Author’s response to reviews

Title: Diagnosis and management of lumbar spinal stenosis in primary care in France: a survey of general practitioners

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Author’s response to reviews:

Robin L. Cassady-Cain
BMC Musculoskeletal Disorders

Paris, August 14th, 2019

Dear Robin L. Cassady-Cain,

Please find enclosed a revised version of our manuscript BMSD-D-19-00201.R1 entitled “Diagnosis and management of lumbar spinal stenosis in primary care in France: a survey of general practitioners” by Marie-Ombeline Chagnas and colleagues, that we would like to submit to BMC Musculoskeletal Disorders for publication as a research article.

We are grateful for the opportunity to revise our manuscript. We would like to thank the reviewers for their valuable comments that helped us to substantially improve our manuscript. We have addressed all the comments and have included the reviewers’ suggestions in the revised version of our manuscript. Enclosed you will find a marked version of the manuscript with modifications indicated in blue and a point by point reply to the comments.

All the authors have read and approved the revised version of the manuscript. All authors have given necessary attention to ensure the integrity of the work. We hope that our work will be suitable for publication in BMC Musculoskeletal Disorders.

Best regards,

Associate Professor Christelle NGUYEN
Reviewer #2

This is a revised manuscript describing the results of a survey of GPs in France regarding their diagnosis and treatment of LSS. The authors have attempted to address some of the prior critiques but some concerns remain. The fundamental weakness is the low response rate though the authors address this issue and cannot improve it, the conclusions regarding the responses are necessarily limited. I have a number of remaining concerns which all seem to relate to a number of the authors opinions and apparent biases which seem to creep into the manuscript in part because the amount of substantive methodology and results are limited.

1/ In the introduction line 119 they state "high-level evidence from large clinical trials is lacking" with regards to LSS in general which is not strictly true, I am aware of at least 3 sizeable RCTs regarding surgery vs non-operative treatment in LSS, at least 1 large high-quality RCT of epidural steroids, numerous systematic reviews/meta-analyses, etc. which are the highest level evidence available. Thank you. We now removed this misleading statement (lines 118-119).

2/ Similar on lines 271-272 they conclude that effectiveness of epidurals cannot be concluded due to low quality evidence citing Lurie et al. but this is not the conclusion from that review and not strictly true as previously discussed; the actual conclusion from that review was "limited evidence of a lack of effectiveness [of epidural injections] in spinal stenosis". We now corrected this statement (lines 271-272).

3/ A major point of the manuscript seems to be a lack of recommendation for specific flexion-based exercise or endurance training. I have 2 concerns about this. The first is that nearly all (94%) state they would refer for physical therapy. It would seem a reasonable interpretation that if they were referring to PT, they would not pre-judge the PT regimen to be prescribed but leave that to the therapists. If the PTs do not know the evidence regarding the role of the flexion-based exercise and flexion postured endurance training, that seems to be a system deficit above and beyond GPs and primary care, and if they do, then the strategy of PT referral by the GP seems to be a reasonable one that addresses the issue. Thank you. We agree that this other interpretation of our findings is plausible. We now added it to the discussion (lines 286-290).

4/ Moreover, it is unclear from reviewing the survey how they draw the conclusion of the low rates of recommending doing flexion-based exercise since this was not an option for selection on the survey they seem to base this on the failure to specifically state this in terms of advice given but this is of such a different character than selecting from a list that it is unclear whether this is useful data. Prescribing cycling was actually an option for selection on the closed-ended question n°17 of the survey "Which non-pharmacological treatment do you prescribe?". The low rates of recommending doing lumbar flexion-based exercise were drawn from answers to that question. As the question addresses only cycling rather than lumbar flexion-based exercise in general, we now clarified this in the corresponding results (line 212) and discussion (line 278) sections.
5/ The survey is also confusing in terms of non-operative treatments as there are rows for both Physiotherapy and Physical Therapy and it is unclear if there is a distinction, whether this is a typo in the survey or in the manuscript and if the former whether the two were combined for reporting. (In Table 4 it appears to be listed as Physical activity but in the survey in the appendix it reads Physical Therapy). Thank you. “Physical therapy” was a typo. We meant “physiotherapy” on the one hand and “physical activity” on the other hand. We now corrected Appendix 3 (question n°17, page 7).

6/ The authors say they got rid of the term "radicular claudication" but it remains in Figure 2. Thank you. We now amended Figure 2.

7/ Also the data on referral seems very confusing. There are numerous different reasons for referral and different specialist but unclear if there is any relationship between the two. In other words for those who would refer "on evocation of the diagnosis" are the specialist similar to those who would only refer "in case of therapeutic failure" given the evidence in the literature, referral to a surgeon in the latter case is much more reasonable than in the former for example. Thank you, we agree with the reviewer comment. We did not specifically assess the potential relationship between reasons for referral and different specialists. Because the selection of answers to questions n°21 “When do you refer the patient to a specialist?” and n°22 “To which specialist?” could be multiple, we will not be able to clarify if there is any relationship between different reasons for referral and different specialists based on our dataset. We now mentionned this limitation in the discussion (lines 297-299).

8/ Table 4 seems to be a compilation of free text answers based on the survey format, were all the grouped answers really identical (i.e. "to strengthen the muscles") or were they grouped thematically; if they were grouped was this done formally using qualitative research methods or just based on the opinion of one or more authors? Figure 4 summarizes free text answers to the open-ended question n°16 “What are the 3 main objectives of non-pharmacological treatments in people with LSS?”. After review of the free text by the first and last authors, answers were grouped when they were identical based on their opinion. This was now clarified in the figure legend (lines 425-427).

Reviewer #4

9/ The authors have adapted the manuscript accordingly to my comments and questions. Thank you.