

Correspondence

Correlation between disability and MRI findings in lumbar spinal stenosis

Sir—The work of Sigmundsson and colleagues (2011) represents a very good analysis of a cohort of persons subjected to surgery for spinal stenosis. However, the discussion of the disease itself far over-reaches any conclusion that can be drawn from this highly selected population that contains no control group. The Michigan Spinal Stenosis Study, a controlled study including asymptomatic persons and persons with back pain, addresses the same issues with contradictory findings. We found that, of 14 different measures of the lumbar vertebrae, including the thecal sac area and 'smallest two area' measures similar to those used in their study, only two measures (spinal canal diameter and smallest two spinal canal diameters) had any relationship to the clinical diagnosis of stenosis, and these two had no discriminant value (Haig et al. 2007). Furthermore, these measures have no relationship to level of disability including three standardized scales, a 15 minute laboratory walking test, or pedometer-measured community walking at inception (Geisser et al. 2007). Spinal measures did not predict function or pain at follow-up more than 18 months later (Haig 2006). The study included primarily people who were not referred for surgery. However, other large cohorts that do include asymptomatic persons fail to show any important relationship between the imaging and clinical presentation of stenosis (Haig and Tomkins 2010). We must disagree with the authors' opening statement that "MRI is the modality of choice when diagnosing spinal stenosis". Evidence to date tells us that the test only rules out dangerous diseases and assists with surgical planning.

The decision of a surgeon to operate and of a patient to accept an operation is a complex one that goes beyond objectivity on the part of both parties (Lurie et al. 2008, Deyo 2009). Any conclusion about the disease itself—including a belief that smaller canals relate to worse symptoms—but also pathophysiological observations that women have more multi-level findings, the percentage of persons with spondylolisthesis, etc.—must be viewed in light of the surgeon and patient's own belief systems and biases.

Clinical spinal stenosis remains a complex and poorly defined, and poorly validated syndrome. There may well be some relationship between spinal measures and disability. Despite the well intentioned work of Sigmundsson et al. modern research is showing that a surgical convenience sample does little to clarify the syndrome.

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Sir—We appreciate Dr Haig's interest in our study (Sigmundsson et al. 2011) and also recognize the important works having been performed by him and his collaborators (Haig et al. 2007, Haig and Tomkins 2010).

It goes without saying that a decision to operate, which should be a contract between the surgeon and the patient, is biased and certainly influenced by beliefs and previous experiences.

However, the clinical picture of spinal stenosis with pseudo-claudication symptoms, i.e. radiating symptoms that prevent walking for long distances and is relieved by flexion of the spine, is fairly distinct, and this is the type of patients being selected for surgery, where MRI is used for confirmation of the diagnosis.

We agree that a general correlation between measurement of dural sac area and clinical symptoms is most unreliable. Asymptomatic spinal stenosis is not unfrequent but this fact is also true for example lumbar disc herniation, hip osteoarthritis, meniscal injury and so forth. What our study mainly discusses is whether positive and negative prognostic factors can be identified in patients with clinical symptoms enough to justify surgery. In this group there is a relationship between spinal measures and disability in some aspects as presented in our study, and these facts can be used in the clinical situation when surgery is discussed with a symptomatic patient. This would be of clinical significance as satisfaction with surgery is only obtained in 65–80 % of patients. We draw no further conclusions from our study, which, however, in our opinion is another small piece in the puzzle of "complex and poorly defined and poorly validated syndrome" called lumbar spinal stenosis.

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