Letters to the Editor

NEUROSURGICAL FORUM

Nonpenetrating titanium clips for dural closure during spinal surgery

TO THE EDITOR: We enjoyed the recent article by Ito et al.[1] (Ito K, Aoyama T, Horiuchi T, et al: Utility of nonpenetrating titanium clips for dural closure during spinal surgery to prevent postoperative cerebrospinal fluid leakage. *J Neurosurg Spine 23:*812–819, December 2015). The authors report the results of bench studies and clinical data in 31 patients with the use of nonpenetrating titanium clips to close the dura during spinal surgery. They found that clips have superior leakage pressure when compared to conventional suture material. The leakage incidence was acceptably low when examined using postoperative MRI at 2 weeks (3.2%). The authors conclude that the clips are useful in spinal surgery as they can close the dura without creating any holes, do not create significant MRI artifact, and have a superior leakage pressure when compared with conventional suture material. We agree with all of their conclusions.

However, we were disappointed to see the authors state that "no published experimental or clinical studies have described the usefulness of nonpenetrating titanium clips for the prevention of postoperative CSF leakage." We published our experience with the same clips in 2010.[2] We reviewed our experience in 26 children undergoing 27 operations over a 20-month period. We found similar results: no CSF leakage and no significant artifact on CT or MRI. One patient required reoperation 13 months after the initial surgery; the prior use of clips did not make the subsequent exposure more complicated. Since 2007 we have used these clips routinely in our practice and have continued to have success in approximately 150 cases. We have made the following observations since our original publication: 1) There are different sizes available. We have found that the 2-mm "large" AnastoClip VCS (Le Maitre Vascular Inc.) works best. 2) Thicker dura like that found in the posterior fossa can be difficult to close satisfactorily with the clips. 3) Unlike suture, the clips cannot be used to bring dura together under any significant tension. A completely tension-free closure is required. 4) When applying the clips one can hold the trigger down to continually grab the 2 edges of dura through the deployed clip while using the other hand to move down the line to reapproximate the dural edges at the site of the next clip. By alternately grabbing the dural edges with the clip applier and dural forceps, one can rapidly close the dura. It typically takes less than 30 seconds to close a 2- to 3-cm long durotomy in this manner. 5) The clips are ideal for deep, narrow exposures such as that seen with muscle-sparing split laminotomies. One only requires an approximately 7- to 8-mm width of bony exposure to adequately apply the clips, less than that required for suturing with a TF or C-1 needle (13 mm) typically used with 4-0 or 5-0 suture.

We expect the omission of our previous work in this area was a mere oversight. We would like to congratulate Dr. Ito and colleagues on their thoughtful, more thorough investigation on the utility of these clips.

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Disclosures

The authors report no conflict of interest.

Response

Thank you for alerting us to the availability of your publication. Please accept our sincere apologies for not including your valuable publication in our paper.

We have now read your paper and understand the important contribution it makes regarding the usefulness of the nonpenetrating titanium clip. In addition, we have learned many important techniques that will prevent postoperative CSF leakage based on your experience with the cases in which you have applied the nonpenetrating titanium clip. We are extremely grateful to you for this.

Our article describes a fundamental study that investigated the leakage pressure and leakage pattern associated with the nonpenetrating titanium clip, and we determined that it helps to prevent postoperative CSF leakage. Compared with the cranial dura mater, the spinal dura mater is very fragile and it tends to tear easily. Based on the findings from your own study and our study, the nonpenetrating titanium clip can be considered very useful for approximating the spinal dura.

We greatly appreciate your ongoing support of our endeavors.

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High-grade spinal cord glioma

TO THE EDITOR: We read with interest the article by Crowley et al.[1] (Crowley RW, Burke RM, Lopes MBS, et al: Long-term cure of high-grade spinal cord glioma in a pediatric patient who underwent cordectomy. *J Neurosurg Spine 23:*635–641, November 2015). We found the article well written and well illustrated, but short on 1 reference. We realize that not all publications on any topic need to be referenced. However, our case, described in Viljoen et al.,[2] represented, up until its publication last year, the longest documented survival. Our patient did survive after cordectomy for spinal cord glioblastoma for 12 years and did witness his daughter graduate from physical therapy school. We agree with Crowley and colleagues that cordectomy is indeed an operation that has its indications, and we excuse the authors for their oversight.

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Disclosures

The author reports no conflict of interest.

Response

We regret the omission of Dr. Hitchon's published paper in our recent paper. Their article is an excellent illustration of the potential utility of cordectomy for spinal cord glioma. It should have been referenced in our report. We take this opportunity to thank the authors for bringing this oversight to our attention.

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Steroid use in anterior cervical discectomy and fusion

TO THE EDITOR: I read with interest the article by Jeyamohan et al.[5] (Jeyamohan SB, Kenning TJ, Petronis KA, et al: Effect of steroid use in anterior cervical discectomy and fusion: a randomized controlled trial. *J Neurosurg Spine 23:*137–143, August 2015). The study was well conducted, and the conclusions were of interest to many. However, I am concerned about a protocol that required 3 fine-cut CT scans of the cervical spine at 6, 12, and 24 months after surgery for fusion assessment in a group of patients averaging 54 years of age. Computed tomography scanning has long been considered the gold standard in assessing the results of arthrodesis (see, for example, Siambanes and Mather, 1998[9]); however, recent data on the potential harm from CT-related ionizing radiation has added an element of caution to the routine use of CT imaging, especially in children (see Table 2 and Fig. 2 in Hikino and Yamamoto, 2015[4]).[2,6,7] The Committee to Assess Health Risks from Exposure to Low Levels of Ionizing Radiation promulgated a significant linear, no-threshold dose-response relationship between ionizing radiation dose and the development of cancer in humans, based in part on data from Japanese atomic bomb survivors.[3] It has been estimated that 29,000 future cancers might be attributed to CT scans performed in the United States in 2007.[2]

A cervical spine CT is estimated to expose a patient to 4 mSv of radiation.[10] According to one assessment, a routine neck CT in a 40-year-old patient would cause 1 radiation-induced cancer in 4430 female or 6058 male patients (see Table 4 in Smith-Bindman et al., 2009[10]). In those 60 years of age, the estimated risk was reduced to 1 case in 6700 female or 8030 male patients. Accordingly, 3 cervical CT scans would raise the risk for any cancer to 1477 females or 2019 males at 40 years of age. Some have questioned even the routine use of postoperative radiographs in post–anterior cervical fusion patients whose clinical course is unremarkable.[1] To spare patients unnecessary radiation exposure, CT or flexion-extension radiographs have been utilized only if clinical symptoms or radiographs were suggestive of pseudarthrosis.

As regards cervical spine CT in children, one report[8] described a risk of excess thyroid cancers ranging from 1 to 33 cases per 10,000 CT scans in females or 1 to 6 cases for 10,000 CT scans in males. In another recent report,[4] the thyroid cancer estimate was as high as 100 cancer cases in males or 700 cancer cases in females per

100,000 scans. At the high end of the estimate in the article by Schonfeld et al.,[8] 3 cervical CT scans might increase the incidence of thyroid cancer in younger patients to 100 cases per 10,000 scans. This would imply that 1 or more of the 112 patients in the study by Jeyamohan et al.[5] might suffer a thyroid cancer as a result of participating in that study. In the Hikino et al. report,[4] there could be as many as 2 new cases per 100 female patients undergoing 3 cervical CT studies. These authors asserted that "limiting neck CT scanning to a higher-risk group would increase the gap between benefit and harm, whereas performing CT routinely on low-risk cases approaches a point where its harm equals or exceeds its benefit." This statement probably applies to adult patients as well.

Radiation doses, visceral exposures, and potential lifetime cancer risks are much higher for abdominal CTs and therefore, presumably, for lumbar CTs.[10] These data suggest that performing CT in every spinal fusion patient should no longer be the gold standard to ascertain whether bony fusion has occurred. Such scanning should be reserved for those patients with clinical problems suggesting that pseudarthrosis may be present and requires attention. The continuing insistence of clinical studies and article peer reviewers that CT scans are needed to prove arthrodesis should be relaxed to spare patients unnecessary radiation exposure.

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Disclosures

The author reports no conflict of interest.

Response

We very much appreciate the concern with regard to obtaining 3 postoperative CT scans and the increased risk of radiation-induced cancers in the study population. The effects of gamma radiation have been studied in large populations (Hiroshima, Chernobyl), and the link to certain cancers has been well established. Though the chromosomal damage caused by beta radiation (CT scanners and radiographs) may be greater than that by gamma radiation, the method of administration to small regions of the body, with lower doses separated by greater periods of time, probably mitigates some of these risks.[1] In fact, no direct data link radiographs to cancer in the adult population, probably because of the impracticality of performing such a study. An indirect analysis by Smith-Bindman et al. revealed that performing CT scanning of the head and neck will induce 1 additional cancer in 4430 females in the 40-year-old age group;[3] however, this finding is based on multiple assumptions. The authors readily acknowledge some of the weaknesses of their study: the radiation dosages varied significantly; they were unaware of actual absorbed radiation; and they could have improperly estimated the lifespan of the study population, which would have a significant statistical effect in predicting cancer rates.

We acknowledge that we subjected our patients to risks when obtaining CT scans, but we do object to the statement that we may have caused 1 or 2 cancers by conducting the study. This prediction was based on a study by Schonfeld et al., who looked at cancer rates in the pediatric population.[2] It is well known that CT scanning in infants carries a greater risk of causing cancers later in life. With regard to thyroid cancer specifically, there is an inverse relationship between age and radiation-related cancer risk. We had no pediatric patients in our study. Therefore, the statement that we caused 1 or 2 cancers simply has no foundation.

Studies are performed to answer questions and to change our practice patterns for the good of our patients. Most medical studies have risks, and many times those risks are not completely understood at the time of study inception. Some risks may be quite significant, and for that reason, we have institutional review boards and an extensive consent process for the protection of patients. Would the study have been accepted for presentation and/or publication without the CT data? I do not know, but I doubt it since we were wondering if fusion rates were altered by dexamethasone administration. Certainly, we hope that the publication of our study will help many, with no harm to our study subjects.

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Reduction of early postoperative dysphagia via steroid use after anterior cervical surgery

TO THE EDITOR: An analysis of a randomized controlled trial was recently performed by Jeyamohan et al.,[1] who showed that the use of dexamethasone significantly reduces the manifestations of dysphagia and edema of the airway without affecting long-term fusion rates following anterior cervical discectomy and fusion (Jeyamohan SB, Kenning TJ, Petronis KA, et al: Effect of steroid use in anterior cervical discectomy and fusion: a randomized controlled trial. *J Neurosurg Spine 23:*137–143, August 2015). The methodological approach is consistent with the obtained result; however, it may generate confusing expectations by failing to consider determining variables in the mechanism of injury to soft tissues: the results of dysphagia, swallowing disorders, phonation disorders (recurrent laryngeal nerve), and the tracheal airway. That is, the impact of the automatic retractors used (Thompson-Farley static retractor) and the prolonged mechanical compression, as well as the level at which surgery is done, the surgical technique, and the type of instrumentation used, are determining factors in this outcome. This means that simply by reducing pressure on the soft tissues by allowing the tissue to relax during the periods in which no surgical work is being done (downtime), there is a significant reduction in the complications of soft tissues. Therefore, we believe that the role of the retractors and the way that they are used are crucial in the mechanical trauma to adjacent tissues. Our primary advice is to think about the principles of nontraumatic surgery or minimal invasion, in which it is possible to be efficient in the surgical objective without compromising the function of neighboring tissues. Such is the case in our own experience in which, without using automatic retractors that are highly traumatic and by prioritizing the microsurgical strategy, it has been possible to notably reduce the soft tissue complications seen in this kind of approach in most cases, without the use of steroids, without compromising the fusion process, or without osteoporosis risk and other collateral effects.

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Disclosures

The authors report no conflict of interest.

Response

We thank the authors for their comments and thoughts regarding the etiology of dysphagia and other issues after anterior cervical discectomy and fusion. We tend to agree that mechanical compression on surrounding structures during the procedure and esophageal irritation by prominent hardware can lead to many of the stated issues. However, the purpose of our study was to evaluate the role of dexamethasone in alleviating these conditions and determine whether it impacted fusion rates. We did our best to control for these other factors in our study. All cases were led and primarily performed by the lead surgeon (D.J.D.). In every case Thompson-Farley retractors were used without fail, no relaxation time or "downtime" was granted, and the same hardware system was used, including an anterior plate and screws. In our experience, we have found that automatic, fixed retractors allow peak efficiency in moving from level to level without the need to adjust or replace retractors, significantly decreasing downtime and difficulties with exposure and thereby minimizing surgical, anesthetic, and overall retraction times. Therefore, we encourage surgeons to use the system of their choice while maintaining efficiency during the procedure. Regardless of the system, we believe our study supports the role of steroids in controlling for these complications without significantly affecting long-term fusion rates, and we hope these findings can be useful in other surgeons' practices.

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Lumbar intraspinal synovial cysts

TO THE EDITOR: We enjoyed reading Sukkarieh et al.'s[1] technical description and case series of patients undergoing a contralateral minimally invasive approach for

resection of lumbar synovial cysts (Sukkarieh HG, Hitchon PW, Awe O, et al: Minimally invasive resection of lumbar intraspinal synovial cysts via a contralateral approach: review of 13 cases. *J Neurosurg Spine 23:*444–450, October 2015). The work is thorough and well written, but we disagree with the paper's ultimate conclusion.

While the precise etiology of lumbar synovial cysts remains unclear, it is generally accepted that their development is related to advanced degeneration of the facet joint and capsule and that definitive treatment for symptomatic cysts is resection with or without arthrodesis. From 2010 to 2015, our group performed a minimally invasive laminectomy and facet cyst resection without arthrodesis through an 18-mm tubular retractor on 17 patients (10 males, 7 females) utilizing an ipsilateral approach. While we agree that either an ipsilateral or contralateral minimally invasive approach yields excellent visualization of the cyst and neural structures, minimal blood loss, and short hospital stays, the contralateral approach can create a difficult situation should the cyst recur and require repeated operative intervention.

As described by Sukkarieh et al., during the contralateral approach the ligamentum flavum is resected from the central canal to the contralateral lateral recess/facet. Therefore, after completion of the procedure, the contralateral dura is directly opposed to the underside of the contralateral lamina and facet complex. The ligamentum flavum no longer exists as a barrier between the two. If repeated surgery is then undertaken again via the contralateral approach there will be dense adhesion between the dura and the prior laminar defect and the undersurface of the contralateral lamina, adjacent to the cyst, which must be dissected to reach across to the facet in the contralateral lateral recess, putting the patient at an increased risk of CSF leakage. If an ipsilateral approach is taken, the surgeon will then encounter the dura immediately upon passing through the bone, without the ligamentum to serve as a buffer, and the patient will also be at an increased risk for CSF leakage. However, if the ipsilateral approach is used initially and the cyst recurs, ipsilateral revision is straightforward and the pathological process is generally lateral to the area of prior resection where the residual facet joint and ligamentum flavum still reside, offering some protection of the neural structures during the early stages of the procedure. Overall, the authors should be commended for their science and technical skill, but we believe that their approach may carry underappreciated risks for revision surgery, particularly in the hands of inexperienced surgeons.

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Disclosures

The authors report no conflict of interest.

Response

We read with interest the commentary on our article by Dr. Ammerman and colleagues. Our article describes our experience with 13 patients who had not previously undergone operations. Since the submission of this paper, none of these patients have experienced recurrence. We have not encountered any complications, CSF leaks or otherwise, during or subsequent to surgery.

Dr. Ammerman and colleagues believe that, in case of surgery for recurrent synovial cysts, complications are more likely when the index operation is through the contralateral approach. We have found that the contralateral approach to lumbar synovial cysts is safe, and that it safeguards the integrity of both the ipsilateral and contralateral facet joint. Because we have not experienced any recurrences, we cannot agree with Dr. Ammerman and colleagues that our approach is any worse than the ipsilateral approach in predisposing to recurrence or complexity of surgery. Most reoperations are more complex than the index operation, be they ipsilateral or contralateral. Perhaps time will prove Dr. Ammerman and colleagues' hypothesis right or wrong. Until data are available supporting one approach over the other, their argument, although logical, remains hypothetical.

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