Letters to the Editor

NEUROSURGICAL FORUM

The Chêneau brace

TO THE EDITOR: I read with great interest the article by Fang et al.³ (Fang MQ, Wang C, Xiang GH, et al: Long-term effects of the Chêneau brace on coronal and sagittal alignment in adolescent idiopathic scoliosis. *J Neurosurg Spine* 23:505–509, October 2015).

This study supports our previous findings on the high variability of the effect of the brace treatment.¹ We see once again that global statistics are misleading. The best illustration of this is Table 2. When compared to Table 3, we see that global statistics do not reflect the whole truth. For example, the Cobb angle did decrease in this study in half of the cases, but we see that in 10 patients it remained unchanged, and it even increased in 6 cases. Basing the analysis on the global statistics would lead to the conclusion that brace treatment is inefficient with regard to the Cobb angle, which is wrong in half of the patients in this study. The same comment is possible for all other parameters. This paper is once again a plea for a careful interpretation of global statistics when dealing with brace treatment. It is very important to spread this idea because our gold standards are built on global statistics. This is the reason why there is no consensus on brace treatment efficacy.

Initial in-brace correction data are not available in this study. It would have been of great interest to compare the initial in-brace correction and the final follow-up correction. This would help in finding predictive factors for brace success. We have already stressed the importance of collecting the largest amount of data before, in-brace, and at the end of the treatment.² This will give us the opportunity to refine numerical simulations, which are actually lacking a clinical validation such as this one.

The authors stress the importance of the brace maker's experience. In the future, we aim to circumvent this "craft" part of the process by numerical models. Analyzing the variability of the brace's effect on the first in-brace radiographs and throughout follow-up for each patient is essential.

This study highlights the possible adverse effect of brace treatment on the sagittal plane. We see here that brace treatment yielded a permanent thoracic hypokyphosis, which is usually related to an inappropriate positioning (too posterior) of the thoracic pad. We have demon-

strated that this kind of adverse effect of brace treatment may also be visible in the axial plane.¹ This is the reason why 3D analysis is now mandatory for us to assess the efficiency of all types of spine-related treatments.

It would have been interesting to analyze the initial curve magnitude of the "good" results with a decrease of the Cobb angle. This would have led to a discussion on the proper timing for initiation of bracing. In this study, the inclusion criteria were scoliosis ranging from 20° to 40°. In our opinion, 40° is too late to start a brace treatment. Even if the authors' findings suggest a slight decrease of the Cobb angle at last follow-up in half of the patients, a brace is more efficient when the curve is small. In our experience, the best corrections are achieved for small curves. Despite the actual recommendations of the Scoliosis Research Society (SRS), we believe that starting a brace as soon as possible, even for curves less than 25°, helps in obtaining a good correction (even overcorrection) and a better compliance. Those small reducible curves are amenable to nighttime bracing, which is less harmful on the sagittal balance.⁴

Finally, this study shows that brace treatment is difficult and still unpredictable. Because no validated numerical models are available, the success of the bracing relies on a perfect synergy between the surgeon and the brace maker and, above all, on a systematic clinical and radiological evaluation of each case.

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Disclosures

The author reports no conflict of interest.

Response

Thank you very much to Dr. Courvoisier for his letter and advice. We know that he has deep accomplishments in this field. It's a great honor to have an exchange of communications with him.

We agree with his viewpoint on global statistics. Global statistics are the foundation of our gold standards for brace treatment. But we must be clear that the main objective of brace treatment is to stabilize the shape of the scoliotic spine and prevent curve progression, and we defined the success of bracing as an unchanged or decreased curve. The success rate of brace treatment in our study was 81%, not 50%, which meant that the global statistics may not be misleading. The same interpretation is possible for all other parameters.

The collection of initial in-brace correction data was what we had to do. The immediate correction of adolescent idiopathic scoliosis (AIS) by the brace has been well documented.² The purpose of this study was to analyze the variability of the long-term effects of bracing on AIS, so we haven't compared the initial correction, in-brace, and the final follow-up correction. It's a good idea to design a numerical model that can predict the progression of brace treatment. We sincerely hope to cooperate with Dr. Courvoisier in this field.

Most spine surgeons today agree that scoliosis is a complex 3D spinal deformity, and a 3D understanding of the curvature is therefore crucial for assessing deformity, determining proper treatment, and tailoring surgical procedures for the correction of the condition.^{1,3} Because of the limitations of radiographic techniques, we did not evaluate progressive correction of transverse plane parameters in this study. Accurate 3D global and local morphological information has the potential to improve deformity assessment, advance understanding of the scoliosis deformation, and ultimately optimize the treatment strategies. The development of a relatively new low-dose x-ray device (the EOS system) has the capacity to allow 3D spine reconstructions to be created from biplanar (posteroanterior and lateral) standing radiographs, to which it is critically important to minimize exposure, especially in developing children and adolescents.

There is still controversy about the proper timing of bracing initiation. Because we do not have much experience, we defined the initial Cobb angle according to the recommendations of the SRS.

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The Brain and Spinal Injury Center score

TO THE EDITOR: I read with great interest the article by Talbott et al.¹ (Talbott JF, Whetstone WD, Readdy WJ, et al: The Brain and Spinal Injury Center score: a novel, simple, and reproducible method for assessing the severity of acute cervical spinal cord injury with axial T2-weighted MRI findings. *J Neurosurg Spine* **23**:495–504, October 2015). As I understand it, the purpose of the study is to classify the pattern of intramedullary T2 signal hyperintensity on axial MR images of the cervical spine obtained after a trauma into an ordinal-type, categorical variable that they have named the BASIC (Brain and Spinal Injury Center) score. The scores range from 0 to 4 and the scoring system is being developed with the hope that better categorization of axial-image T2 signal hyperintensity alone can help with prognosis of AIS (American Spinal Injury Association [ASIA] Impairment Scale) grade.

In reviewing Fig. 1, I was troubled by the presence of an obvious confounding factor that was never mentioned in the article. The examples shown clearly demonstrate that the spinal cord has been physically deformed in several images. The normal elliptical shape of the cord seen in typical axial T2-weighted MR images in BASIC 0 is clearly not present in BASIC 2 and 3; rather the cord is crescent-shaped from what seems to be severe ventral compression. Strangely enough, even their “cartoon schematic” of BASIC 3 clearly showed a different shape; compared to the other examples, the cartoon image for BASIC 3 looks compressed. I believe this factor cannot be ignored because it compromises the validity of the scoring system. For example, what would the shape of central gray matter be in a crescent-shaped cord? That is, would observers reliably be able to distinguish a crescent-shaped BASIC 1 from a crescent-shaped BASIC 2?

If a patient presents with a BASIC 2 or BASIC 3, as shown in their images, with compression, and is not AIS Grade E, then an urgent surgical decompression is indicated. How then, can we be sure that any improvement in functional outcome is not due to the intervention? Another way of thinking about this is to imagine 2 patients with a BASIC 3 score, one of whom has a compressed spinal cord while the other does not. The management of these 2 cases is going to be different, but this was not addressed in the paper. The authors did not mention what surgical

interventions were done in this cohort after the imaging, although they took care to say that no surgery was performed before the MRI studies. I think the study would have been more useful if MRI was performed after the decompression.

Also, how do we know there are no microhemorrhages hiding in the crescent-shaped BASIC 3? Assuming a surgical decompression was performed, I would not be surprised if the expansion of the cord back to its normal shape allowed those microhemorrhages to expand, thereby advancing the score to BASIC 4.

Another issue with the study is that the method for scoring BASIC 1 was insufficiently described. From a practical perspective, distinguishing between BASIC 0, 1, and 2 essentially relies on being able to determine if there is pathological hyperintensity within the gray matter on axial T2-weighted fast spin echo (FSE) images. The problem is that gray matter hyperintensity on T2-weighted FSE images is a rather non-specific finding. Distinguishing pathological hyperintensity due to trauma depends on several factors: 1) history, 2) location (the authors relied on the sagittal T2 sequence to help locate the “epicenter” of the spinal cord injury [SCI]), and 3) the magnitude of the intensity compared to what may be normal (this can vary from patient to patient). Figure 2A shows the T2 MERGE (multi-echo recombined gradient echo) sequence from a single normal patient that was used “as a control reference for identifying margins of gray matter at the upper, middle, and lower cervical levels.” Figure 2B shows manual image segmentation of the single normal patient MERGE sequence, although the authors did not mention what purpose this served in scoring. Then, Fig. 2C goes directly to pathological BASIC 1 SCI images that, we must assume, are FSE sequences. The authors mentioned that, during testing for inter-observer reliability, each rater was shown images of both the injury epicenter and the adjacent normal-appearing spinal cord. Assuming the observers were shown normal FSE images obtained in the same patient to compare to the pathological ones, why did they publish MERGE images from a single normal patient? It would have been more useful to point out the difference in FSE images. As it is described, the authors seem to make scoring a BASIC 1 seem easier than I think it would be in reality. Another option would be instead of comparing the FSE images to the MERGE images from 1 normal patient, simply compare the FSE images to MERGE images obtained in the same patient. Most cervical MRI trauma protocols (including the NINDS SCI Common Data Elements mentioned by the authors [<https://commondataelements.ninds.nih.gov/SCI.aspx>]) recommend axial T2-weighted images for both FSE and gradient echo sequences. I do not know if restricting their protocol to axial T2-weighted FSE images was entirely necessary.

The online images of the 3D-surface color plot created by ImageJ software in Fig. 2 looked nice but there was absolutely no explanation for how it was generated or why. In fact, it leads to more confusion. BASIC 0 normal spinal cord is red. The normal portions of the spinal cord for BASIC 1 and 2 are purple. The authors stated this color plot was not used for primary image analysis or interrater

observability testing but only for figure production. What are we supposed to learn from this figure then?

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Disclosures

The author reports no conflict of interest.

Response

We appreciate Dr. Bhangoo’s interest in our recent article and this opportunity to clarify a few points of confusion. Below we would like to address each of Dr. Bhangoo’s comments in order as outlined in his letter.

Dr. Bhangoo expressed concern that varying degrees of spinal cord compression/deformation might confound the prognostic capacity of the BASIC score—specifically, that cord deformation might complicate distinction between BASIC 1 and BASIC 2. To put it simply, this is not the case. We have observed varying degrees of spinal cord compression and deformation within all BASIC subgroups in more than 200 SCI patients at our institution, and cord compression does not significantly impair BASIC classification in our experience. We remind readers that the BASIC 1 score is assigned to cases in which pathological T2 signal hyperintensity is “approximately” confined to gray matter. BASIC scoring is a pattern approach that does not require that signal intensity precisely respect the exact white matter boundary lines. When the cord is compressed, it is not difficult for those experienced with spinal MRI interpretation and spinal cord anatomy to infer the approximate morphology of the deformed “butterfly” appearance of central gray matter in the cord. Our high BASIC score interrater reliability confirms this fact.

A second and perhaps more important related point raised by Dr. Bhangoo involves the potential confounding effect of spinal cord compression on surgical decision making and outcome. This represents a valid concern that is not specifically addressed in the article Dr. Bhangoo is writing about. However, utilizing multivariate statistical analysis, we recently published our results on the prognostic validity of numerous MRI findings in thoracolumbar SCI, including BASIC score, metrics of spinal cord compression, and spinal canal compromise.¹ Importantly, the BASIC score was highly predictive of outcome in this cohort, independent of spinal cord compression or canal compromise. We are encouraged by preliminary findings in ongoing studies utilizing similar analysis in a larger cohort of cervical SCI patients, which confirm these findings for the more common cervical SCI and further suggest that the BASIC score may be a robust and simple image classification scheme for prognosis in acute SCI.

Regarding the potential for undetected microhemorrhages, the *JNS: Spine* study did not utilize susceptibility-weighted sequences for their detection. Pathological analysis of human SCI specimens demonstrates that microhemorrhages are almost certainly present in many BASIC 1, 2, and 3 injuries. While their prognostic significance is of interest, determining the presence of microhemorrhages was not our aim. Rather, we aimed to develop a simple axial T2-weighted MRI-based classification system that reflects the transverse extent of acute pathological signal abnormality in the cord and correlates with injury severity and outcome. Axial T2-weighted imaging is universally applied in spinal cord MRI protocols and does not require specialized hardware or post-processing; thus we believe that this represents a pragmatic scoring system applicable to practitioners in academic and private practice settings. Because macrohemorrhage is readily visible on axial T2-weighted MRI sequences and has been previously shown to be a strong predictor of poor outcome, we incorporated its presence into our BASIC classification scheme. Whether microhemorrhages are present in BASIC 3 (or other) injuries and might expand after surgery is inconsequential to the study in question, as our focus was on the imaging appearance of the preoperative spinal cord. Indeed, surgical decompression may drastically alter perfusion dynamics and tamponade effect of spinal canal stenosis, thus significantly complicating the postoperative appearance of the spinal cord on MRI. While these surgery-related changes may be of clinical interest, they are not germane to our preoperative MRI prognostic scoring system.

Dr. Bhargoo questions the specificity of T2 signal hyperintensity in our acute SCI patients. While T2 signal hyperintensity in the spinal cord after acute injury may relate to a variety of traumatic pathological changes, including vasogenic edema, inflammatory infiltrate, reactive astrogliosis, and cytotoxic edema, multiple experienced readers in our study confirmed that the T2 signal hyperintensity observed in our cohort was pathological and related to the patient's acute SCI. The normal spinal cord has relatively uniform isointense T2 signal with little to no intrinsic contrast between gray and white matter. For this reason pathological signal can be readily identified and localized within the cord, which explains why T2-weighted imaging has been the workhorse for SCI detection since the widespread adoption of clinical MRI in the 1980s. In contrast, the MERGE sequence accentuates intrinsic T2* relaxivity differences between gray and white matter in the spinal cord. In our experience, the intrinsic hyperintense signal of normal gray matter in the MERGE sequence complicates the detection of pathological edema confined to gray matter and thus limits its application for BASIC scoring. Our use of MERGE images in Fig. 2 was simply illustrative. As clearly stated in the figure legend, these images were included to demonstrate the normal gray matter morphology at upper, middle, and lower cervical vertebral levels and to familiarize the reader with normal internal gray matter morphology on MRI for assigning BASIC 1 and 2 scores across cervical levels.

The 3D color surface plots in Fig. 1 are also illustrative and were merely included to graphically highlight the BASIC patterns. As clearly described in the *Methods* sec-

tion, these images were constructed from the original T2 DICOM images using a 3D surface plot plugin in ImageJ. Variability in color scale simply represents slice-to-slice variability in signal intensity and does not interfere with the 3D graphical distinction in BASIC patterns.

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Primary spinal epidural cavernous hemangiomas

TO THE EDITOR: We read with interest the article by Li et al.³ (Li TY, Xu YL, Yang J, et al: Primary spinal epidural cavernous hemangioma: clinical features and surgical outcome in 14 cases. *J Neurosurg Spine* 22:39–46, January 2015). The authors presented their experience with 14 cases of solitary spinal epidural cavernous hemangiomas (SSECHs) and discussed nicely their clinical characteristics, differential diagnosis, radiological features, and treatment options. There are a few points worthy of being mentioned and added to their collection to make a more complete and informative communication.

1) The authors unfortunately omitted our report² in their reference list reporting 9 SSECHs. 2) As mentioned in our article, there had been more than 104 similar cases published up to 2013 to which we added our 9 cases. 3) The clinical presentation and dissemination of the lesions were similar to those reported by Li et al.,³ but the MRI characteristics were different—i.e., they were all isointense on T1-weighted images and hyperintense on T2-weighted images, homogeneously enhancing after contrast material injection. We did not encounter any cases of SSECH in which the lesion was hyperintense on T1-weighted images. We also did not observe a hypointense ring shape change in any of our cases and stressed that this stigma was the main difference between the extradural and intradural-intradural cavernomas. 4) Massive intraoperative bleeding was not encountered in any of our cases while we operated with all the patients in the standard prone position. 5) Gross-total resection was achievable in all of our patients without any recurrences seen in the follow-up period, but Li et al. had to leave some of the lesion when uncontrollable bleeding occurred, even though their patients were operated on in the lateral position. 6) The use of an-

giography in the preoperative evaluation of spinal arteriovenous malformations (AVMs) is essential even in cases of occult spinal AVMs.¹ Relatedly, it is suggested that if any lesion suggestive of an SSECH is detected on MRI and contains a flow void area that is hypointense on T2-weighted imaging, a vascular study should be performed before surgical intervention.

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Disclosures

The authors report no conflict of interest.

Response

We would like to thank Dr. Amirjamshidi and colleagues for their interest in our article.

Most epidural cavernous hemangiomas were isointense on T1-weighted images and hyperintense on T2-weighted images, and intralesional hematoma of different age or even cystic lesion can be hyperintense on T1-weighted images. Sudden clinical deterioration usually indicates hemorrhage, either intralesional or perilesional, and perilesional hemorrhage can evolve into a hypointense signal over time. Hemosiderin signal can be seen in epidural hematomas, but not as commonly as in intramedullary cavernomas, since the epidural hematoma and hemosiderin can be absorbed more efficiently.

In principle, like other vascular lesions, cavernous hemangioma should be removed en bloc to avoid uncontrolled hemorrhage, but because many epidural cavernous hemangiomas extend outside the spinal canal through one or multiple neural foramina, they must be transected, and massive bleeding can occur. Engorged, fragile epidural veins can also contribute to the hemorrhage after sudden decompression, and, in this case, we prefer to apply Gelfoam rolls for hemostasis. We believe the lateral position has advantages over the prone position when this type of hemorrhage occurs because this position allows the blood to drain away from the operative field for better exposure.

Because epidural cavernous hemangiomas are uncommon lesions and share many radiographic features with other common tumors, like schwannoma, ependymoma, and meningioma, most of our cases were misdiagnosed

preoperatively, and thus spinal angiograms were not acquired. In cases in which spinal angiography was performed, the lesions were angiographically occult; only a tumor blush could be seen in the delayed venous phase. Hypointense flow void areas are usually not associated with epidural cavernous hemangioma on T2-weighted images, but when it does show, spinal angiography should be performed to rule out other types of spinal AVMs, and, otherwise, spinal angiography is not very helpful for diagnosing spinal epidural hemangioma.

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Atlantoaxial instability

TO THE EDITOR: I follow with great interest the scientific debate¹ regarding the role of atlantoaxial instability in the pathogenesis of Arnold-Chiari malformation Type I (CM-I), described by Prof. Atul Goel² (Goel A: Is atlantoaxial instability the cause of Chiari malformation? Outcome analysis of patients treated by atlantoaxial fixation. *J Neurosurg Spine* 22:116–127, February 2015).

In his landmark description of the condition Prof. Goel uses the term “listhesis.”³ The Greek word ολισθησις, olisthesis (slippage), comes from the verb ολισθαίνω (to slip). In the term spondylolisthesis, the final “o” from the word spondylo (vertebra) and the initial “o” in olisthesis merge. The same applies to other terms of Greek origin; for example, the combination of πλάσμα, plasma, and αφάιρεσις, apheresis, (removal), becomes plasmapheresis.

May I respectfully suggest to Prof. Goel that the correct term “olisthesis” replace “listhesis” in order to avoid upsetting Hippocrates and his friends!

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Disclosures

The author reports no conflict of interest.

Response

Man has a swollen head and a shrunken, atavistic tail. Yet, in Nature's eye the two are on par. It is fascinating to realize that the well-recognized lumbosacroli¹sthesi¹s has a cranial counterpart atlantoaxialfacetallisthesi¹s. Nature's penchant for embryologically treating the hind-area (proc-todeum) on par with the mind-area (stomodeum) gets re-flected even in the variations of the craniovertebral axis.¹ No wonder the sacralization of the lumbar vertebra has its cranial image in occipitalization of the atlas. Realization of presence of "listhesis"—or if Hippocrates is to be happy "olisthesis"—symbolizing instability of facets of atlas and axis has a great potential for enhancing the understanding of the region. As they say, a rose by any other name will

smell as sweet (after William Shakespeare). What is there in a name, philosophy matters!

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