Reviewer's report

**Title:** Outcome and clinical changes in patients 3, 6, 12 months after a severe and major hand injury - can sense of coherence be an indicator for rehabilitation focus?

**Version:** 2  **Date:** 2 November 2010

**Reviewer:** Felix Angst

**Reviewer's report:**

Major compulsory revisions

15. Conclusion match aims.
The description of the aims has been markedly improved. In the discussion/conclusion, one should only describe that there are differences between the low and the high SOC group. It is not possible to conclude that the SOC is predictive for outcome since the influence of confounding was not assessed in this study (see below, item 19). The study is a descriptive study and not a prediction study. I would avoid the terms "predictor", "prediction" throughout the paper.

16. No pre-injury SOC data.
This is now stated on item 6 of the comments of Langeland. Move this reasoning into the paper, Methods. Discuss this weakness in the discussion, limitations, also the ref. of Langeland. Again, the event of the injury may have affect the post-injury SOC.

17. Low sample size.
Is now stated in the limitations. Outline there (in the paper, discussion) how this fact will limit the results and conclusions in greater detail.

18. 54 statistical tests in 45 patients.
This is only discussed in the comments to the revision but not in the paper. The reasoning given there is too vague. Either use less tests or quantify "the risk of many false positive tests" by the help of your statistician.

19. Dichotomizing the sample by the median SOC score.
This has clearly described in the Methods. However, the study can observe outcome differences between the 2 groups but in cannot attribute these differences to the SOC score. Outcome can be also predicted by higher age or more comorbidities etc. in one group when compared to the other. The study does not make attempts to control for such confounders. Thus, the study results are descriptive and not analytic for prediction. See item 15.

This has been improved on p. 4. However, there may be inter-individual differences in therapy which can confound the outcome.

Minor essential revisions


Has been much improved. I would clearly state that only descriptive and not prediction analysis was performed. This should be discussed in the limitations.

22. Consistent reporting of decimal places of the data:

In OK now for Table 1. Still to improve: Tables 2 and 4: 0 is not 0.0, 10 is not 10.0 (and vice versa), report 2 digits for ES (as everybody does). Tables 3 and 5: means and stddevs should have the same number of decimal digits. P-values: 3 digits throughout. Z-scores: 2 digits.

23. SOC description.

Has been sufficiently improved.

24. Description of the DASH, HISS.

The DASH is now sufficiently described in Methods, Instruments. The HISS: move the description form Methods, Participants into Methods, Assessment instruments. Give more details about the items and construct-domains asked in the HISS.

25. Scaling 0=worst, 100=best for all instruments.

I am not sure whether this has consistently been done. The DASH scaling is indicated vice versa on p. 6: 0=best, 100=worst. Please check.

26. VAS grip strength.

The description that this is a subjective assessment is now stated in the Methods. Why did you not use ("objective") strength measurement by dynamometer as done everywhere in literature? Please discuss that and state this limitation.

27. Ref. for Swedish norms.

Is perfectly given now.

28. Z-scores for the not-normally distributed SF-36 scores.

State the reasoning why to use z-scores and the weaknesses of that in the Methods, Analysis and the weaknesses in the limitations.

The use of the Wilcoxon tests is well done.

29. Coping deleted.

Well done.
Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests.