Author's response to reviews

Title: Anterior cervical discectomy and fusion: Comparison of titanium and polyetheretherketone cages

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Author's response to reviews: see over
Manuscript revision “Anterior cervical discectomy and fusion: Comparison of titanium and polyetheretherketone cages”

Dear doctor Bederman,

thank You very much for the revision of the manuscript. We appreciate the efforts and constructive comments of the reviewers and have hopefully addressed all mentioned concerns adequately.

**Reviewer 1 (Dr. Isaacl Moss):**

**Major Compulsory Revisions**

1. The conclusion in the abstract as well as the body of the text states that modulus of elasticity of the chosen synthetic graft did not influence the outcome measures. The authors themselves state that there are other differences in the two cages types including the material itself, surface micro and macro architecture, as well as baseline differences in the cohorts. Therefore, I do not believe that the data in this paper supports the conclusion with respect to lack of influence of modulus of elasticity, it really only speaks to lack of influence of these particular materials given a consistent geometry in this cohort. Each material has numerous of physical properties, modulus of elasticity is only one of them. The conclusions should be restated to better represent the data.

2. The two cohorts had a statistically significant difference in age, with the PEEK cohort being older (57.64 vs 51.09). Older patients can be expected to have weaker bone and therefore a potentially higher rate of subsidence. This phenomenon may have introduced
bias into the study favoring the titanium implants. This issue should be discussed in the paper.

-Minor Essential Revisions: (all are "Minor issues not for publication")

3. Background paragraph 2: TTN-cages have been criticized to produce a "minor clinical outcome". Please clarify or re-word what is meant by "minor clinical outcome".

4. Background paragraph 2 and Discussion paragraph 4: TTN implants "show a good osseointegration", please revise syntax.

5. Methods paragraph 1: change “Patient collective” to “Patient cohort”

6. Methods paragraph 2: change “By making sure of the patients’ anonymity” to “by protecting the patients’ anonymity”.

7. Methods paragraph 6: clarify what is "physical recreation" in the post op protocol

8. Discussion paragraph 14 (Bone formation): “The comparatively little number of bone formation in our study may relate to the choice of diagnostic means and criteria to assess bony fusion. Assessing bone formation within the cages [30] was no criterion in our PEEK-group, since this would have compromised a proper comparison with the TTN-group on lateral radiographs.” Please revise syntax/word choice of underlined phrases for clarity.

9. Figure 1: clarify what is meant by “did not appear” – lost to follow up?

Author’s reply:

1. We appreciate the advice of the reviewer to focus the conclusions more on our results and discussion and have rewritten the conclusions. The influence of design, shape, size, surface architecture of a cage as well as bone density, endplate preparation and applied distraction during surgery were mentioned as further important factors to be considered besides the elastic mode. Please see our changes at p2, last paragraph and p13.

2. The reviewer points out an important issue and a potential bias. We have emphasized the influence of patient age as a potential bias in our discussion according to the reviewer’s advice. Please see our changes at p10, 2nd paragraph.

3. The inferior outcome was related to the bone graft cohort of the cited study. We have clarified the sentence. Please see our changes at p3, 2nd paragraph.

4,7,8. Thank you very much for the correction. We have changed the manuscript accordingly.

6. Our patients were advised to have physical rest for 6 weeks after surgery. We have changed recreation to rest.
9. Yes, patients that did not appear to at least one-year follow-up were lost for our study. We have clarified this in our methods section. Please see our changes at p4, 1st paragraph.

Reviewer 2 (Dr. Ilker Eyupoglu):
1. Although cage sintering is measured in relation to the height of the intervertebral disc space in current literature, this method has disadvantages placing its validity in question. The advantages and disadvantages of this method ought to be therefore more thoroughly discussed.
2. Although both filled and empty cages are routinely used in clinical practice, standards have yet to be defined in this regard. This point merits mention and discussion.
3. The point of view of the authors with regard to plating versus soft cervical collar for several weeks following surgery ought to be mentioned and the respective advantages and disadvantages of both methods be discussed.

Author’s reply:
1. We agree with the reviewer that different methods exist to measure cage subsidence. We found that the measurement of disc height may cause high interobserver errors, since the shape of the vertebral body in the degenerative spine may be strongly altered and influence a valid measurement. We have emphasized this point in the discussion. Please see our changes at p11, 2nd paragraph.

2. We agree with the reviewer that many surgeons use filled cages with autologous bone, allograft or other bone substitutes. However, we have made good experiences with empty cages in the cervical spine. We have added a paragraph to discuss this important issue at p11, 4th paragraph.

3. The reviewer points out an important issue. Many surgeons favour the plating after ACDF and/or cervical immobilization by a collar postoperatively. We have added a paragraph to discuss this important issue as well at p12, last paragraph.

Reviewer 3 (Dr. Christian-Andreas Müller):
1. The study population is inhomogeneous due to a pooling of patients with radicular and myelopathic disorders. This might influence the clinical outcome. The authors should remark this in the discussion.
2. Unfortunately the preclinical data are only rudimentary available and the only clinical outcome measurement in the manuscript is Odom’s criteria. This should state in the surgery and study design section.

3. The authors should discuss how far an additional plate-screw fixation could prevent the rate of subsidence after TTN or Peek cage implantation.

4. It would be interesting how far the fusion rate is influenced by filling the cages with bone substitutes. Are there some data based on the group of the 23 patients which was excluded from the analysis?

Author's reply:

1. The reviewer is right that patients with radicular and myelopathic diseases can be only compared in a limited way. We have not separated these patients, since the focus of our study was on the radiological results, the rate of cage subsidence in particular. We have added a paragraph in the “surgery and study design”-part of our discussion at p12, 3rd paragraph.

2. We have added a respective statement according to the reviewer’s advice at p12, 3rd paragraph.

3. The reviewer points out an important issue. Many surgeons favour the plating after ACDF and/or cervical immobilization by a collar postoperatively. We have added discussed this important issue as well at p12, last paragraph.

4. This is an interesting point. 21 of 23 patients did not show a movement in flexion-extension radiographs. A bone bridging was observed in 15 of 23 cases, which would be less than in case of empty cages. This might be surprising, but we assessed bone bridging only on lateral radiographs and did not assess bone formation within the cages. It is possible that filled cages fuse inside comparably quick, which may result in a reduced stimulus for further bone formation around the cage.

With 3 exceptions all of the patients received a TTN-cage. Due to the comparably small number of patients that hinders a good statistical comparison, and particularly due to our focus on a comparison of TTN and PEEK we did not include these patients in the study.
Thank You very much for considering the manuscript for publication. We hopefully provided satisfying answers and statements towards the comments and concerns of the reviewers.

With kind regards,

Mario Čabraja, MD