Neurosurgical Forum LETTERS TO THE EDITOR

Laminotomy outcomes in pediatric patients

TO THE EDITOR: We read with keen interest the article by Arocho-Quinones et al.¹ comparing the outcomes of split laminotomy versus conventional laminotomy in pediatric patients (Arocho-Quinones EV, Kolimas A, LaViolette PS, et al: Split laminotomy versus conventional laminotomy: postoperative outcomes in pediatric patients. *J Neurosurg Pediatr* 21:615–625, June 2018).

We commend the authors for reporting this first of its kind study in the pediatric population, although it is well reported in adults. The use of developmentally appropriate pain scores and the rate of change in paraspinal muscle (PSM) fat infiltration rather than absolute value in order to account for growth are positive points. However, we would like to seek clarifications on some of the shortcomings that we found in the study.

The reason for the development of the split laminotomy technique is to minimize muscle dissection, thereby leading to a lower incidence of chronic pain, spinal instability, and deformity.⁴ This study had a relatively short follow-up duration (2 weeks to 4 years), and tended to focus on short-term outcomes such as postoperative pain control and PSM volume and fat atrophy. Only 37 patients underwent postoperative imaging, which could have led to bias in the results. A study with a longer follow-up duration considering the abovementioned outcome would serve a better purpose.

The split laminotomy approach is suited for midline spinal pathology, spanning single or multiple levels at various sites (cervical, thoracic, and lumbar).² However, in this study, only patients who underwent primary surgery for filum lipoma, spanning less than or equal to two levels, were considered in the statistical analysis. This is a very limited indication, and the results cannot be generalized to different spinal levels or pathologies, thus limiting the applicability of the technique. The authors also fail to report the incidence of conversion to conventional laminectomy and the factors associated with it.

Filar lipoma is usually associated with spinal dysraphism.³ In such cases, laminotomy or laminar distraction would not be possible and split laminotomy is thus not feasible. There was no report of the incidence of spinal dysraphism in either group.

The feasibility of the use of nonpenetrating titanium

clips for dural closure and technical difficulty with dural approximation with suture also need to be accounted for.

Finally, we would like to congratulate the authors on reporting the largest series of this technique in a pediatric population. This article lays the foundation for a larger study spanning various spinal levels for different indications with a longer follow-up duration.

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Disclosures

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Response

We would like to thank the authors of the Letter to the Editor for their valuable comments and critiques. As stated in our discussion, our study has several limitations, some of which are inherent of a retrospective study. Our follow-up times were variable, ranging from 2 weeks to 4 years, as many patients were lost to follow-up after their initial wound check appointment at 2 weeks. Only a minority of the patients had postoperative imaging available as we do not routinely obtain these studies on all patients.

We obtain postoperative imaging on patients with specific follow-up needs, whenever there is concern for symptom persistence or recurrence, or when there is concern for possible complications. This creates a potential bias, because presumably the patients with better outcomes (i.e., those with symptom resolution, normal wound healing, and no concern for complications) are those who would not otherwise follow up past their initial wound check evaluation. Given the obvious limitation in follow-up and available postoperative imaging studies, which precluded direct assessment of the development of spinal instability, spinal deformity, and chronic pain, we focused on the assessment of PSM fat infiltration and muscle atrophy,^{5,8–12} as this has been linked to the incidence of chronic back pain and is believed to predispose to spinal instability and progressive dysfunction.^{9,10,12}

Postlaminectomy spinal deformity is more prevalent in the pediatric population than in adults. Risk factors associated with iatrogenic spinal deformity include the use of adjuvant radiation, multilevel exposure, and the particular level of surgery (incidence is higher for cervical > thoracic > lumbar cases);³ hence we sought to compare equivalent-level surgeries. The statistical analysis was limited to lumbar cases for filum terminale sectioning as this group had a sufficient number of patients to reach statistical significance and represented a more homogeneous population for comparison. Unfortunately, the number of controlled thoracic-level cases was minimal, with only 4 cases for each surgical group and a disproportionately higher number of tumor cases in the conventional group (Table 1 in our paper), thus carrying additional confounding risk factors for the development of spinal instability and spinal deformity. Although we previously demonstrated the applicability of the technique at various spinal levels in the case report of an extensive multilevel exposure for debulking of a cervicothoracolumbar spinal cord lipoma,⁷ there were no isolated cervical-level cases for comparison in the current series.

As mentioned in our discussion, the split laminotomy affords sufficient exposure for dorsal/midline approaches such as filum sectioning, and we encountered no iatrogenic laminar fractures with the use of the laminar distractors; hence there was no need for conversion to conventional laminectomy in any of the split laminotomy procedures.

It is unclear to us what is meant by the statement “filar lipoma is usually associated with spinal dysraphism.” A lipoma of the filum terminale is a form of spinal dysraphism.^{1,2} If the authors refer to an association between filum lipomas and spina bifida, our experience has been similar to that noted in the excellent paper by Morota and colleagues⁶ that was referenced:

Indeed, the presence of a lipoma in the filum terminale, the final product of secondary neurulation, is increasingly being acknowledged. The availability of MRI has made the discovery of caudally located lipomas, which do not penetrate the dorsal aspect of the dura mater or fascia, and are unassociated with spina bifida, more common. It should be remembered that spina bifida diagnosed in such patients does not constitute true “pathological” spina bifida, but merely “physiological spina bifida” formed by the cartilaginous part of the lamina in the course of normal development.

Our findings are consistent with this statement, i.e., that the only spina bifida associated regularly in patients with filum lipomas is of the physiological form, a cartilaginous midline in the infant patients. The split laminotomy procedure remains the same in these patients. We should note that we choose lumbar levels below the conus rather than sacral levels for filum sectioning as this keeps the incision farther away from the diaper area and lessens the risk of stool contamination during the postoperative period.

In our experience, the width of exposure of the split laminotomy reaches approximately 1 cm (Fig. 9 in our paper), which limits its application to mostly dorsal/midline approaches and creates a practical challenge when using sutures for dural closure. As it was not an intended outcome of the study, we did not quantify the frequency with which nonpenetrating titanium clips were used. However, nonpenetrating titanium clips are routinely used for dural closure in our split laminotomy cases as it requires less exposure than do conventional suturing techniques.⁴

The results of our study reinforced the short-term benefits of the split laminotomy in minimizing acute postoperative pain and the longer-term benefits of decreasing muscle atrophy and fatty degeneration, which are known to be associated with the development of chronic pain and spinal instability. However, we agree that larger studies including various spinal levels, broader indications, and longer-term follow-up would be ideal.

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Pediatric thalamic tumors

TO THE EDITOR: I read with interest the report by Cinalli et al.¹ on their series of 27 children with thalamic tumors (Cinalli G, Aguirre DT, Mirone G, et al: Surgical treatment of thalamic tumors in children. *J Neurosurg Pediatr* 21:247–257, March 2018). I was disappointed that in their literature review they did not refer to our report on 72 pediatric thalamic tumors in the MRI era in Canada, which is one of the largest reported series of these tumors in children.² Cinalli et al. have shown again that radical resection of unilateral thalamic tumors can be performed relatively safely, although in the Canadian experience 19% of patients had permanent neurological deficits, mainly motor, after resection. With our larger series, we were able to do a multivariate analysis, which showed that outcome was related only to the tumor grade and not to the extent of resection. In the light of these findings, we need to be cautious in how strongly we pursue gross-total resection, even for low-grade thalamic tumors. Perhaps the goal of surgery should be maximal resection without causing new permanent deficits.

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Disclosures

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Response

We are grateful to Dr. Steinbok for spotting this missing reference³ in our article. The truth is that our paper had a long gestation and several versions, and in the last check of recent literature I overlooked their reference that had been published online a few months before. I (G.C.) alone am responsible for that. I personally encouraged Dr. Steinbok to write a letter to the editor to keep alive the debate on this difficult and challenging condition. The very nice report on the Canadian experience differs from ours because it is a large, multicenter study collected over a long period of time (11 centers over a 24-year period, 1989–2012). Their study involves several differences among centers' attitudes and surgeons' experiences and the evolution of technology over the studied period when compared with our series, a single-center, single-surgeon series of patients treated over a shorter period (14 years, 2002–2016). Collection of their patients started in a period when the surgeon had fewer surgical and diagnostic tools (less intraoperative monitoring [IOM] experience, no endoscopy, lower-field MRI, no diffusion tensor imaging [DTI]) and was highly influenced by the general conservative attitude that, as reported both in our and their papers, slowly changed after publications in the mid-1990s and early years of the new century. Our patients were treated in a period during which technology offered much greater help to the surgeon and, above all, a general attitude in favor of attempts for radical surgery was already largely established.

For these reasons, we would be very cautious about generalizing their conclusions. Their survival rate for low-grade tumors was 84%. In our 18 patients with low-grade tumors, only 1 patient died, due to malignant transformation of a low-grade thalamic ganglioglioma that was partially resected and conservatively followed up at the beginning of our series. This patient's course contributed significantly to a very drastic change in surgical attitude toward low-grade thalamic neoplasms at our institution. Moreover, Steinbok and colleagues' data clearly show that radical resection offers significantly higher chances of survival (80% vs 50%). These 2 points could be sufficient to close the discussion.

However, a major point needs to be stressed, which is the different use of staged surgery in the 2 series. In the study of Steinbok et al.,³ programmed staged surgery was never performed, and only 5 patients underwent reoperation, but only at the time of progression. We feel, in agreement with Puget et al.,² that staged surgery, far from being a fallback, should be considered as an elective treatment strategy in many cases of deep-seated or giant lesions in children, and low-grade thalamic tumors represent the ideal lesion to demonstrate its effectiveness. In fact, after

selecting the ideal approach that spares eloquent areas and the corticospinal tract on the basis of DTI findings, the main danger during surgery resides in the removal of the deepest part of the lesion at the tumor-parenchymal interface. The very last layer of tumor can be especially difficult to recognize at the end of the procedure, and its manipulation for dissection and removal can be especially dangerous for deeply located nerve fibers where the precision of IOM is questionable. Our strategy in these cases consists of planning the maximal safe resection, usually allowing removal of 80%–90% of the lesion in the first procedure, and removing the residual lesion, usually through the same approach, after a period ranging from 1 to 2 months after obtaining new MR images. During the second procedure, the residual tumor is much easier to recognize in terms of color and consistency and usually bulging into the surgical cavity, making total resection safer and feasible in our experience.

As a general rule, in thalamic low-grade gliomas incompletely resected at first surgery, we always perform a second-look surgery with the aim of complete resection before considering any adjuvant treatment (chemo-/radiotherapy).

For the aforementioned reasons, we feel that the retrospective review of the Canadian experience³ kindly brought to our attention by Dr. Steinbok offers an extraordinary historical perspective of a single nation on the treatment of this pathology. Nevertheless, in the field of low-grade tumors, we feel that their conclusions should be toned down and reviewed in the light of more recent and homogeneous experiences, where concentration in high-volume centers, systematic use of preoperative DTI, IOM, and intraoperative imaging offer significant support to the surgeon compared with 25 years ago, and we favor resection instead of conservative treatment. We agree instead with Dr. Steinbok that the role of surgery in the field of high-grade lesions remains debatable. In fact, although good surgical removal of a high-grade lesion seems to have a favorable impact on overall survival and progression-free survival,¹ their infiltrative pattern and their dimensions carry higher surgical risks in our experience, both for neurological function and life.

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Posterior-only approach: an erroneous phraseology

TO THE EDITOR: We read with interest the article by Deng et al.¹ (Deng A, Zhang HQ, Tang MX, et al: Posterior-only surgical correction of dystrophic scoliosis in 31 patients with neurofibromatosis Type 1 using the multiple anchor point method. *J Neurosurg Pediatr* 19:96–101, January 2017).

During the last couple of years, authors have come up with the label “posterior-only approach” in their endeavors in correcting deformities of the spine.^{1–4} The phraseology, although applied to convey the authors’ intended meaning, seemingly fails, thus leaving the reader with no option but to draw his own conclusions.

Deng et al. used the controversial phraseology “posterior-only correction or approach,” which creates some confusion for the reader.¹ Though the writer may have used it in good faith based on the valid assumption that the word “only” used as a postfix transmits the message that the author has in mind. To us, however, the word “only” fails to clarify the exact message, thus the reader is at a loss to grasp the correct meaning of the word “only.”^{1,5}

Phrases customarily consist minimally of a “head.” A string of elements may appear before the head or after the head, but the head needs to be clarified, otherwise it will lose all of its charm and thus appear ambiguous. Moreover, a prepositional phrase normally begins with a preposition and ends with a noun, pronoun, or a clause. Where does the word “only” fit in the titles mentioned in the aforementioned references, and what exactly does it convey?⁵ Perhaps a single hyphen or a dash could be used to clarify the confusion.^{1–3}

At times, a comma or a dash marks a slight break between different parts of a sentence. Used correctly, commas make the meaning of sentences more clear by grouping or else separating words, phrases, and clauses. Many times, authors display uncertainty about the correct usage of commas, hyphens, or dashes, and that is when their inappropriate display creates uncertainty for the reader. To conclude, we would suggest the titles of “posterior approach: the only-modality” or “posterior approach as the solitary modality” instead of using “posterior only approach” without any hyphen or dash transferring an erroneous impression for the reader and above all appearing semantically incorrect for the shrewd observer.

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Response

We thank Dr. Khan and colleagues for their comments on the phraseology “posterior-only correction or approach,” which possibly creates some confusion for the reader sometimes. They suggest the titles “‘posterior approach: the only-modality’ or ‘posterior approach as the solitary modality’ instead of using ‘posterior only approach’ without any hyphen or dash.” First of all, the phraseology “modality” is most commonly used in physics and mechanics^{1,10} but is rarely used in articles about spinal deformity surgery indeed. However, the phraseologies “posterior approach alone,” “a single posterior approach,” “a posterior approach,” “a single stage posterior approach,” and especially “posterior-only approach” are routinely used in articles about spinal surgery, which all mean “operation only by posterior approach.”^{3–6,9}

Secondly, the number of words in titles is restricted in many scientific journals, so conciseness, clarity, and generalization are required in titles. Meanwhile, the phraseology of titles should be particular and representative for convenience of a literature search and the needs of academic exchange and information transmission. Connected by a single hyphen or a dash, “only” follows close behind “posterior,” which is specific to the meaning of “only by posterior approach and without anterior approach,” and makes titles concise, representative, and easier to search by academic search engines.^{2,3,7} Thirdly, in literature searching, the phraseology “posterior-only approach” frequently appears in the title of articles related not only to spinal deformity, but also to spinal tuberculosis, tumor, and fractures.^{2,3,7,8}

Finally, the phraseology “posterior-only approach,” which means “operation only by posterior approach and

without anterior approach,” has been clearly elaborated in each of the previous articles related to spinal deformity.³ Accordingly, the common phraseology “posterior-only approach” is representative of correcting deformities of the spine at present. When the readers see the phraseology of a title, they can certainly understand how the authors operate in terms of surgical approach.

In conclusion, we would still suggest using the phraseologies “posterior-only approach,” “posterior approach alone,” and “a single posterior approach” in the title of articles related to spinal deformity for conciseness and representativeness.

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