Author’s response to reviews

Title: Reducing the extent of facetectomy may decrease morbidity in failed back surgery syndrome

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Author’s response to reviews:

Dear Prof. Georg Osterhoff:

We would like to express our sincere gratitude to you and the reviewers for the opportunity to revise our manuscript entitled “Reducing the extent of facetectomy may decrease morbidity in failed back surgery syndrome.”

We have studied carefully each comment of the reviewer. All corresponding revisions are marked in red. We have tried our best to revise our manuscript according to their comments. Attached herewith is the revised version of our manuscript, which we would like to submit as an Original Research. In line with the requests of BMC Musculoskeletal Disorders, We added a figure to meet the comments of reviewers (Fig.1), reformatted the manuscript and references and modified some statements that are not clear but have not been mentioned by reviewers (Page 2, line 20), (Page 4, line 10) and (Page 10 line 5). And the reversed manuscript was polished in mother tongue. Our point-by-point responses to reviewer comments are given below:

Our responses to the comments of Prof. Christian Johannes Pfeifle are shown first.

1. Page 5, Line 2

The underlying pathology of an interbody fusion does not have to be the same as that of a pure discectomy. A comparison is not possible here and should be omitted.
Response: In line with the comment, we have reviewed literatures concerning the indications of the pure discectomy and interbody fusion surgeries. The statement about interbody fusion was deleted accordingly.

2. Page 5, line 17

A facetectomy is not inevitable during PTED. Neither the papers cited nor the current literature mention a facetectomy. Expansion of the neuroforamen is a sufficient procedure to get the discectomy done. Facetectomy is used as an additional decompression technique or in cases of hypertrophy of the facet joints.

Response: In line with the comment, the statement about the necessity of facetectomy was changed as follows: “In PTED, it is necessary to perform facetectomy to expand the neuroforamen, and this procedure is sufficient to complete the discectomy. In patients who suffer from hypertrophy of the facet joints, extensive facetectomy is needed for the decompression of exiting nerve roots.” (Page 1, line 15) , (Page 2, line 15) and (Page 8, line 11). And the compression of exiting nerve roots when patients suffer from hypertrophy of the facet joints have been shown in the Figure 1 (Page 18, line 2).

3. Page 7, line 7

Why using a healthy volunteer for performing CT-scans to gain the FE model? One should find enough DICOM data of healthy lumbar spines without using extra ionizing radiation in the database of a hospital. The use of ionizing radiation on a healthy volunteer without need is ethically questionable.

Response: Thank you for your concern regarding ethical issues. In the hospital where the authors are currently working, to reduce the ionising radiation to patients undergoing CT, the area of the scan was strictly limited to the predetermined lesion area. For example, only the discs in the lower lumbar spine are scanned for patients suspected to suffer the lumbar disc herniation, rather than the whole lumbosacral spine area. Hence, we were unable to find the data needed to model reconstruction in the hospital’s existing database.

In addition, the volunteer who performs the CT is one of the authors in this paper. As an orthopaedic surgeon, he is aware of the whole content of this study and the potential damage of ionising radiation to the body and voluntarily accepted CT after he signed the informed consent (it was written in Chinese, so we did not upload it). Meanwhile, this study was approved by the ethics committee of our hospital and according to the provision of the Project of Jiangsu province health and family planning commission (H2018025), the volunteer received 800 yuan
as compensation. Therefore, we believe that our volunteer recruitment is in line with the ethical principles related to medical research.

4. Page 8, line 14

Constant load of 800N is not a realistic load whilst the load for one segment of the spine is changing between 100 and over 2000N. The facet joints may react different in case of heavier loads.

5. Page 9, line 12

The authors are doing a good evaluation of their FE model with changing loads. Why not testing different loads with the facetectomy samples?

Response: The two questions above are related. Please allow me to incorporate our response. Most published finite element studies have been done under a single load (please see references 4, 5, 24, 27 and 35 for details). In the experimental design, to accomplish modelling validation and data comparison with previously published finite element studies, such loading conditions have been selected in this study. Of course, the selection of this loading condition is derived from previously in vitro cadaver studies, in which such loading condition was determined based on the average body weight of the population (please see references 34 and 35 for details). In addition, as mentioned in the manuscript, the current study was conducted as a qualitative research, which aimed to illustrate the variation trend, rather than a quantitative study. We think that the data from the available experimental results are sufficient to draw a reliable conclusion.

Of course, we must admit that your suggestions are of great value in improving the reliability of experimental results. However, considering that we cannot complete this experimental study within a relatively short reversion period, we would like to add such guidance in the “Limitations” part of the submitted manuscript, with our most sincere gratitude (Page 10, line 13), and we plan to carry out related work in future research.

6. Conclusion

According to the lack of different tested axial loads one can't state that a small degree of facetectomy may not increase morbidity. Higher load may also lead to increased morbidity. The conclusion should be: the less facetectomy the better. Please change the conclusion this way.
Response: In line with the comment, we have changed the conclusion of the current study to “Less facetectomy is better in PTED for which may reduce the risk of biomechanical deterioration and consequently FBSS.” (Page 2, line 4), (Page 10, line 8) and (Page 10, line 19)

We would like to express our sincerely gratitude towards Prof. Christian Johannes Pfeifle for his kind comments and precious guidance. Further, we thank Prof. Simon Tiziani for reviewing our paper. Our responses are given below.

7. Background:
Introduction is too long --reduce by one third

8. Discussion:
Overall the discussion, the same as the introduction are too long. Moreover the authors repeat themselves on multiple occasions. Please move the whole discussion open with percutaneous down to the discussion.

Response: The two questions above are related. In line with the comment, in this revision, we have screened and deleted repeated descriptions and rearranged the manuscript for logical flow. The introduction has been reduced from 816 words to less than 420 words, and the discussion about percutaneous was transferred appropriately (Page 7, line 20).

9. page 5, line 31:
please rephrase the sentence "... shortly connected...". Language editing

Response: The phrase “…shortly connected…” was deleted in the revised manuscript.

10. page 5, line 53:
inevitable --> inevitably

Response: The phrase “…inevitable…” was deleted in the revised manuscript.
11.page 5, line 59:
previous --> previously, have --> has

Response: All occurrences of “previous” were changed to “previously” in the revised manuscript, and the singular and plural forms of verbs have also been revised. We sincerely apologise for the grammatical mistakes. We guarantee that the manuscript and this response letter have received native language polishing before submission.

12.page 6, line 1: omit "the" --> "... in degenerative..."

Response: This sentence was deleted in the revised manuscript.

13.page 6, line 1: "performance" seems to be wrong --> rephrase

Response: This was deleted in the revised manuscript.

14.page 6, line 28: What do the authors mean by "biomechanical degeneration"?

15.page 6, line 37: Which biomechanical "characteristics" do you mean? Be precise.

Response: The two questions above are related. We have incorporated our response to these questions. We apologise for the unclear expression of this concept. The term “biomechanical degeneration” was changed to “deterioration of biomechanical indexes” to illustrate adverse changes in biomechanical indexes as the shear stress on the annulus and von Mises stress on the cartilages and resulting degenerative change in such structures.

As mentioned in the manuscript, biomechanical deteriorations is one of the most important triggers for the generation of postoperative complications after spine surgery, and adverse changes in biomechanical indexes can be seen as predictors of FBSS. On this question, we have carried on the explanation in many places in the revised manuscript (Page 2, line 1), (Page 7, line 17), (Page 9, line 1), (Page 9, line 8), (Page 9, line 13) and (Page 10, line 1) . And please see references 2, 4, 5, 6, 15, 25, 26 and 35 for details.

16.page 6, line 28: "and... and... and.." --> Please rephrase

Response: This statement was rephrased accordingly (Page 8, line 6).
17. page 6, line 34: What do you mean by "contradictory results"? You list studies showing negative outcomes of both procedures, why is this contradictory?

Response: We apologise for the unclear expression of this concept. In the revised manuscript, we clearly mentioned the following conflicting conclusions: “PTED reduces the risk of FBSS seems credible” and “minimally invasive spine surgery could decrease the risk of FBSS may be unreliable.” (Page 8, line 3) and (Page 8, line 8).

In other words, previous studies state the issue of whether PTED can decrease the risk of FBSS and made different conclusions. Hence, we think that the optimisation of PTED to decrease the risk of FBSS is meaningful.

18. page 6, line 45: change to: "... to provide adequate field of view,....

Response: This was deleted in the revised manuscript. And in line with the comment, the statement about the necessity of facetectomy was changed as follows: “In PTED, it is necessary to perform facetectomy to expand the neuroforamen, and this procedure is sufficient to complete the discectomy. In patients who suffer from hypertrophy of the facet joints, extensive facetectomy is needed for the decompression of exiting nerve roots.” (Page 1, line 15) , (Page 2, line 15) and (Page 8, line 11). And the compression of exiting nerve roots when patients suffer from hypertrophy of the facet joints have been shown in the Figure 1 (Page 18, line 2).

19. page 7, line 45:

It remains unclear why the authors chose to do a finite element analysis. What was the authors' hypothesis?

Response: As mentioned in the submitted manuscript, “many studies have reported that biomechanical deterioration is the most crucial reason for postoperative complications such as FBSS” (Page 2, line 12). And “As a mechanical simulation research method, finite element analysis has been widely used in the investigation of postoperative biomechanical variations to infer the risk of postoperative complications” (Page 3, line 14). In other words, biomechanical deteriorations will cause the increased risk of postoperative complications, which can be estimated by finite element analysis (please see references 2, 3, 4, 5 and 24 for details).

The hypothesis of current study was based on published literatures. Since facetectomy may lead to postoperative biomechanical deterioration, as a typical postoperative complication, the risk of FBSS may be affected by varying extents of such deterioration (Page 3, line 11) and (Page 3, line 17).
20. page 8, line 20:

Why did you perform a CT scan of a healthy young volunteer? Was it not possible to acquire a CT scan (e.g. abdominal CT) of a patient that underwent computed-tomography for a different indication other than the spine? Was this volunteer compensated? Was any statistics performed on the data?

Response: We have addressed the issues concerning the CT scans and corresponding volunteer recruitment in point 3. The current study was accomplished by a single model without statistics. Notably, all published finite element studies have been accomplished by such method without statistical analysis (please see references 3, 4, 5, 24, 25, 31 and 37 for details).

The reliability and universality of the finite element analysis results are verified by the validity of the modelling. The validated model is considered close to the mean of the experimental results of gross specimen studies and thus can represent the general situation of the population (Page 6, line 11).

21. Conclusion:

Your data does not allow for the conclusion drawn in this manuscript. The authors present a biomechanical finite element study. The question whether a patient develops FBSS, cannot be answered biomechanically! Only a prospective clinical study with an adequate sample size would allow for such a conclusion. Please revise your statement.

Response: Under the guidance of the reviewers, the conclusion of this study was changed as follows: “Less facetectomy is better in PTED for which may reduce the risk of biomechanical deterioration and consequently FBSS.” (Page 2, line 4), (Page 10, line 8) and (Page 10, line 19).

As a finite element analysis, the conclusions in our manuscript may not be as definitive as in a clinical study, but they still provide a biomechanical reference for the relationship between different extents of facetectomy and the risk of FBSS and guidance to the clinical application of PTED. Hence, it’s still meaningful.

We have responded to all comments of the reviewers. We would like to express our sincere gratitude to the editor and reviewers for their guidance and we sincerely hope this revised version of our manuscript is acceptable. We look forward to hearing from you soon.
With kind regards

Sincerely

Lin Xie and co-authors of this paper.