Reviewer’s report

Title: Second opinion for degenerative spinal conditions: an option or a necessity? A prospective observational study

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Reviewer: Paul Willems

Reviewer's report:

First, I would like to compliment the authors upon their study which must have been a tremendous work in terms of logistics to have so many patients evaluated for a second opinion.

The authors have looked at differences between first and second opinion in patients who were scheduled for spine surgery by spine surgeons from private practices in Brasil and then sent by their insurance company for second opinion to a general physiatrist and orthopedic surgeon or a specialized spine board. A large discordance in diagnosis and indications for surgery was found and it was concluded that by means of a second opinion unnecessary surgeries can be avoided.

I feel that the finding of discordance between physicians and even spine surgeons (specialized board of spine surgeons) is quite important for clinical practice. It shows that patients seeking for medical care for their spinal problem may get totally different treatment advices, which can be quite confusing. Moreover, it may show that spine surgeons in private practices choose for surgery more easily than surgeons in a not-for-profit hospital do. These are, of course, alarming findings in an era of Evidence based Medicine.

I wonder what the authors would suggest now knowing these findings: Should every patient with a degenerative spinal condition indicated for surgery have a second opinion from another spine surgeon, or should these patients be evaluated and treated in a multidisciplinary matter and should thus the indication for surgery be made by this team of different physicians? Could you please specify this more clearly in your discussion?

On p.3, r.18-19 the authors state that a second opinion will reduce inappropriate surgery which is defined as elective surgery without prior appropriate (what is appropriate?) conservative care. This is of course true, but shouldn't surgeons always try conservative measures if possible first, before the indication for surgery is made?

p.4, r.16-18: the secondary aim of the study" to compare functional and quality of life endpoints in a subset of patients who were subsequently treated at HIAE with either surgery or conservative care" does not add value to this paper. First, it distracts from the main message
(large differences in first and second opinions) and second, as only a subset of patients was treated and followed-up in their centre and groups were not selected randomly, comparison between conservatively treated and operated patients is comparing between 'apples and pears' and thus, not reliable. I would strongly advice to skip the secondary aim from this study.

r.22-23: What was the interval between the first and the second opinion? Especially in the diagnosis of cervical or lumbar radiculopathy this may cause differences in diagnosis because of natural course. Please specify.

I wonder, how was the second opinion offered to the patients? Were they free to decide whether they would have the 2nd opinion or not or was it more or less compulsory, e.g., that they would not get reimbursement for their treatment from the insurer unless they had the second opinion?

p.5, r.4-6: The authors made a large exclusion of diagnoses for spinal surgery ("spinal fractures, major scoliosis, congenital spinal deformity, spondyloarthopathies, spinal tumours or infection were excluded"). Thus, only patients with degenerative spinal disorders were included. This is an important limitation as there is considerably less consensus on spine surgery for degenerative conditions as there is for nondegenerative conditions as listed above. Therefore, I would suggest to change the title into: "Second opinion on surgery for degenerative spinal conditions: an option or a necessity? A prospective observational study".

p.6, r.20-21: For second opinion, patients were evaluated by a physiatrist and a general orthopedic surgeon who does not perform spine surgery. This is a rather unfortunate limitation, as in current highly specialized medicine the diagnosis/indication for surgery should be made by those clinicians most specialized in this field. It seems quite logical that the physiatrist and orthopedic surgeons saw less indications for surgery. If a general practitioner had evaluated the patients, probably even more discordance would have occurred. It would have been better if all patients had been evaluated by the Spine Review Board with highly specialized spine surgeons.

p.9, r.18-20: as mentioned before, what's the use of evaluating treatment outcome in a subset of patients for this paper? Additionally, cross-over patients from failed conservative treatment who had surgery later were analysed as conservatively treated. This sort of "Intention to treat" analysis is misplaced here as there was no randomization of patients for treatment. Once again, the secondary aim only distracts from the main message.

p.17, r.11: "Myofascial pain syndrome" and "mechanical low back pain" are not a diagnosis, but merely descriptions, this difference in nomenclature may also be a cause for the discordance of classification found in this study.
The fact that conservatively treated patients and operated patients had comparable outcomes does not prove that a second opinion reduces unwarranted spine surgery, as patients were specifically selected. Only if those patients selected for conservative treatment had been randomized for conservative or operative treatment and if then patients in both groups had similar outcomes or even better for conservatively treated patients, such a statement could be made.

The purpose and design of this study was not to look at effectiveness of spinal surgery versus conservative treatment, but to look at discordance of opinion between physicians. As mentioned earlier, the inclusion of outcomes of patients only distracts from the central message of this paper and it would be best to omit the secondary aim.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

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