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:

Dear Darren Byrne,

Please find enclosed a revised version of our manuscript BMSD-D-19-00201 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.

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 #1
This manuscript titled "Diagnosis and management of lumbar spinal stenosis in primary care in France: a survey of general practitioners" has some worth, but is not suitable for publication in BMC Musculoskeletal Disorders. For my opinion, this manuscript is much better for the field of General practitioner.

1/ As authors mentioned, only 90/330 (27.3%) GPs were available for analysis. This study was just a cross-sectional study. Participation rate was too low. In France, there are 88137 GPs, as authors described in line 110.

We agree that the rate of participation is low in our study and we now clearly mentioned this limitation in the discussion (line 309). Conducting a prospective survey on the subject would be interesting, especially after educational interventions, and could be one of the perspectives of our paper.

There are indeed 88137 GPs in France. We chose to select a representative sample of all general practitioners, randomly drawn from the list of French Medical Board. The method of the random draw has been described (line 133).

2/ LINE 98-100: There are currently no national or international guidelines for the diagnosis and the treatment of people with LSS. North American Spine Society has already published guideline for LSS. And Japanese Orthopaedic Association also has a clinical guideline for LSS but it is in Japanese.

We agree that there are guidelines and we now removed the sentence which can be confusing (line 98). We meant to mention the lack of guidelines specific for primary care for the diagnosis and treatment of spinal stenosis (line 111). We now added the precision “in primary care” (lines 226 and line 332). As highlighted by our article, the GPs refer their patient in priority to a surgeon for therapeutic management. GPs are the front line of symptomatic complaints of the patients. We hypothesize they may need criteria adapted to their exercise and their consulting time.

The N-CLASS criteria of North American Spine Society have been cited in our study (line 234). We now also mentioned the SSHQ and LSS-DST criteria of the Japanese Orthopaedic association (lines 245-246).

Reviewer #2

3/ This manuscript reports a descriptive study of knowledge and attitudes of GPs in France regarding lumbar spinal stenosis. The topic is interested and the main finding of limited confidence and limited knowledge is expected. Fundamental weakness are the purely descriptive nature of the report, the fairly small sample size, and the low survey response rate which, while similar to other survey type studies, limits the conclusions that can be drawn due to a high risk of response bias. Several aspects of the manuscript need revision.

We agree that our study have limitations. We thank you for your comments that we have now taken into account in the discussion (line 310).

4/ The use of the word "radicular claudication" is very confusing and not the standard terminology. In English the leg pain provoked by walking and relieved by rest/lumbar flexion is typically referred to as either pseudoclaudication (in the older literature) or preferrably...
"neurogenic claudication". The term radicular is not appropriate here as the leg pain associated with LSS is not radicular (in a specific nerve root distribution) in character but usually more generalized or diffuse (it can be radicular in nature when there is foraminal stenosis with nervroot impingment but this entity differs in many respects from LSS with neurogenic claudication due to central canal stenosis).

We agree that the term neurogenic claudication is clearer and less confusing than the one we used. We now replaced radicular claudication with neurogenic claudication throughout the manuscript (lines 71, 82, 97, 193, 280), and we replaced the term “radicular pain” by “leg pain” (line 105).

5/ There are a number of errors in the manuscript including a description in the methods that confidence was measured using a numeric rating scale while the survey in the appendix shows these to be Visual Analog Scales, and typographical errors such as the citation on line 313, missing % sign on line 220, etc.

We thank you for your comments. The scale used is indeed a Visual Analog Scales. We made changes throughout the manuscript (lines 69, 157, 193, 201 and 209). We also corrected the mentioned typographical errors (lines 214 and 306).

6/ The very low exposure to LSS in this sample (almost half of respondents seeing < 5 patients per year) seems to have important implications any recommendations but is not really addressed. We agree that the rate of LSS covered per year is very low. This may explain the results of the study. Our hypothesis would be that LSS may be under-diagnosed in primary care. We have discussed this hypothesis (lines 322 to 324).

7/ The discussion on page 12 lines 245 - 251 is difficult to understand. The authors seem to imply that the lack of congruence between published criteria for LSS and those cited by GPs implies that the criteria may not be applicable to primary care and that developing criteria among GPs may be more appropriate/relevant, however the GPs in the survey stated themselves that they were not confident in the diagnosis of LSS so such a Delphi process among GPs may well produce uninformed/invalid criteria rather that "primary care relevant" criteria. A more thorough consideration of the issues involved seems in order.

We agree that this part of discussion is confusing. We now removed the sentence referring to a Delphi study, which is not relevant in this situation. We added a sentence on the issue of developing clinical criteria adapted to the constraints of primary care (lines 242 to 244). We believe that clinical criteria should be more widely disseminated to GPs and implemented to primary care.

Reviewer #3

Dear Authors,

this is an interesting study of the knowledge of diagnosis and management of LSS among French GPs. Many of these patients are primarily seen by GPs and therefore to study their knowledge about this condition is of high interest. The results showed that many GPs in France are lacking proper knowledge of some important facts about LSS. This may delay proper diagnosis and appropriate treatment for these patients. Results from this study can be used to improve the continuous medical education of GPs regarding degenerative spinal conditions, which are a huge health care issue.
However, I have a few comments on this manuscript:
The introduction/background is well written and leads the reader to the topic of this study
The methods section describes the study design, the participants and recruitment, the questionnaire, statistics and ethics.
8/ the authors chose to include 330 French GPs out of 88137 registered in France, why only 330?
We did not aim interviewing as many GPs as possible, but a representative sample, selected by random draw. We obtained a list of 15 physicians per region, with the aim of contacting 10 per region. The French Medical Board kept the breakdown into 22 metropolitan regions. We got a list of 405 GPs corresponding to 0.5% of French GPs.

9/ the recruitment process could be described shorter and more clearly, it is a little bit confusing.
We agree that the description of the recruitment is too long. We now reduced and have specified the number of GPs selected by region (line 135).

10/ the questionnaire is shortly described but could be shown as a figure for example.
We thank you and. The details of the final questionnaire are in Appendix 3 (line 164).

11/ Statistical methods should be more described.
We thank you for your comment and we have completed the paragraph "statistical methods" (line 170).

12/ The results section describes the participants, diagnosis and treatment. Unfortunately, the response rate was very low (27%), in fact only 90 GPs answered representing only 1 promille of French GPs. The numbers are also described in a confusing way.
We agree that the rate of participation is low in our study and we now clearly mentioned this limitation in the discussion (line 309). Conducting a prospective survey on the subject would be interesting, especially after educational interventions, and could be one of the perspectives of our paper.
There are indeed 88137 GPs in France. We chose to select a representative sample of all general practitioners, randomly drawn from the list of French Medical Board. The method of the random draw has been described (line 133).
As requested, we now simplified the presentation of the results. We added in the "statistical analyses" section that the results were expressed in absolute frequency and relative frequency (line 170). Further, we now removed the word "mean" from "results" section which was an unnecessary clarification (line 188).

13/ Some of the results are presented in tables that could be simplified or shown as figures for better understanding.
We agree that tables 4 and 5 contain a lot of data. We now replaced Table 4 by Figure 3.

The discussion is well written, includes some of the study limitations and leads the reader to the conclusions.

Reviewer #4

General:
14/ For me it is not clear why GP's should be able to manage patients with LSS; I would agree
with the statement that they should take into consideration a lumbar spinal stenosis in patients
with symptoms compatible (described in the paper) with this illness. To my knowledge the
decision whether to recommend surgery or another treatment to such patients is not easy and
needs a lot of experience.
We agree that the LSS surgical decision should be made by spine experts. However, before
surgery, conservative treatment is offered first and GPs have a role. GPs also have a role in the
diagnosis, so that the first-line treatment and secondary referral to a spine expert would be timely
and appropriate.

15/ The discussion is much too long; (no mandatory changes in the number or words; I would
prefer a focus on 1. the triage function of GP's in these patients. 2. The efficacy of non-surgical
interventions including the 'lumbar belt' (excluding epidural steroid injections, which I presume
are not offered in GP's offices).
1. We agree that GPs have an important role in the diagnosis of LSS and need tools to easily
classify the symptoms of patients, to guide their care and to refer them to a spine expert in a
timely manner.
2. We agree that GPs should be able to offer non-surgical treatments to patients. In the
Cochrane review published in 2016 (“Surgical versus non-surgical treatment for lumbar
spinal stenosis”), there were no clear benefits with surgery versus non-surgical treatment.
Further, non-surgical LSS treatments had the advantage of having no side effects. These
findings suggest that clinicians should be very careful in informing patients about possible
treatment options, especially given that conservative treatment options have resulted in no
reported side effects. A recent clinical trial (Effect of a prototype lumbar spinal stenosis belt
versus a lumbar support on walking capacity in lumbar spinal stenosis: a randomized
controlled trial, C. Ammendolia et al. / The Spine Journal 19 (2019) 386-394), compared the
effectiveness of a lumbar belt and a prototype LSS belt (which reduces lumbar lordosis) on a
functional level. There was no superiority of one of the belts.

16/ Line 198; Diagnosing people with LSS; Low back pain, radicular claudication and
paresthesia in lower limbs were the 3 most frequently cited clinical signs leading to the diagnosis
of LSS. Later on …in line 246 … the authors write that the above mentioned clinical signs
differed from those included in the two published datasets --- and the reason for this difference is
or could be the fact, that the datasets were developed by spine specialists and only 1% or less of
the participants were GPs'. This argument doesn't make sense to me; please explain in the text
why the percentage of GP's (which seem not to be very confident in the diagnosis of LSS) in
setting up a list of diagnostic criteria for LSS should be of any relevance.
We agree that this part of discussion is confusing. We now removed the sentence referring to a
Delphi study, which is not relevant in this situation. We added a sentence on the issue of
developing clinical criteria adapted to the constraints of primary care (lines 242 to 244). We
believe that clinical criteria should be more widely disseminated to GPs and implemented to
primary care.

Minor
17/ Line 313 typos, spinal disorders (25), 25).
We thank you. We now corrected this typographical error.