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Letters

To the Editor:

Re: Cheng JC, Chau WW, Guo X, et al. Redefining the Magnetic Resonance Imaging Reference level for the Cerebellar Tonsil: a Study of 170 Adolescents With Normal Versus Idiopathic Scoliosis. Spine 2003; 28(8):815-818

We read with interest and concern the article by Cheng *et al*. In this article the authors redefine a normal MRI reference level for cerebellar tonsils. They reference existing literature, which generally defines Chiari malformation as tonsillar herniation 5 mm below the foramen magnum (basion-opisthion line)[1,2] and then create their own standard based on review of 117 patients with adolescent idiopathic scoliosis and 53 controls. The authors believe that any descent of the cerebellar tonsils below the foramen magnum should be considered pathologic, and state that "scoliosis could be an important manifestation of subclinical tonsillar herniation." The authors did not discuss the degree of tonsillar compression on MRI, nor did they discuss the role of CSF flow studies. They did not consider whether minor tonsillar descent below the foramen magnum may actually be an epiphenomenon of idiopathic scoliosis. They assume that scoliosis is a result of tonsillar herniation, and imply that all tonsillar herniation should be treated in patients with scoliosis.

As pediatric neurosurgeons we are quite concerned that this article opens the door to unnecessary surgical treatment of completely asymptomatic "Chiari malformations" in pediatric patients with idiopathic scoliosis. Chiari malformation is probably one of the most overly operated cranial conditions in neurosurgery. In response to overuse/abuse of the operation, the American Association of Neurologic Surgeons published a position statement regarding widespread Chiari decompression surgery,[3] indicating the magnitude of the problem. In 2000, an extensive survey of pediatric neurosurgeons in the United States overwhelmingly demonstrated their unwillingness to perform prophylactic surgery for asymptomatic patients with tonsillar descent, and in that study the threshold was 5 mm below the foramen magnum.[4] Further studies looking at 22,000 patients have shown a significant incidence of asymptomatic Chiari malformations in the general population, again defining Chiari malformation as tonsillar descent greater than 5 mm.[5] If these recommendations by Cheng *et al* are accepted as guidelines for treatment, there will be countless surgical decompressions that will be of little or no benefit to the patient. As pediatric neurosurgeons with extensive experience with Chiari malformations, we would strongly recommend restraint on the part of physicians in drawing any treatment conclusions from this article, which will put hundreds, if not thousands, of adolescents at risk for unnecessary surgery.

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In Response:

We read with interest the comments from Dr. Mark Proctor and Dr. R. Michael Scott on our published article and would like to thank them for their interest and concerns. We are aware of the fact as pointed out by them that there is a significant incidence of asymptomatic Chiari malformations in the general population and agree totally that overuse of the decompression neurosurgery should be cautioned against.

However, we do think that the comments in the letter are overstated, *e.g.*, "They assume that scoliosis is a result of tonsillar herniation, and imply that all tonsillar herniation should be treated in patients with scoliosis." We draw conclusion from our results that any tonsillar herniation, even the inferior displacement of less that 5 mm, should be considered (diagnosed) as structurally abnormal or, in other words, pathologic in adolescent patients with idiopathic scoliosis. However, we have never stated that, nor is it our intention to imply that all tonsillar herniation should be treated in patients with scoliosis.

Dr. Proctor and Dr. Scott should understand well that "pathologic" could cover a wide range from "of diagnostic significance" to "of treatment significance," or even "of surgical treatment significance." For instance, we could have in the case of adolescent idiopathic scoliosis (a permanent pathologic change of the spine) presenting with Cobb's angle of approximately 20 degrees, which itself is not equivalent to an indication for surgical correction. Our study was not designed to answer any questions related to the treatment of adolescent idiopathic scoliosis through decompression of tonsillar herniation.

Our observation that "scoliosis could be an important manifestation of subclinical tonsillar herniation" is a hypothesis that certainly would require further elaborated studies before drawing any definitive conclusion on a causal related effect. Ongoing studies by our group include the CSF flow dynamics and the volume/morphologic changes of the tonsils and their relationship with the degree of tonsillar herniation as mentioned by Dr. Proctor and Dr. Scott. We are also studying the SEPs and postural balances of scoliosis patients and their correlation with MRI findings on top of many other related studies.

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To the Editor:

Re. Sasso RC, Kenneth Burkus J, LeHuec JC. Retrograde Ejaculation After Anterior Lumbar Interbody Fusion Spine. 2003;28:1023–1026

We read with interest the article by Sasso *et al*. The authors conclude that there is a 10-fold increase in the occurrence of retrograde ejaculation when the transperitoneal approach to the lumbosacral junction is used *versus* the retroperitoneal approach. We found this conclusion to be at odds with our practice and have considered reasons for the disparity.

In our series of anterior spinal cases, either as part of a circumferential fusion using pedicle screw fixation posteriorly, as a stand-alone fusion, or as a disc arthroplasty, prospectively followed since 1997, no cases of retrograde ejaculation have occurred in 46 men. In this period, a total of 121 anterior spinal cases have been performed. Of the 46 patients, 17 procedures were carried out using a retroperitoneal approach and 29 used a transperitoneal approach, all by the same surgeon.

Patients were warned of the risk of retrograde ejaculation, and six men younger than 40 years of age elected to bank sperm before surgery. However, at follow-up none had experienced even temporary ejaculatory disturbance. Our transperitoneal patient numbers are comparable with those of Sasso *et al*, and if the risk of retrograde ejaculation is as stated by the authors, we would have expected less than one case for the retroperitoneal procedures but as many as three for the transperitoneal cases. In our opinion, our series, conducted in one center by a single operator, should give a reasonable guideline to the operative risk for these patients. If this is the case, it would seem that either our patients have been extraordinarily lucky over the last 6 years or that there are confounding factors in the Sasso *et al* study that might not have implications for the generality of spinal surgery, in which case the conclusions of the study are less relevant than they might appear at first sight.

The Sasso *et al* series was part of an FDA IDE study examining the efficacy of anterior interbody fusion devices. It was multicenter with each participating surgeon carrying out relatively few cases. The paper does not indicate the overall level of experience of the participating surgeons and particularly does not comment on how many anterior approaches each surgeon had performed before enrolling patients into the trial. In addition, there is no comment made on how many spinal surgeons carried out surgery with the assistant of a general surgeon. It is therefore impossible to know where each surgeon was on the anterior spinal surgery "learning curve."

In the diagrams of the anterior lumbar spine in the Sasso *et al* report, the superior hypogastric plexus is seen as being swept off the left common iliac vein *en masse* for the retroperitoneal approach, but it is retracted to the left and right in the transperitoneal approach. Direct access to the L5–S1 disc through the middle of the plexus will, of course, put the elements of the plexus at higher risk than if they are gently retracted to the left or right.

This issue is addressed directly in Frymoyer's seminal text *The Adult Spine: Principles and Practice*: "The hypogastric plexus . . . typically courses over the left iliac vessels and the surface of the sacral promontory to reach the anterior surface of the sacrum. In order to expose the intervertebral disc without damaging the plexus, the posterior peritoneum should be opened carefully on the right-hand side of the bifurcation. . . . After bluntly dissecting down to the anterior vertebral cortex and disc on the right side of the interspace, the presacral tissues may be elevated from the periosteum and swept from the front of the sacrum as a block. Care must be taken to avoid dissecting through the plexus, cutting transversely across it or using cautery during the dissection."[1]

If this really was the surgical technique used for the transperitoneal approach, it may be the main reason for the reported incidence of retrograde ejaculation in these patients. We agree with the authors that the use of blunt dissection in the retroperitoneum and the very limited use of monopolar diathermy are important as is operator experience, but these might be less critical in preventing retrograde ejaculation if the plexus is subjected to a traction injury anyway.

We would suggest that, even with a transperitoneal approach, the disc space may be approached from its lateral side once into the retroperitoneum, and with blunt dissection the vessels as well as the superior hypogastric plexus may be mobilized with minimal trauma laterally, thus creating the space needed for implant insertion.

We are not suggesting that this procedure can be performed without complications, but meticulous surgical technique will reduce the risk to an acceptable level. In our opinion, this paper gives a distorted view of the risks of a transperitoneal approach to L5–S1 and would suggest that there are too many confounding factors in the study to assume that their conclusions are valid.

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Reference

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To the Editor:

Re: Aure OF, Nilsen JH, Vasseljen O. Manual therapy and exercise therapy in patients with chronic low back pain: A randomized, controlled trial with 1-year follow-up. Spine 2003; 28: 525-531

This article obviously received high praise from the editors in that it presented a randomized controlled trial of two different modes of therapy for chronic low back pain. Most manual therapy as well as what is said to be exercise therapy had equal affect on the participants. This is a total of 49 patients. The only physical measurement was range of motion, although several pen and paper tests were provided such as the visual analog scale, Oswestry Disability Questionnaire, Dartmouth COOP function charts, and return to work statement. The exercises, however, were by no means standardized, and were said to include strengthening, mobilizing, coordination, and stabilizing exercises for the abdominal, back, pelvic, and lower limb muscles, suited to the clinical findings. The therapist was free to chose the type and the characteristics of the exercise. It took place with or without training equipment. The manual therapy patients, in addition to that treatment, had five general exercises as well.

I am astonished that this paper received first article status. It was by no means a clean comparison, and it was to a very small number of patients. Manual therapy treatment was diluted with a significant amount of exercise.

A more serious area of criticism, from my standpoint, however, is the blur of exercise treatment. For the exercise therapy treatment, it was merely total body therapy, with no apparent rationale offered. Worse than that, the efficacy of therapy was not defined by any physical test other than spinal range. It is not surprising that both groups improved in spinal range in that both groups did stretching exercises to the spine. A more pertinent definition of exercise treatment would be one that offered a baseline of performance and noted changes at the conclusion of the exercise program indicating whatever exercise had been done was effectively performed. Unless the exercise is measured in some manner, its therapeutic effectiveness cannot be assessed. In that the exercises provided by the therapists were quite varied, there is no way to assess what might have been more or less effective therapy as well. The program described exercise therapy seems to me just as ridiculous as if one would treat osteomyelitis with red or blue antibiotic capsules as often as you like, and did not take follow-up radiographs or blood tests. How can such a description of exercise therapy receive high marks in such an evidence-based journal as *Spine*?

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In Response:

As we are honored by Dr. Mooney's interest in our study, we would like to respond to his criticism of our article. Dr. Mooney makes a point of the exercises not being standardized for the patients and that the study did not offer a clean comparison. In our opinion, a major criticism to previous research in this field is the lack of individualization of treatment. No two patients are alike, particularly for low back pain (LBP), making it mind-bending to justify standardized treatments. This fact is receiving more attention in the evolving biopsychosocial research interest to LBP, which in its term acknowledge the complexity and diversity of LBP and treatment choices.

Dr. Mooney criticizes the choice of exercise for lack of rationale. There is no evidence to argue in favor of one exercise approach over another, neither on basis of pathophysiological mechanisms nor previous intervention studies on LBP. The International Paris Task Force, as report by Abenhaim et al in Spine 2000; 25: 1–33, and as stated in our article on pg. 525, concludes that "there is sufficient scientific evidence to recommend that patients who have chronic low back pain perform physical, therapeutic, or recreational exercises, keeping in mind that no specific active technique or method is superior to another." This conclusion is also supported by the Swedish Council on Technology Assessment in Health Care (for references see our article in *Spine*). The manual therapy approach in our study emphasized exercises and segmental techniques attuned to specific findings by the manual therapist as opposed to the more general exercise approach. Exercise therapy in our study complies with how most general physiotherapist conduct exercises in Norway and elsewhere. Dr. Mooney unmasks his lack of knowledge about manual therapy when he remarks that it was diluted with exercises, when in fact specific exercises have always been integrated with this approach. With a little more cautious reading Dr. Mooney would also find explanations to what he erroneously perceives as blurred exercise treatment (see pgs. 527, 530 and 531).

The efficacy of therapy should, according to Dr. Mooney, be defined by a physical test. We strongly disagree as the efficacy of therapy in our world is defined by reduction of pain and improvement in function, including return to work. A physical measure or test is at best an intermediary or explanatory variable for improved health status in the patients, not a measure of health improvement in itself. So far, there hardly exist any biomarkers or structural changes indicative of LBP or change in LBP. The remark about red and blue antibiotic capsules is thus absurd and irrelevant. We used outcome measures commonly adopted in low back pain studies worldwide, which we found clinically relevant. On the other hand, the study is of course, not perfect, as we believe we carefully discussed in our article (pg. 529), for example the small number of patients, the flexibility and diversity of the intervention, and issues related to interpretations of the results.

We are still honored to have raised engagement and have confidence in *Spine* as a journal of the highest standards.

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To the Editor:

Re: Shao Z, Rompe G and Schiltenwolf M. Radiographic changes in the lumbar intervertebral discs and lumbar vertebrae with age. Spine 2003; 27: 263-268

In a series of previous studies,[1–3] our group developed a new precise protocol for measuring the height of human lumbar discs, which compensates for radiographic magnification, axial rotation, lateral tilt, and off-center positioning. Normal values of age and gender dependent disc height were presented.[2,3] We appreciate that Drs. Shao and colleagues refer to our measurement protocol, replicating our illustrations, and copying some text passages word-by-word from our previous work.[4] If this gave the impression that Drs. Shao and colleagues actually used our measurement protocol and that the results of our previous studies and those of the aforementioned publication of Drs. Shao and colleagues were comparable, this would be misleading.

First, our measurement protocol to determine disc height is based on "corners" on the vertebral contours, which are objectively located by a computer algorithm. This was performed to exclude subjective errors. Drs. Shao and colleagues apparently base their disc height data on points subjectively placed on the contours.

Second, our protocol states disc height in relative units, *i.e.*, divided by the mean depth of the vertebrae. This was performed to compensate for variations in radiographic magnification. Drs. Shao and colleagues apparently quote disc height in centimeters, as measured from the radiographs (although actual units of disc height are not given either in the text or in the legends of their article). Because radiographic magnification is generally not known when processing archive images, this will introduce a considerable scatter in the data. For this reason and because of the subjective placement of the landmarks, our error for determining disc height (*i.e.*, 3.9%) cannot be applied to the results of Drs. Shao and colleagues as cited.

Third, Drs. Shao and colleagues mention that they angle-corrected their measured disc heights using correction coefficients determined in our previous studies. Our correction coefficients, however, refer to disc heights given in relative units as described. Applying these coefficients to disc heights measured in centimeters will fatally flaw the correction.

Fourth, Drs. Shao and colleagues measure mid height of vertebral bodies and quote a concavity index. Vertebral contours on radiographs do not, however, look like the example shown in their Figure 3, except for vertebrae imaged in the central beam. In fact, because of central projection, the outer contours of vertebrae imaged off-center are convex (see their Figure 2 for a mild example). It follows that these outer contours cannot be used to measure mid vertebral height. Alternatively, if Drs. Shao and colleagues relied for the measurement of vertebral mid height on the eye-catching, broad, radiographic, dense lines seen within the vertebral silhouette on radiographs, their data are fatally flawed. It is common radiographic knowledge (and in addition was actually illustrated in a previous study[1]), that these lines originate from structures beneath the vertebral endplate. They do not reflect the concavity of the endplate.

Finally, in contrast to what the legend to their Figure 2 suggests, Drs. Shao and colleagues quote no data from a previous report.[4]

We mention these points so that readers may put the findings from Drs. Shao and colleagues into perspective.

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physical exertions or vibration at the workplace. Part I. Shape of vertebrae and intervertebral discs. Study of a young healthy population and a middle aged control group. *Clin Biomechan* 1994;9Suppl1:S1–S83.
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In Response:

It gives us great pleasure to answer the questions of Drs. Frobin and colleagues. The two dorsal corners (1 and 3), which are positioned on the outermost contours, and the two ventral corners (2 and 4) can always be distinguished. We determine disc height on the basis of the corners placed on the vertebral contours. The precision of the disc height measurement of Drs. Frobin and colleagues amounts to 3.9%. The concavity index (vertebral body) is derived by the formula B/A (refer to the measurement protocol of Twomey and Taylor[1]). The data we quote is from figure 2 of the article by Leivseth *et al* (reference 4 of Letter).

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