Author's response to reviews

Title: Outcome analysis following removal of locking plate fixation of the proximal humerus

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Author's response to reviews: see over
To

The Editor in Chief

BMC Musculoskeletal Disorders

Dear Editor,

Thank you very much for the kind consideration of our manuscript (4452522120287388) “Outcome analysis following removal of locking plate fixation of the proximal humerus” and the valuable comments of both reviewers. We revised the complete manuscript according to their suggestions. Hence, it is a privilege to submit a revised version hereby.

Concluding, we 1st answered all raised questions, 2nd we inserted additional clinical information according to the reviewers suggestions, 3rd the sections introduction and discussion were significantly shortened and 4th the whole manuscript was edited by a native speaking colleague.

Indexed overview of reviewer comments:

Reviewer 1:
1. Did the rotation deficit group improve their rotation? What were the before and after range of motion?

Thank you for this important hint. We added the information regarding external rotation in the results section.
2. Did the impingement group improve the specific symptoms and motion?

You are absolutely right. As it is of major importance for the appraisal of clinical relevance, we first inserted the criteria for inclusion into group Hardware related impingement (HI) by adding: “Subacromial impingement was stated if the following clinical criteria were positive: 1st painful arc 60°-120°, 2nd a positive Neer’s sign, 3rd a positive Hawkins/Kennedy test.” Furthermore we inserted the outcome data in the “results” section: “Regarding symptoms of impingement after 6 month in group HI 21 patients (84%) had no residuals at all, 4 patients (16%) had an isolated positive Hawkins-Kennedy test.”

3. This paper would be much improved if the authors can compare their group to one who did not have hardware removal.

The intention of this study was to analyze distinct effects of plate removal in symptomatic patients following Philos-ORIF. Moreover there are several articles analyzing the outcome following Philos in a quarterly manner up to 24 months (Kettler et al, Handschin et al., Bartsch et al.), which clearly demonstrate that patients do not significantly improve after 12 month post ORIF. We therefore do not believe, that adding a control group would improve the specific information of our manuscript.

4. In the conclusion, the authors state that routine hardware removal is not justified. However, this is the exact opposite of what they did in the group of patients who requested hardware removal (35% of all patients). Also, if their results were so good, with minimal complications, why would they not state that this can be done. The conclusion should be changed to read that based on their results, they recommend removal of hardware in patients with symptoms.

We modified the conclusion and tried to focus it towards a general recommendation.

Reviewer 2:

1. Overall the paper is well written but I would recommend that the authors perhaps reread the paper and try to shorten the introduction and the discussion and be more precise and concise in their language. I have not attempted to do this as it is difficult to do and not change the meanings of their conclusions. However, the paper does read a little long.

We also thought the sections “introduction” and “discussion” were too long. Therefore both sections were significantly shortened.

2. Page 4 line 94 when they refer to “adequate time period” to reach maximal recovery. I believe that this should be a time period or a definition. With the term as it is, it is very vague as to how long they did wait and what was considered a maximal recovery.

Thank you for this important comment. Although we discussed the issue of recovery after the initial surgery extensively in the discussion, we forgot to insert this information into the “patients and methods” section. We changed the sentence to: “The indication for hardware removal was stated earliest 12 month after initial ORIF.”
3. Page 4 line 98 on the impingement group. Do the authors have any criteria that they used radiographically for determining whether or not the plate is too high and is causing impingement.

The reviewer is absolutely right. We inserted the definition of “superior plate placement” as follows: “Following the AO-instructions for the implant, a distance <8mm from the proximal end of the plate to the upper margin of the greater tuberosity was defined as superior placement“

4. Page 5, line 116, they talk about the plate being removed after the tension band wiring. There is some confusion as to what this means. Did they tension band wire the fracture prior to removal or did they also remove the tension band wire when the plate came out.

Of course you are right, we have changed the sentence to: “The plate was exposed, and then first the tension band wiring (FiberWire, Arthrex, Naples, FL, USA) was removed. After removal of all screws, the plate was taken out.”

By the way, we would like to thank a reviewer like you, who really makes efforts to improve the manuscript of colleagues in old Europe.

5. As to the results, I would be interested to know two other important pieces of data. First would be what is the total number of proximal humeral fractures that they have seen during this study time period. The second question is the total number of open reductions and internal fixations done in which hardware was not removed. Also, if the authors do have any information, on this group of ORIF without hardware removal so as to compare it to the removal group.

This is a valuable comment. We added this important information regarding the total numbers by inserting the following paragraph: “From July 2003 to August 2007 a total number of 282 proximal humeral fractures in 277 patients has been seen at our department. From these, 231 fractures in 226 patients have been treated by ORIF. In total 79 patients underwent plate removal, from these 20 patients had to be excluded due to known infection, secondary screw perforation or avascular necrosis of the humeral head and subsequent prosthetic replacement.”

However, 56 patients (59 fractures) who received ORIF, between October 2006 and August 2007 did not reach the “adequate” observation period. Concluding from 172 fractures 79 fractures (45%) received plate removal.

6. On page 4, line 93, they have a sentence which states “all fractures were conjoined radiographically”. This is not a known term in English and needs to be defined.

We apologize for our “German” English and changed the sentence to: “After this, radiographs for preparing plate removal were taken. Union of the fracture was defined as radiographic presence of mature callus on 2 planes as determined by an experienced musculoskeletal radiologist.”

7. On page 4, line 97, I would simply say that “instead of according to medical reasons”, I would state “according to the reason for hardware removal, the patients were divided”

The sentence was changed according to your suggestions.
8. On page 4, line 99, I would state that “instead of to inadequate high-plate”, I would state “to superior plate placement”.
   This term was also changed.

9. On page 4, line 101, “time, hardware in place” probably should be replaced by “duration of implantation” or “duration of fixation device implantation”
   The term was changed. Moreover, we modified the corresponding phrase in the results section.

10. On page 5, line 116, the word “uncovered” should be “exposed”
    “uncovered” was changed to “exposed”

11. On line 117 on page 5, the sentence beginning with “Fluoroscopically” I would suggest be changed to “Complete hardware removal was confirmed by fluoroscopy”.
    The sentence has been changed according to your suggestion.

    The phrase was modified.

We hope that our revised manuscript is now acceptable for publication in BMC Musculoskeletal Disorders

Respectfully,
Dr. Chlodwig Kirchhoff, M.D.