Reviewer’s report

Title: Depression as a 2-year outcome predictor of lumbar spinal stenosis surgery: a prospective study of preoperative, 3-month, 6-month and 12-month follow-up phases

Version: 1 Date: 31 January 2010

Reviewer: Gustavo Zanoli

Reviewer’s report:

REVIEWER dr. Gustavo Zanoli

Overall comments:

The authors should be congratulated: this is an interesting study that falls within the scope of a musculoskeletal journal. It is a prosecution of a long-lasting and fruitful study so not entirely new, but the methods are well-established and sound. Little work is needed before it can be considered, in my opinion, useful and acceptable for publication, even though the degree of interest for the readers should be assessed by the editorial team.

1. Is the question posed by the authors new and well defined?

The question is well defined but not new, however authors justify the reason to publish this study. Maybe this can be made more explicit for readers.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work?

In general, Yes. See also comments.

3. Are the data sound and well controlled?

Yes

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?

Yes

5. Are the discussion and conclusions well balanced and adequately supported by the data?

Not always in my opinion (see suggestions)

6. Do the title and abstract accurately convey what has been found?

I think they can be made more informative especially differentiating what is new in this paper (even knowing the other ones, it took me some time to grasp).
7. Is the writing acceptable?
Yes

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Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

Methods

1) VAS: It is a common rule that patients should fill in outcome instruments alone, without external interference. I wonder why of all VAS measurements, the most biased one was chosen.

2) Cut-off for depression: I am not familiar with the BDI instrument, it seems to me that if 10/63 is abnormal, it might have a significant ceiling (or floor) effect. Do the reference you quote justify your choice? I believe you should expand on this point for the more general readers.

3) Study sample: I think it would be useful to know how sample size was calculated, if it was related to the present aim of the study or if it was taking into account other expected outputs of the study, published elsewhere or not.

4) Surgical outcome. I would have rather used absolute measures of success rather than relative ones. If I understand correctly, median split means that there is always 50% belonging to the poorer scoring half. This might not reflect clinical outcome, as usually LSS patients do well in more than 50% of the cases, at least in the first 2 years. Why not simply using absolute score, of improvement above a predefined MCID? It would be interesting to know how result would look like, this way.

5) Predictors: Did you explore other possible predictors? For instance, surgical severity or peri-intervention adverse events, or surgical achievement (fusion, decompression) as judged by the surgeons themselves? If not, please comment, or at least address in discussion.

Results

6) Expand: This section is really succinct, maybe it could be usefully expanded, for example adding more data or exploring alternatives such those suggested in point 1) and 3).

Discussion

7) Personally, I am not completely convinced by your interpretation of the data, at least the ones you present here. How can you say what comes first, symptoms or depression? It is a common experience that chronic patients tend to be depressed, but many of them would improve after solving the organic somatic problem, as it happens in your sample 48% depressed preop vs 29% postop with no specific treatment for depression. It would be interesting to be able to differentiate among these 2 types of “depression”. Does BDI do that?

8) Did you consider alternative explanations, such as that those patients which were correctly operated did better and THEREFORE are not depressed anymore?
and the others which did not improve in their symptoms, also did not improve depression. In this case depression could simply accompany symptoms, being present pre- and postoperatively in a subset of persons who are prone to depression when they experience clinical problems...please make your point more clear if you want refute this idea.

9) I strongly disagree with the idea that your data “pinpoints the need for …appropriate treatment”, especially if you mean medications. I believe this statement is not supported by your findings and should be addressed with a specific RCT, in case.

10) As you point out, it is curious that only a minority of patients received medication compared to the high % of patients considered “depressed” with the BDI cut-off? Again, I am no expert (I am a surgeon), but why do you think this is necessarily a mistake? Couldn’t they receive other types of treatment? How does this compare with normal prescribing behaviour in Finland? Is there a commonly accepted guideline?

11) Page 11 line 16-17: “these result support the use of antidepressants”. I disagree. See pint 9. And I believe is outside the scope of this paper, I would simply delete it.

12) Apart from suggestion in favour of medicaments, I also see another danger in your paper: surgeons will use depression instruments to exclude patients form the operation, in order not to ruin their “statistics”, Did you consider that? Do you think it would be ethical to do so? I believe not. If depression is a clinical problem people should receive appropriate attention, but if they also have a real physical problem, they should get the right intervention without discrimination!

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Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Methods

1) WAI for co-morbidity. Modifications of validated instruments are to be avoided. Specifically, self reported number of illnesses could significantly underestimate co-morbidities in my opinion. Could you motivate your choice?

2) Stucki: Your study, with all the data you have, would be a very good occasion to publish the Finnish validation with little additional effort. In my personal opinion this probably more urgent and useful than some of the information in your present study. I am sure you are planning to publish it, I just wanted to encourage you.

Discussion

3) Page 11 1st line: 10-15% is the percentage of patients who get better, or is it their individual improvement?

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Discretionary Revisions (which the author can choose to ignore)
Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

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

Declaration of competing interests:

Conflict of interest

I know personally one of the authors (OA), with whom I worked fruitfully in the past in the preparation of the European Guidelines for LBP

As a clinician I see patients with LSS and sometimes operate on them