Letters to the Editor

NEUROSURGICAL FORUM

The Chêneau brace


This study supports our previous findings on the high variability of the effect of the brace treatment.4 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. Bas- ing 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.

Aurélien Courvoisier, MD, PhD
Grenoble University Hospital, Grenoble Alpes University, Grenoble, France

References
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. 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. 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 deformations, 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 brace initiation. Because we do not have much experience, we defined the initial Cobb angle according to the recommendations of the SRS.

Ming-Qiao Fang, MD
Hua-Zi Xu, MD
Zhejiang Spine Research Center, Second Affiliated Hospital of Wenzhou Medical University, Wenzhou, Zhejiang, China

References

The Brain and Spinal Injury Center score

TO THE EDITOR: I read with great interest the article by Talbott et al. (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