Author’s response to reviews

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

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Response letter
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Second opinion on spine surgery: an option or a necessity? A prospective observational study

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Authors’ response:

We thank the reviewers for their helpful comments and suggestions. We think that the suggested changes and clarifications have improved the manuscript and its clarity. Reviewer 1 suggests removing part of our paper regarding patient outcomes but Reviewer 2 and 3 did not. We would prefer to retain it for what we believe are important reasons provided in our response to
reviewers and await the Editor's decision about this. Responses and how we have addressed each one is provided below.

Reviewer reports:

Reviewer 1: 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.

Authors’ response:

We thank Reviewer 1 for his/her positive comments. The suggested changes have further improved our manuscript.

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?
Authors’ response:

We have revised the last two paragraphs of the discussion to more clearly specify that we suggest that every patient with a degenerative spinal condition offered surgery should seek a second opinion. While another spine surgeon could provide this, we think that multidisciplinary assessment and management could optimise quality improvement, reduce costs through high volume efficiencies and create market differentiation with high patient satisfaction.

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?

Authors’ response:

We agree that this is a given and have therefore removed the definition of inappropriate surgery from this sentence.

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.

Authors’ response:

We did consider this approach. However while we agree that there are limitations to the design of the secondary aim of the study and hence interpretation of its results that we have fully acknowledged in the discussion we feel compelled to fully publish our findings to avoid publication bias and add to the currently available evidence on this topic. At the very least our data suggests that obtaining a second opinion may reduce unwarranted spine surgery without resulting in poorer outcomes (i.e. without causing harm). Further, ethical approval of this study requires us to publish our results in full and we owe it to the study participants to do so.
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.

Authors’ response:

We agree that one possible explanation for differences between opinions may be that the clinical status had changed over time and we have added this to the discussion (see page 20, lines 16 to 19). Unfortunately we do not have the precise time intervals but we have clarified in the methods that the second opinion is required within 21 working days of receipt of the request (see page 5, line 17).

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?

Authors’ response:

Yes they are free to decide. Both patients and health insurance companies have the right to request a second opinion. However it is not mandatory that patients comply with a health insurer request for a second opinion (or the recommendations of the second opinion). They would be reimbursed for their treatment from the insurer irrespective of whether or not they complied with the second opinion request. We have clarified this in the text – see page 5, lines 5-10.

p.5, r.4-6: The authors made a large exclusion of diagnoses for spinal surgery ("spinal fractures, major scoliosis, congenital spinal deformity, spondyloarthropathies, 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".
Authors’ response:

We agree and changed the title as suggested.

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.

Authors’ response:

We agree that it would have been better for all patients to have been evaluated by the Spine Review Board and have already included this shortcoming as a limitation in the discussion, i.e.: “Finally only participants deemed to require surgery or for whom consensus could not be reached were reviewed by the spine review board and it is therefore possible, but in our view unlikely, that some patients recommended CM by the physiatrist and orthopaedic surgeon could have been recommended surgery by the spine board” (see page 21, lines 6 to 10).

On the other hand, we consider that non-operating clinicians, such as the physiatrist and general orthopaedic surgeon in our study, can also be highly competent in evidence-based management of back and neck pain including indications for surgery. As outlined in our discussion, studies have demonstrated that consultation with a non-spine surgeon for patients offered elective spine surgery decreases the rate of spine surgery and patients are mostly satisfied with the results of their treatment.

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.
Authors’ response:

As above, we agree that there are limitations to this component of the study but we prefer to retain it in the paper. We think that our finding of similar outcomes from surgery versus conservative treatment (including patients who later crossed over to surgery) suggests that at the very least, obtaining a second opinion does not cause harm. Our results are in keeping with expected outcomes based upon randomised controlled trials and other observational studies.

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.

Authors’ response:

We agree that the wide variety of diagnostic labels used may have been a cause of discordance of classification. However, as outlined in our methods (pages 9 lines 20 to 24 and 10, lines 1 to 9) we reduced the number of diagnostic categories to group like diagnoses together. If we had not done this, the discordance would have been even greater.

p.18, r.4-9: 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.
Authors’ response:

We agree that comparable outcomes in the subset of patients who were treated at HIAE with either surgery or conservative treatment does not prove, by itself, that a second opinion reduces unwarranted spine surgery. However it provides some data to support the contention that at least for the cohort of patients treated at HIAE it did not cause harm, and it is consistent with other studies that have reported similar results. We have clarified this further in the discussion (see response to Reviewer 2 below).

We also agree that determining the effectiveness of spinal surgery versus conservative treatment was not the purpose or design of this study.

Reviewer 2: This is an interesting study that investigated the role of second opinions in spine diagnoses and treatment recommendations. The rate of discordance found between opinions is certainly concerning and suggests that obtaining a second opinion (or third) could potentially reduce the rate of unnecessary surgeries. The authors acknowledge the limitations of their study, primarily that: 1) they did not have data to compare the concordance in diagnosis between the physiatrist/surgeon compared to final review board, 2) they didn’t compare concordance between the physiatrist and surgeon, and 3) lack of follow up data from the patients that were treated elsewhere. However, I think the paper provides interesting findings and is suitable for publication. My only recommended change is that the authors temper their conclusion on page 18 that the “HIAE triage process was effective in correctly assigning patients to the most appropriate care.” It is possible that some of the patients treated with surgery would have also improved with CM and vice versa. Further, the outcomes of patients treated elsewhere is not available for comparison. Similarly temper the conclusion on page 21.

Authors’ response:

We thank Reviewer 2 for the positive feedback.

We have tempered the discussion regarding the effectiveness of the HIAE triage process as recommended (see page 22, lines 1 to 7) and also tempered the conclusion (see page 22, lines 12 to 16).
Reviewer 3: The authors present a manuscript examining the disparity of treatment recommendation for spinal surgery in Brazil. Their methods are well described, and the results speak for themselves. They observed a significant disparity in surgical recommendation between 'community spine surgeons' and surgeons at their hospital. Only in a minority of cases was there a consensus on surgical recommendation.

Authors’ response:

We thank the Reviewer 3 for his/her helpful feedback.

The manuscript is interesting and represents a contribution to the literature in that it highlights a wide disparity of surgical indication among spine surgeons. It would be interesting to see for what reasons there was disagreement with the initial surgical indication (insufficient non-operative treatment, disagreement of effectiveness of surgical treatment, etc.) If these data are available, it would greatly enhance the quality of the manuscript if these data were included.

Authors’ response:

We do not have this more detailed information to understand the reasons for the disparity but agree that it would be worthwhile to explore this topic further as an alternative way of reducing surgery that may be unwarranted.

I strongly recommend the authors refrain from the term "inappropriate surgery". This carries a judgmental connotation and as the authors themselves noted, it is challenging to label a recommendation as "right" or "wrong". I suggest that the authors use verbiage that is more neutral. Otherwise, the large disparity observed highlights the wide spectrum of surgical opinion in spine surgery (at least in Brazil). I recommend publication with minor revision.

Authors’ response:

We thank the reviewer for this insightful comment and agree that the judgmental connotation is inappropriate (no pun intended). We have therefore altered the wording throughout to ‘potentially unnecessary surgery’ or simply ‘surgery’ where no adjective is required.