Our study aimed to characterize the types of patients who pursue surgery rather than medical management; however, we cannot comment on changes in disease severity before and after surgery.

Our analysis of the relative effectiveness of surgical versus nonsurgical methods in treating KFS-related pain is limited by small sample size. However, as indicated in the study, we found no significant difference between the level of pain relief provided by prescribed oral medications versus surgery ($p = 0.087$). Further research with larger patient cohorts is required to substantiate these findings.

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Education and evidence-based medicine in neurosurgery

TO THE EDITOR: As representatives of the leadership in organized neurosurgery, we are writing to commend the *Journal of Neurosurgery: Spine* for the invited publication by Hadley and Walters¹ in the October 2019 issue (Hadley MN, Walters BC. The case for the future role of evidence-based medicine in the management of cervical spine injuries, with or without fractures. JNSPG 75th Anniversary Invited Review Article. *J Neurosurg Spine*. 2019;31(4):457–463). You chose two of our current neurosurgical thought leaders who are at the forefront of education and evidence-based medicine in neurosurgery, particularly in its applied form through the development of clinical practice guideline recommendations. As noted, these authors and their colleagues were recognized by the United States Congress for their work in this field.² In their publication, the authors have identified several important issues currently within our specialty that we would like to comment upon.

There is the recognition of the value and efficacy of the systematic approach to the development of assessment, diagnostic, and treatment recommendations, as opposed to the more historical mechanism of well-known practitioners declaring the best way to approach a problem (not necessarily based on scientific evidence). The systematic, evidence-based approach improves patient care through the scientific evaluation of new, suggested management pathways, and it identifies faulty practice patterns undertaken without careful examination of proffered evidence in the literature. In addition, guideline development helps to identify areas within patient care that require further study for the generation of more solid evidence to define the most effective treatment approaches. By investing the energy and effort to create the best-possible recommendations, these authors and their colleagues provide exemplary mechanisms for improving patient care within our specialty.

In addition to detailing the value of evidence-based guideline development, the authors provide examples of where we have gone wrong in our treatment approaches by not clearly understanding the successes and failures in the science that underlie certain approaches to patient care. An example is the use of methylprednisolone to treat patients with spinal cord injury, which is a treatment para-

digm followed for decades without an appreciation of the flaws in the trials supporting its use.³ This is surprisingly still under discussion, even though there have now been several studies and guidelines refuting its use in this patient scenario.

Perhaps even more importantly, these authors bring to readers' attention the publication of other guidelines that have failed to achieve support or the imprimatur of our leading organizations (the Congress of Neurological Surgeons and American Association of Neurological Surgeons) and falsely claim to be carried out "under the auspices" of these organizations.⁴ This claim was reiterated, unfortunately, in one of our own publications,⁵ but it was refuted and clarified by these authors, to the benefit of all neurosurgeons.

Finally, we wish to thank Drs. Hadley and Walters for their steadfast support for scientific approaches to neurosurgical care and for helping their colleagues achieve a greater understanding of the best available evidence.

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Response

We are very grateful for and humbled by the letter to the editor from our esteemed colleagues who represent the current, immediate past, and future leadership of our two national neurosurgical member service societies, the American Association of Neurological Surgeons (AANS) and the Congress of Neurological Surgeons (CNS). Their response demonstrates their recognition of the importance of evidence-based medicine (EBM) to our specialty, particularly in the management—including assessment, diagnosis, and treatment—of patients with neurosurgical disease. The rigid application of evidence-based medicine in the development of a comparative study design to obtain at least class II medical evidence is essential. This stance was embraced early on by both of our large national organizations.¹

In researching our article, we found it ironic and astonishing that several AANS/CNS Trauma Section leaders have suggested in a recent edition of the *Neurotrauma & Critical Care News* that evidence-based medicine may be over-rated, perhaps too rigid and onerous, preferring a return to "practice-based medicine." In other words, they would prefer to take a stance of "this is what we do and it seems to make sense" rather than to rigorously study what is being done scientifically to determine whether it makes a difference (positive or negative) based on the principled science of evidence-based medicine.^{2,3} That these stances are in direct contradistinction to those espoused and supported by our basic science colleagues (among whom these authors surprisingly number) is unfathomable. This is the opposite of everything we have been taught regarding translational research by these esteemed colleagues, and it is in direct contrast to the standards espoused by our national organizations and outlined by our leaders in their letter to the editor as "exemplary mechanisms for improving patient care within our specialty."

For example, the AANS/CNS Trauma Section authors specifically reference early surgery for decompression of the spinal cord after a patient has sustained traumatic cervical or thoracic spinal trauma associated with spinal cord injury.³ Early is better for intracranial epidural hematomas, so why shouldn't the same principle apply to acute spinal cord compression? In fact, it might, and while it makes a lot of sense to decompress the compromised spinal cord early, it unfortunately hasn't been proven yet with any degree of scientific certainty (level I or II comparative medical evidence).⁴

In addition, they appear to favor the abandonment of the scientific rigor of evidence-based medicine in the study of traumatic neurosurgical injuries and suggest a

more casual, less scientific, practice-based approach to traumatic brain and cervical spine and spinal cord injuries, harking back decades to reliance on level III prognosis-with-treatment case series.² We may personally favor (and have practiced) early, rapid realignment and reduction of cervical and thoracic fracture dislocation injuries by either closed or open surgical means—spinal cord injury present or not—in the hope of preventing potential neurological injury if the reduction and/or decompression is not early enough, and have both exemplified this paranoid bias all of our respective careers. However, we are aware that because we “believe it to be so” doesn’t “make it so” scientifically. For this scenario—and for others—we concur with our organizational leaders who continue to embrace the pursuit of the “best available evidence” derived by the application of evidence-based medicine to the care of our neurosurgical patients.

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