Letters to the editor

Idiopathic Spinal Cord Herniation

TO THE EDITOR: I read the interesting article by Barrenechea et al.,
which clearly illustrates the importance of this condition and the
potential for misdiagnosis and delayed diagnosis. In my experience,
clinically improved, where-as the patient in Case 7 (a patient with
ISCH at vertebral body level) did not. Clearly, clarification of this
potential correlation might help with accurate case selection.

Barrenechea et al. did not appear to consider the possibility
that ISCH might represent neurenteric canal remnants. Such a possibility
would explain the anterior location of the dural defect, as well as the
coeXistence of ISCH with occasional congenital anomalies.6 No other expla-
nation (apart from that implied by the case reported by Ewald et al.)
adequately explains the existence of the all-important anterior dural defect.1

Regarding symptom production, Barrenechea et al. have overlooked
the important case report of Ewald et al.2 In which the symptoms actually
predated herniation. This latter report puts into stark relief notions such as incarceration
and tethering in symptom production, and, instead, suggests
that the primary pathological entity of ISCH actually resides (at least in some cases)
within the cord itself.3 Such an abnormality might permit cord/dural contact anteriorly
as a precursor to the development of an anterior dural def-

Finally, although they point out that ISCH continues to be
associated with dorsal arachnoid cysts, it is worth noting
that, notwithstanding the cases reported by Barrenechea et al.,
significant improvement may still be procured with cyst
removal alone prior to definitive ISCH reduction.3,4

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Neurosurgical forum

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Spinal Stenosis

In the article describing their experience treating a consecutive series of 374 patients with lumbar spinal stenosis
by unilateral laminectomy for bilateral microdecompression, Costa and colleagues have raised several interesting

Object. Surgical decompression is the recommended treatment in patients with moderate to severe degenerative lumbar spinal stenosis (DLSS) in whom symptoms do not respond to conservative therapy. Multilevel disease, poor patient health, and advanced age are generally considered predictors of a poor outcome after surgery, essentially because of a surgical technique that has always been considered invasive and prone to causing postoperative instability. The authors present a minimally invasive surgical technique performed using a unilateral approach for lumbar decompression.

Methods. A retrospective study was conducted of data obtained in a consecutive series of 473 patients treated with unilateral microdecompression for DLSS over a 5-year period (2000–2004). Clinical outcome was measured using the Prolo Economic and Functional Scale and the visual analog scale (VAS). Radiological follow-up included dynamic x-ray films of the lumbar spine and, in some cases, computed tomography scans.

Results. Follow-up was completed in 374 (79.1%) of 473 patients—183 men and 191 women. A total of 520 levels were decompressed: 285 patients (76.2%) presented with single-level stenosis, 86 (22.9%) with two-level stenosis, and three (0.9%) with three-level stenosis.

Three hundred twenty-nine patients (87.9%) experienced a clinical benefit, which was defined as neurological improvement in VAS and Prolo Scale scores. Only three patients (0.8%) reported suffering segmental instability at a treated level, but none required surgical stabilization, and all were successfully treated conservatively.

Conclusions. Evaluation of the results indicates that unilateral microdecompression of the lumbar spine offers a significant improvement for patients with DLSS, with a lower rate of complications.

The most striking observation for this reader to take away from this paper is that, of the 5 patients chosen to illustrate their paper by pre- and postoperative CT scans, not one had an adequate foraminotomy. The foramina remain essentially unchanged. One would assume, the authors have selected the best radiographs to illustrate their paper. Degenerative spinal stenosis is not simply canal stenosis but is usually associated with multiple degenerative changes. Hypertrophy of the inferior articular process protrudes into the spinal canal, and hypertrophy of the superior articular process protrudes into both the spinal canal and into the nerve root foramen dorsal to the nerve root. Spondylytic ridging further narrows the foramen ventrally and thickened ligamentum flavum extending into the foramen contributes to both canal and foraminal stenosis. Most of the patients with failed minimally invasive surgery for spinal stenosis that I have seen for second opinion have had inadequate foraminotomies. The authors have evaluated the completeness of foraminotomy by observation through the microscope, which is notoriously inadequate, especially when evaluating the contralateral foramen viewed head on. What lurks laterally is unknown. The only proven method of evaluating a foraminotomy at the operating table is by the tactile and proprioceptive feedback from a ball probe, preferably angled, passed through the foramen.

The authors have done an excellent job of decompressing the spinal canal but have only partially completed the operation, which leaves the reader questioning the meaning and usefulness of their surgical results. The other confusing aspect of their paper is how to factor in the unusually high percentage (76%) of patients with single-level disease. To make meaningful conclusions about the effectiveness of their technique one would have to break down the series into single-level and multilevel disease, which presents 2 different surgical strategies, and compare with already published similar groups.

My second criticism is that they have entitled their paper “Degenerative spinal stenosis,” yet they present an extensive discussion of congenital stenosis. They have attempted to distinguish between 2 congenital axial configurations seen on CT scans, horizontal and vertical, which present technical challenges in performing their operation, a valuable observation. However, the CT scans illustrating these 2 configurations are not convincing. The preoperative images appear to be caused chiefly by huge facet joints protruding medially into the canal. It is unclear why a discussion of congenital stenosis is included in this paper. Their large series would have been more useful if a separate discussion of congenital stenosis, confirmed by congenital changes at adjacent levels, had been presented along with the customary population characteristics and results.

Finally, the take-away message from this paper is that all patients with lumbar stenosis should not be treated the same way. This is a heterogeneous group of patients requiring individual surgical approaches. We should not be slaves to the small incision, especially in the elderly patient with multilevel disease in whom an open operation preserving dorsal elements, removing all ligamentum flavum, and addressing the foramina, is faster and more definitive. Their operation may have a role in treating single-level stenosis primarily caused by hypertrophy of the ligamentum flavum.

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References

Response: We thank Dr. Beatty for his observations about our article. Some of his statements are fairly offensive; nevertheless we will try to answer point by point.

The foramina, as clearly demonstrated by the images, were completely decompressed on both sides. In all surgical procedures, the root is always decompressed ipsilaterally and displaced by a root retractor, and the disc underneath is examined. Contralaterally the decompression is considered complete only when the spinal root is completely demonstrated during surgery under microscopic visualization. We have occasionally removed herniated lumbar discs on the opposite side through this unilateral approach.

Dr. Beatty stated, “The authors have evaluated the completeness of foraminotomy by observation through the