Peer Review File

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Reviewer A

This article is looking at the re-do surgeries for patients with DCM whom presented with recurring symptoms. Overall, it does present interesting findings, however the generally very low numbers and the innate biases of the type of study chosen can be very difficult to generalise findings. It is admirable to see DCM in the spotlight as this is not well researched topic. The results presented are confusing in terms of the metrics you have used, and some of your conclusion statements are contra dictionary. Moreover, I suggest listing a specific paragraph dedicated to the limitations of the study you have chosen. Here are a couple of comments below:

1. You continuously use the term revision surgery, for me personally this means surgery performed on the same level as before because of various reasons. I do think there is a better placed term to explain the re-do surgeries for patient already treated for DCM.

This indeed does bring about confusion. Within the introduction we define 'revision surgery' as the "the necessity for a second decompression procedure in patients who experienced neurological improvement post-op, with the etiology due to restenosis not limited to the previously operated level/s but also at adjacent and skip levels" (Lines 71 – 74).

2. Your cohort of patients is very small, and you have restricted this quite significantly in terms of inclusion. It seems like you have excluded all patients needing re-do surgery for recurrent symptoms which happened within the first 2 years of the primary surgery. What is the reasoning behind this? Would this not introduce bias? Would you consider including these patients as well?

Early revision surgery was due to causes for neurological deterioration / non-improvement such as inadequate decompression, hematoma formation, and iatrogenic instability. This would not address our question of interest as defined in the introduction (see also response to previous question), and the rationale for this exclusion is further highlighted in the Methodology (Line 99 – 101).

3. Methodology section needs clarification, this was a retrospective study, which is not specifically mentioned. Why have you chosen 2009 and 2020 in terms of years?

This has been described in the Methodology under 'Patient selection criteria' in the statement "This period was selected to ensure patients had received a minimum of 2-year of post-operative follow-up, and to facilitate electronic retrieval of cases in accordance with diagnosis / surgical procedure since earlier records were not digitized" (Line 93 -96).

4. You have used JOA score mainly, which a good score and correlates well with mJOA, however did you consider using mJOA as your primary score?

This was a mistake from the authors, and after clarifying with the Occupational Therapist, mJOA has been used within the duration of this study, and not JOA. This has been amended accordingly throughout the text.

5. It seems that you have excluded patients with cervical disc and OPLL compression suggesting they are not due to degeneration? Is this correct, if so this introduces significant bias, in your patient selection, as I feel they could be degeneration related as well, as post-operatively. We see often hyperostosis at the sites of previous ACDFs which can cause compression and recurrent symptoms, does that mean that you exclude these patients? Also, there is no exclusion of traumatic causes of the recurrence, e.g central cord syndrome, has these patients been included in your cohort? Please clarify these points.

Our methodology states that we included causes of cervical myelopathy due to degeneration, disc prolapse, or OPLL, but excluded causes due to other reasons such as trauma. This has been amended to make our message clearer, please refer to lines 107-108, 112-113.

6. You have chosen the highest, point of JOA recovery, rather than a specific timepoint, is there any particular reason why? And what about choose a fixed timepoint e.g 12m? You should mention your reasoning in the text.

We have explained this in the methodology, with the statement "Due to scheduling difficulties, patients were not all seen at exactly 12-months after surgery, and therefore peak post-operative mJOAs were taken as a point of comparison for maximal recovery" (Line 130 – 132).

7.Results data presented does not specify which numbers are you representing, are you representing means, average values, or medians? Are you using Standard deviation or

IQR? This needs to be clarified in the text.

We have described this in the 'Statistics and reporting framework' section of the methodology with the statement "Continuous variables with normal distributions are described by mean values ± standard deviation" (Line 163 – 164).

8. Posterior approach was favoured in majority of the cases is there any particular reason why? I suspect this will be of interest, thus suggest adding this information.

We have described this in the results section with the statement "Congenital cervical stenosis was common in our locale, leading to a large proportion of patients developing multilevel stenosis and indicated to receive posterior decompression upon possessing a favorable lordotic cervical alignment" (Line 176 – 178).

9.Language needs to be improved. Words such as "enjoyed" on row 177, does not collate with symptom improvement. Please refrain to academic scientific writing norms.

We have replaced "enjoyed" with "exhibited" upon its use in both the Introduction as well as Discussion, as this constitutes a more academic term.

10.Limitation section has to updated and separated from the main discussion text. All the results presented have quite a large ranges and deviations, which makes the analysis along with the low number of very low power.

Lines 298 – 307 constitute a separate limitations section with specific mention of low statistical power.

Line 253 and 254, you suggest that reoperation may be of lower relevance for elderly patients, despite confirming that re-do surgeries help patients recover no less than primary surgeries. This seems like a contradiction, and should be carefully considered, especially with the increasing prevelence of elderly population in high income countries, and increases in the threshold of treatment for various neurosurgical pathologies.

This is a good point, due to the author's misconception of ages considered to be elderly. Our findings are described to be of less relevance in the "middle-old onwards (i.e. 75 years and over)" in Line 273-274.

12. Please arrange a specific conclusions section in your manuscript, as currently this is missing.

The last paragraph of the discussion is a conclusion now demarcated as a subsection (Lines 310 – 317).

13. You do not mention the number of patients used for the non-inferiority testing and how these were chosen? Were these randomly chosen to match the characteristics? Please specify how these were chosen, as this can introduce a selection bias and we need to be transparent.

This is described in the methodology under 'Revision group vs matched group' (Line 153 – 159).

14. Although interesting how the epidemiological aspect of the re-do surgery paragraphs is relevant to the data you are presenting? You could add your own data on patients requiring re-do surgeries and complications rates, especially in the re-operated patients. Are they of higher risk? Please ensure to add reasoning behind the used paragraph and the relevance to your particular study.

In Line 276 – 277, we add the sentence "Complications are not uncommon upon revision, although none were observed in our modest cohort of 14 patients". This is a bridging statement to that of subsequently cited literature, which quote higher complication rates albeit which larger sample sizes.

Reviewer B

Line 81 - add "in our institution"

Thank you for the correction, we have amended accordingly.

Line 86 - good methodology here
Thank you.

Line 92 - abbreviation of MRIs
We have amended accordingly.

Line 119 - can you define more how you calculate recovery rate, please mention other studies that use this calculation. If none, please state that you are the first to do it this way.

We have added the formula of (mJOA postoperative − mJOA preoperative)/(17 − mJOA preoperative) × 100 and referenced the seminal paper by Hiribayashi, in Lines 137-138.

Line 131 - should use a non-parametric test instead of paired T-test due to small sample size? Please consider. It is good that you used the U test for delta JOA and RR
We have amended accordingly (Line 148-149, 156-158) so that pre-operative JOA scores, peak post-operation JOA scores, and absolute differences in JOA scores between the two groups for the comparisons in both Table 2 (first operation vs revision) and Table 3 (revision vs. matched control) now utilize the Mann-Whitney U test. Results remain statistically insignificant.

Line 133 - please cite MCID value for JOA
We have amended accordingly.

Line 137 - great job with matching methodology
Thank you.

Line 231 - what other studies have done this? Are you the first - if so, please make this clearer.

We are indeed the first to compare the first and second surgery in the same patients and have described as such (Line 253-254).

Line 279 - great job with sample size and selection bias identification
Thank you.

Line 289 - thank you for clarifying this is the first study. I would say this more throughout the paper. I would also add a sentence in these limitations that they results needed to be taken with caution because of these limitations and future research needs to be done.

Thank you, we have done so in the highlighted amendments in both the Introduction as well as Discussion.

Reviewer C

The authors present a retrospective study evaluating the neurological outcomes after revision decompression for cervical myelopathy. The authors show favorable recovery rates after revision surgery.

Some issues to address:

1. The rates of patients achieving MCID for the revision surgery and matched controls seem low. I have seen numbers >60% in multiple studies. Can the authors explain?

In considering that the average age of our cohort was over 50, our proportion of patients reaching the MCID was comparable to that of a multi-center study based in North America (doi: 10.3171/2020.2.SPINE191495). From this other publication, only 40 – 50% of patients aged 50 and over achieved the MCID. The proportion of patients achieving the MCID only exceed 60% amongst those age < 50 years old at the time of surgery.

2. The lack of quality-of-life outcomes is a major limitation. This would have been useful to determine if patients experience improvements in non-neurological outcomes as well.

Indeed, and we have listed this as a limitation (Line 303-304). Unfortunately, this data is not readily available as our data collection was retrospective, and QOL outcomes were not routinely measured.

3. More details on the types of revision surgeries is needed- laminectomy or laminectomy and fusion or laminoplasty.

This information is added in 'Patient demographics and revision details' (Line 193 – 196).

4. Some MRIs showing the types of pathology causing recurrent symptoms would enhance the manuscript.

This is now shown in Figure 1.

5. What was the cause of recurrent stenosis at the operated levels?

Increase in degeneration resulting from disc herniation, increased osteophyte formation, as well as laminoplasty 'spring-back' were attributable causes (Line 190-192).

6. The authors acknowledge selection bias in this. But given the results of this study, would they expand their selection of patients for revision surgery. Can they provide numbers for the number of patients with deterioration who were not offered surgery?
Regrettably we do not have this information on hand because our patient list comprises only of patients who received two surgeries. This would comprise a good subsequent follow-up / comparison study with regards to those receiving re-operation and those managed conservatively following neurological deterioration.