Reviewer's report

Title: Correlation of lateral stenosis in MRI with symptoms, walking capacity and EMG findings in patients with surgically confirmed lateral lumbar spinal canal stenosis

Version: 1
Date: 24 May 2014
Reviewer: Andrew J Haig

Reviewer's report:

Reviewer comments:

Correlation of lateral stenosis in MRI with symptoms, walking capacity and EMG findings in patients with surgically confirmed lateral lumbar spinal canal stenosis.

General comments: The relationships between various clinical, radiological, anatomical and statistical concepts of ‘lumbar spinal stenosis’ are increasingly confused. So an article that takes a multifactorial look at the problem is of substantial value. This is especially true for lateral stenosis, since radiological measures of the lateral regions have made research on ‘stenosis’ of that area difficult.

This article is quite clearly written.

The word ‘stenosis’ is used in confusing and incorrectly interchangeable ways throughout the article. For example, “Lateral lumbar spinal canal stenosis (LLSCS) is a related condition…” meaning related to the NASS definition of clinical stenosis which requires anatomical findings AND clinical consequences. However a few sentences further on, “The purpose of the current study was to evaluate the clinical significance of LLSCS found by MRI…” In this context LLSCS is only an imaging concept. Therefore, in each instance throughout the article define whether the word, ‘stenosis’ is clinical stenosis (the NASS guideline definition) or anatomical stenosis (some deviation from normal measurements of spinal architecture, regardless of symptoms) It would be nice if the authors actually defined the ‘deviation’ that allows them to use the word ‘stenosis’ for radiological findings).

12 authors is an unusual number for a study such as this. The academic contributions of each should be documented.

Abstract:

Abstract Methods: This is an uncontrolled study. State that in the first line of the ‘methods’ section of the introduction.

The inclusion criteria of, ‘LSS severe enough to indicate operative treatment’ is not a valid criteria (unless surgery was offered prior to review of MRI, which is not likely). The literature suggests that a surgeon’s impression of the need for
surgery is biased by imaging findings. So, state this more carefully. Perhaps you really mean, “LSS diagnosed by a treating surgeon who reviewed clinical and imaging findings. Potential subjects who were not offered surgery were not included.” Also, did the study include everyone whom a surgeon thought was surgical? Or only those who accepted (or eventually got) an operation?

The number of qualifying subjects eventually was 14. Not a big number when one considers the millions of operations performed for spinal stenosis. However it’s reasonable in relationship to any prior literature that addresses imaging vs. clinical stenosis, and no one that I know of has looked in detail at lateral recess on MRI vs. EMG.

Abstract results: “LLSCS was confirmed in all patients during surgery.” The authors should not present a surgeon’s opinion of his or her operative findings as any evidence of pathology or disease. I am unaware of any research whatsoever that validates the surgical ‘findings’ (whether an opinion, some pathological aspect such as edema, or some actual measurement) as diagnostic of clinical stenosis. I don’t think anyone has even codified these or looked at inter-rater reliability. Furthermore, it would be uncommon for a surgeon to operate and actually admit that his or her findings were not related to the reason for surgery.

Abstract conclusions: A substantial prior literature on positive imaging for radiological stenosis in asymptomatic older people means that an uncontrolled study cannot claim that ‘lateral stenosis seen by MRI is a clinically significant finding.’ We don’t know from this study the sensitivity or specificity of this finding in comparison to people who do not hurt at all. A more correct statement would be, ‘Among persons previously selected for surgery, lateral stenosis seen on MRI correlates with EMG, and thus may be a clinically significant finding.’

Define the abbreviation ‘LLSCS’ in the abstract.

Main paper: The above comments require changes in the main paper, as well. I will not belabor them.

Introduction: Concise and well written

Methods:

Reword the inclusion criteria. Address the problem of defining stenosis and severity. Currently it is based on circuitous logic. Imaging is used as an informal inclusion criteria (the surgeon who decided surgery was needed likely saw the images) and yet it is used as an outcome measure.

Provide more detail about how the cumulative MRI measures were viewed as ‘normal’ or ‘abnormal’. What was the norm to which the MRI’s were compared? What statistical deviation resulted in an ‘abnormal’ finding? The problem of 4 measures x 10 roots per patient requires statistical correction if some normative cutoff (e.g. p<.05 for ONE measure) is used to define abnormal. How is this addressed or not addressed? If statistical norms were not used and justified with a Bonferonni or other correction for multiple measures, explicitly state this as a
problem and discuss the ramifications.

Was the EMG examination masked?

It sounds like the Paraspinal Mapping codified EMG protocol (e.g. reference 4) was used. However the authors do not state this explicitly. If they did use paraspinal mapping they should state this, report findings based on paraspinal mapping scores, and define abnormal based on published statistical norms for paraspinal mapping. If they did not use paraspinal mapping or some other validated, codified technique with established norms, then the authors need to describe this as a methodology weakness. It’s important because asymptomatic older people do have needle EMG fibrillations. Thus the amount of fibrillation has to fall outside of some range of normal. As Elaine Date has shown, simply assuming that any fibrillation is abnormal leads to huge problems with false positives.

As far as we can tell, S1 does not innervate the paraspinal muscles. Reword wherever this assumption is written, e.g. “bilateral paraspinal roots (L2-S1)” might be reworded as ‘Bilateral paraspinal MUSCLES innervated by the L2-L5 posterior primary rami’ if you used paraspinal mapping or a similar technique.

Provide more detail about how the EMG cumulatively was judged to be abnormal. Was an EMG abnormal if ‘any’ fibrillation was found in the limbs or paraspinals? Or paraspinals only? Or limb muscles only? Or F wave (not found useful in previous studies) or H-wave? Or some combination?

And what defined involvement of a specific root? Paraspinal muscle localization? The classic ‘two muscles from two nerves but the same root, plus the paraspinals’? etc.

Please clarify, ‘the analysis was done online.’ I’m left wondering who physically did the test, who interpreted the findings of fibrillations etc. and who aggregated the findings to create an impression. Then I wonder what part of that was done from a distance, e.g. ‘on line’.

Results:

Unless the authors have a valid standard for confirming stenosis at surgery, strike the first sentence.

Explain the offer of surgery to 2 or 3 people with minimal pain and minimal disability. I suspect it relates to a surgeon’s bias after reviewing an MRI, but there could be other explanations.

The F- and H-wave results are presented, however the needle EMG data is not. A paragraph and probably a table are needed to present the data in a way that helps us understand how to distill the myriad individual tests into the ‘normal’ vs. ‘abnormal’ data that is used for analysis.

Discussion:
Discuss methodology flaws, as noted above.

Correct this sentence: “[the] Incidence of lumbar spinal stenosis is increasing probably due to the better quality and availability of radiological imaging facilities and also due to the aging population” I understand that the authors mean that lumbar stenosis is detected more frequently these days. However the incidence (actual presence of disease) is not influenced by the availability of radiological imaging. Also I have no idea how the quality of imaging would increase either the incidence of disease or the detection of previously undetected disease. Certainly ageing does affect incidence. Please rewrite more carefully.

Remove this statement: “MR images are thus needed to establish the level and severity of a stenosis.” It is contradicted two sentences later, “The degree of the severity of the disease cannot be judged based only on MR images either.” The author’s own data, consistent with the rest of the controlled trials in the literature, shows that MRI is not a good measure of severity of the syndrome. If you do need to say something about the utility of MRI, first caution that it is not diagnostic, then state that MRI may be useful in surgical planning and elimination of unusual etiologies of the stenosis syndrome?

Again, although small, and full of many assumptions and confusions, this study data provides an important first insight into the anatomy and physiology of the clinical syndrome of spinal stenosis as presenting in lateral spaces. With careful thought to definitions and wording, and some additional information, I believe it can be a useful contribution to the literature.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

The University of Michigan was funded by the NIH for my research trial, the Michigan Spinal Stenosis Study, which concluded about 4 years ago. No other competing interests.