Author's response to reviews

Title: Costs and outcome for serious hand and arm injuries during the first year after trauma - a prospective study

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Author's response to reviews: see over
Dear Editor,

Thank you for the reviewers’ comments concerning our manuscript “Costs and outcome for serious hand and arm injuries during the first year after trauma – a prospective study”. We do apologize for being late with our reply, but the last months have been filled with clinical duties at our hospital. We appreciate and thank the reviewers for all their valuable comments. We would like to respond to the reviewers’ comments as follow (marked in bold; also marked in bold in the manuscript):

Reviewer’s report 1

Major Compulsory Revisions

1) Statistical methods section – first paragraph: I am a bit puzzled about the Kruskal-Wallis ANOVA. This is valid for comparing multiple independent groups (eg. for comparing place of injury), but the authors also used it for comparing the repeated measurements within patients. As these are not independent, a Repeated Measure ANOVA or Friedman test should have been used for this specific comparison.

Authors’ response:  
The choice of statistical method relates to the question asked. We analysed the three measurements at 3, 6 and 12 months as three cross-sections and asked whether they could be seen as equal. We used the Kruskal-Wallis non-parametric test, rather than a parametric test as the ANOVA, in order to avoid imposing restrictions. The weakness of the Kruskal–Wallis test is that it does not say anything about how it is different. Another question would be: Do these individuals report different DASH using information from all measurements at the same time? And if so, what is the average difference. That is, does DASH scores on average differ depending on the length of sick leave they will end up with. Using panel data regression methods (generalized linear population averaged model). By this method, DASH was on average 10 points higher for both the group 90-179 days and 180-364 days compared to the group with the shortest sick-leave (<89 days). However, the difference was not significant at conventional levels (p=0.13 and p=0.14, respectively). The group with the longest sick leave (365+ days) had rated 26 points higher on average at all measurements. Nevertheless, we found this comparison of mean difference less interesting and more difficult to interpret than reporting median at each measurement. The advantage of comparing medians is of course that it is less sensitive to outliers with high/low values.

2) Discussion section: the authors state they included consecutive patients, which was not the case; they included a random sample of 54 patients, representing about a third of all 153 patients seen during the study period.

Authors’ response:  
Thank you for pointing this out. We have changed to “random”.


Minor Essential Revisions

3) Abstract – Results section: “the majority of 45/153 included patients” is confusing. From the results it is clear that of 153 patients seen at the Department, 132 met the inclusion criteria. Of the latter group, 45 were included.

Authors’ response:

Thank you for pointing this out. We have removed the confusing part.

4) Patient material section: it is unclear why 54 patients were included. Could the authors include details on the pre-study power calculation? How were they selected? Using a prefixed random number list, or was each patient randomized to inclusion or not?

Authors’ response:

Our goal was to include as many patients as possible during the study period. However, these patients came randomly during day/night, weekdays and months and we (authors) were also dependent on colleagues for including patients. Sometimes patients were not asked to participate in the study.

5) Characteristics section – fourth paragraph: was there a reason why patients who were not active in the labour market (students, early retirement, long sick leave) were not excluded from participation? The main objective – studying return to work – does not apply to this group.

Authors’ response:

Return to work was one of several outcomes of interest. Health care costs were relevant for all included patients, as were patient reported outcome measures DASH and EQ-5D.

6) The statistics section does not mention all statistical tests performed. The results section mentions chi-squared analysis and correlation analyses. For the correlation analysis, I assume Spearman Rank correlation (or Kandall Tau) was used?

Authors’ response:

Thank you for pointing this out. We have now added the missing tests in the methods section.

7) Figure 1: The y-axis is labeled as “Survival”. I guess that “Rate of persons who did not regain work” was meant.

Authors’ response:

Thank you for pointing this out. We have revised the y-axis label for Figure 3 to “Proportion remaining at work”.

Discretionary Revisions
8) Background – first paragraph: In the second sentence the word “but” seems misused.

**Authors’ response:**

**Thank you for pointing this out. We have changed the word.**

9) Methods section – second paragraph: has the mapping of DASH scores to EQ-5D dimensions been validated of published before? If so, please add a reference. If not, the procedure should be described in more detail. If data on validity and intra-/inter-observer variability are known, it would be preferable to add these also.

**Authors’ response:**

We thank the reviewer for her interest in the mapping of EQ-5D to DASH. This was a minor part of our analysis, which needs further elaboration to have a stand-alone paper, e.g. validation by additional observers and several patient samples. We report the results from our exploratory analysis as a pilot study, which could inspire other researchers to further develop and validate translation algorithms from DASH to EQ-5D. Please, see the figure below for the mapping results against the patient-reported results.

![Mapping results](image)

10) Statistical methods section – first paragraph: the authors measured return to work. It is unspecified whether or not they only considered full time work resumption, and how they handled resumption of another function (due to inability to return to their previous occupation).

**Authors’ response:**

**Thank you for asking for this clarification; we used a broad definition, which required return to at least some labour market activity. A brief explanation is added in the methods section.**

11) Discussion section: two surprising and interesting findings are mentioned (1) costs for patients regaining work partially exceed that of full time employees, and 2) patients with severe injuries were more likely to return to work than those with a major injury). Intuitively, the opposite was expected. Especially for the first finding, could the authors speculated why the result was as observed?
Authors’ response:
(1) The observation of higher health care costs for partially active people refers to labour market status before injury. We have added “We can only speculate as to why in our sample, but it is possible that jobs where people work half time are more likely to be manual and involve greater risks of injury.” on page 9.

(2) We didn’t understand this remark. If anything, we expected that a severe injury (50≤HISS≤100) would involve better outcomes, including earlier return to work compared to the situation for people with major injuries (HISS>100).

12) Methods section – first paragraph: … and the quality of life questionnaire EQ-5D, including five questions…. “Including” should be “consisting of” as the five questions represent the full questionnaire. If I understand correctly, the authors only used the EQ-5D utility index, but not the EQ-VAS?

Authors’ response:
Thank you for suggesting a more appropriate word. Yes, we used the index, the EQ-5D VAS. The latter is not preference based and has known bias towards mean with few people marking perfect health or lowest health. Changed.

13) Characteristics section – third paragraph: there is a typo in the sentence “The injury mechanisms were equally divided by caused by a machine ….”.

Authors’ response:
Thank you for pointing this out. Changed.

14) Discussion section – first paragraph: in the 9th line of the Discussion, the author refer to their previous data. The reference is missing.

Authors’ response:
Thank you for pointing this out. We have removed the sentence.

15) Discussion section: I don’t really see the relevance of the paragraph about the DASH to ED-5D value recalculation in light of the research question. It is without a doubt a very interesting approach, but seems to be more suitable for a separate manuscript.

Authors’ response:
We wanted to initiate this thought, as many clinical studies have not collected patient reported outcomes by preference based generic measures. Our pilot study may be seen as an inspiration. See other points raised above and our response. However, the sentences in the Discussion are deleted.

16) Discussion section – last paragraph: typo in “we didn’t included al patients…”

Authors’ response:
Thank you for pointing this out. Changed.
Major compulsory revisions
1. The aims of the study are vague. An abundance of data and analyses are presented without any specific focus. Please position this paper in the previous literature on the consequences of severe hand injuries and make clear which specific research questions are addressed by this additional work.

Authors’ response:
We have added a citation to de Putter et al 2012 in Introduction to the previously cited papers in the area. Previous studies underline the often-high costs per case from health care utilization and productivity loss for several types of hand injuries [the manuscript cites 5-7 + de Putter et al 2012]. Our study added the patient perspective and included patient related outcomes measured by the clinical DASH score and the EQ-5D generic quality of life instrument.

2. This is a one clinic study with a very small sample (n=45). The health care costs seem to include in hospital costs only, which is a too narrow perspective. The data are not adequate for a cost-of-illness study, which should be population-based and include costs outside the hospital as well. The data can be used for a much shorter paper focusing on quality of life outcomes only (DASH and EQ 5D).

Authors’ response:
Our study was not a cost-of-illness study with the aim of calculating total costs on a national or per capita level. By using more detailed data than the traditional cost-of-illness studies, our aim was to go beyond reporting the size of costs and to discuss associations between cost, place and type of injury; and patient reported outcomes. We had an episode perspective on costs and included health care-costs associated with the severe/major hand injuries, including treatments during the emergent phase and follow-up visits to physicians, nurses, and for rehabilitation because of the injury. We did not include potential visits to local health care centres during the period of analysis as the Department of Hand Surgery treats patients during the whole episode.

3. Please explain why a small random sample (n=45) of patients with severe hand injuries was included instead of all consecutive (n=132) patients with severe hand injuries.

Authors’ response:
Data collection is resource demanding per se and each patient had four separate measurements (baseline, 3, 6, and 12months). See also our previous response above about inclusion of patients.
4. The discussion is lengthy, but pays too little attention to the limitations of the methods and data sources. I

Authors’ response:
We have revised the discussion and shortened it. We have, as pointed out above, stressed the limitation that only randomly selected patients were included.

We do hope that these changes are sufficient and that you now may consider to publish the manuscript in BMC Public Health.

Looking forward to hearing from you

Sincerely yours

Lars B. Dahlin