Neurosurgical Forum LETTERS TO THE EDITOR

Artificial atlantoaxial joint

TO THE EDITOR: We read the article by Shen et al.² and the accompanying editorial by Urakov and Wang³ (Shen K, Deng Z, Yang J, et al: Novel posterior artificial atlanto-odontoid joint for atlantoaxial instability: a biomechanical study. *J Neurosurg Spine* 28:459–466, May 2018; Urakov TM, Wang MY: Editorial. Dynamic atlantoaxial instrumentation to preserve cervical mobility. *J Neurosurg Spine* 28:457–458, May 2018). We wish to make some comments as the subject is of direct clinical interest to us.

The authors are correct in stating that the atlantoaxial joint is a highly mobile joint, and while the preservation of stability is paramount, attempts must be made toward retaining the movements of the region.² Thus, any implant or device that provides both stability and mobility to the region is most appropriate.

The craniovertebral junction is a structural masterpiece of nature designed to be the most stable and the most mobile region of the body and architected to provide an uncompromising and safe passage to the most critical neural and vascular structures. The atlantoaxial joint is the most mobile joint of the body and is active in saying both “yes” and “no” by virtue of its circumferential movements and moves tirelessly and flawlessly throughout its life. Our long-term experience in the field indicates that complexities of the region are so huge, the natural design so intricate and flawless, that any human effort to mimic the atlantoaxial joint can only be an unrealistic dream.

Shen et al. have conducted a biomechanical analysis on cadavers and have identified instrumentation that provides both stability and mobility to the atlantoaxial joint.² The usefulness of such a device will have to be identified on the basis of subsequent studies. The more critical issues in the conduct of the surgery and its success are that the procedure should be technically simple and the implant needs to be physically sturdy and long-lasting. The movements permitted by the device should be optimum and not more than the normal range. The devastating effects of failed treatment should also be realized.

As the authors have mentioned, efforts to restore stability and retain mobility are rarely discussed. In this context, I invite the authors to view our description of an artificial atlantoaxial joint, which I believe to be the

first described in the literature.¹ We designed an artificial joint in the form of a ball-and-socket construct (Fig. 1).¹ We believe that our design of the artificial joint is rather simple and possibly effective. Biomechanical analysis of the feasibility of the proposed artificial atlantoaxial joint is in progress. However, our current impression is that our construct and that described by the authors are only experimental and far from being ready for any clinical use. It is premature to state that the artificial atlantoaxial joint can ever be as effective in its function as an artificial knee or hip joint. However, it is also true that such efforts can only serve as motivation for further innovations and developments.

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FIG. 1. A: The ball-and-socket articulation is visible. The serrated surface of the implant will be in proximity to the surface of the bone of the facet. B: The fixed implant on the undersurface of the facet of the atlas. The ball on the implant will articulate with the socket in the implant on the axis bone. C: The implant fixed to the facet of the axis bone. The socket is meant for the ball-and-socket articulation. D: The fixed implant and the articulation. Reprinted from Goel A: *J Craniovertebr Junction Spine* 6:147–148, 2015. CC BY-NC-SA 4.0. (<https://creativecommons.org/licenses/by-nc-sa/4.0/>). Figure is available in color online only.

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Disclosures

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Response

We sincerely thank Dr. Goel for his keen interest and profound insights into artificial atlantoaxial joints, as well as his advice.

Our novel posterior artificial atlanto-odontoid joint (NPAAJ) is designed to stabilize the C1–2 complex and achieve motion in different planes, and the surgery used to implant our device is based on the technology used for pedicle screw implantation. Because the NPAAJ requires normal lateral C1–2 joints to achieve motor function, we believe that it is unsuitable for lateral C1–2 joint disorders. Therefore, the NPAAJ may be useful for treating atlantoaxial instability (AAI) disorders caused by congenital odontoid dysplasia, odontoid fracture nonunion, and C-1 transverse ligament disruption (IA, IB, and IIB). The artificial atlantoaxial joint Dr. Goel designed is very unique and may be used in patients with AAI not only due to ligament injuries and fractures but also caused by lateral C1–2 joint diseases such as rheumatoid arthritis and tumors.

The atlantoaxial complex is a very complex structure, and the relative trajectories of C1–2 are also complex and irregular, especially the trajectory of the lateral joint. Currently, there are few studies on the trajectory of the lateral C1–2 joints. Therefore, we intend to further study the trajectory of C1–2 to improve the design of the NPAAJ. Dr. Goel's artificial joint is in the form of a ball-and-socket construct, which is a very good idea. However, we think that the concave design of the joint surface is not suitable for a sphere. We suggest that appropriate improvements would be based on the trajectory of the lateral C1–2 joints to achieve better C1–2 lateral bending and axial rotation. In addition, we propose that osteotomy of the upper and lower articular surfaces of the lateral C1–2 joints should be performed to facilitate the placement of his prosthesis and reduce the tension of peripheral blood vessels, nerves, and ligaments.

The design of his artificial joint is very good, and we believe that he will obtain very good results through his continuous improvements and research. Our study is a preliminary study of cadavers, and our device has not yet been implanted in patients. In future studies, including live animal experiments, we intend to increase the range

of motion of C1–2 with respect to flexion, extension, and rotation and improve the articular surfaces of the NPAAJ.

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Response

We appreciate Dr. Goel's comments on the issue of atlantoaxial joint stabilization with mobility preservation. As he elegantly described, the C1–2 joint is indeed one of nature's design masterpieces. The joint moves in axial rotation, flexion, extension, and lateral bending while providing safe passage of crucial neural and vascular structures. Mechanical studies have shown that the joint is a major actuator during flexion and axial rotation, making it a higher priority for preservation.³

Several groups have tried to produce an artificial joint that would preserve the mobility of the atlantoaxial joint in stabilizing surgeries. Previous artificial joints required an anterior surgical approach for implantation.^{2,4} Generally, transoral approaches carry an elevated risk of highly undesirable complications such as infections, cerebrospinal fluid fistulas, vertebral artery injuries, quadriplegia, and death.¹ The more recent designs of artificial C1–2 joints, such as those described by Shen et al. and Dr. Goel, allow for a posterior approach and therefore may have more favorable risk profiles. Nonetheless, we could not agree more with Dr. Goel that all of these devices have not been adequately tested in clinical application.

Moving forward, any future initiatives to engineer an artificial atlantoaxial joint should consider the following caveats: 1) Its biomechanical properties should include not only the preservation of stability and mobility in the desired directions, but also the minimization of forces at the hardware-bone interface to prevent loosening with cyclic motion. 2) Surgical techniques for implantation should carry a reasonable risk profile to the patient and technical skill requirement amenable to most surgeons. 3) For better or worse, even the most perfect design needs to be evaluated for cost-effectiveness. The appropriate patient population that could benefit from such a mobility-preserving surgery has yet to be defined, but given the importance of this spinal segment, such an implant could significantly improve surgery in the complex atlantoaxial region.

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Considerations about the use of negative-pressure dressing therapy after spine surgery

TO THE EDITOR: I read with great interest the article by Foxx et al.² about the negative-pressure dressing management of recurrent postoperative epidural hematoma and uncontrollable intraoperative hemorrhage in posterior spine surgery (Foxx KC, Mesfin A, Molinari R: Negative-pressure dressing management of recurrent postoperative epidural hematoma and uncontrollable intraoperative hemorrhage in posterior spine surgery: report of 4 cases. *J Neurosurg Spine* 28:220–225, February 2018).

In the article, an open-wound negative-pressure dressing therapy (NPDT) with delayed wound closure is proposed. It is considered a viable alternative in certain patients affected by recurrent symptomatic postoperative epidural hematoma and uncontrollable intraoperative hemorrhage.

In my experience, I have used this technique and disagree with its use. NPDT promotes wound healing by applying a vacuum through a special sealed dressing. The vacuum may be applied continuously or intermittently, depending on the type of wound being treated and the clinical objectives. Typically the dressing is changed 2–3 times per week. The dressing used for the technique includes open-cell foam dressings and gauze, which are sealed with an occlusive dressing intended to contain the vacuum at the wound site.^{1,3–5}

In my clinical practice this technique was applied in patients affected by a spinal infected wound with the aim to promote its healing. I found that the transparent film that is applied as a skin barrier to protect the periwound was not able to remain perfectly attached to the skin after a few days. It required its immediate substitution in order to not contaminate the wound. Perhaps its detachment caused a loss of vacuum and a wound exposure to the surrounding air with its implications for infection.

I was not keen on its application in cases of spinal cord or dural sac exposure; I did not want to apply any negative pressure on these structures because of the implications for the nervous system, and I also wanted to avoid any CSF leak complication. Finally, I do not like to have an “open wound” at the patient’s bed, and also the patient

will require a further operation with a higher exposure to infection.

I appreciate the authors’ effort in proposing a viable alternative in this challenging category of patients, but I think that nowadays many hemostatic agents are available to obtain a valid hemostasis and that this proposed technique presents too many risks in terms of infection rates, requires major bedside patient care, and is a cause of great patient discomfort.

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Response

We thank Dr. Lorenzo Nigro for his letter and appreciate this opportunity to respond. Our original paper reported 3 cases of recurrent symptomatic postoperative epidural hematoma successfully managed with novel open-wound NPDT and 1 case of uncontrollable intraoperative hemorrhage that was managed primarily with the same technique. The details of that technique and its application in our patient series are further described in our article.

In our series, we used NPDT to allow sufficient evacuation of hematologic products, while also providing a tamponade effect, to prevent neurological injury. We were not using NPDT to treat wound infection. The 3 patients with symptomatic postoperative epidural hematoma had significant recovery of neurological function during and after NPDT, and the patient managed primarily with NPDT for uncontrollable intraoperative hemorrhage had no further neurological deterioration. After a period of 3–9 days, our

patients were taken back to the operating room for removal of the negative-pressure dressing (NPD), washout, and wound closure.

We do not dispute that adequate hemostasis can be obtained using cautery and hemostatic agents in the vast majority of patients, and we certainly applied the NPD technique in an extremely small percentage of our cases. However, operating on patients with significant risk factors for postoperative epidural hematoma can sometimes be unavoidable. Of the 3 patients with recurrent postoperative epidural hematoma, the first had coagulopathy secondary to dual antiplatelet therapy, in the second it was due to cirrhosis, and the third was receiving therapeutic anticoagulation. All 3 had already undergone at least 1 hematoma evacuation and closure over suction drains prior to NPD placement. We used NPDT primarily in 1 patient who had uncontrollable intraoperative hemorrhage secondary to pharmacologically elevated blood pressures that were needed for cerebral perfusion in the setting of an ongoing stroke.

The risk of wound infection after NPDT is a valid concern. All patients in our series were treated with skin flora antibiotic prophylaxis while the NPD was in place.

Although our series was small, none of our patients developed a wound infection after definitive closure. Patient comfort during NPDT was also a concern we shared when using this technique. We were able to obtain adequate pain control by using narcotic and muscle relaxant medications in our patients.

We successfully used NPDT in the cervical and lumbar spine by using the technique described in our original article. We would not recommend using this technique in patients with an intraoperative CSF leak. CSF leakage in the setting of an open wound with pressurized drainage could lead to meningitis or complications associated with intracranial hypotension, as noted in our original article.

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