Reviewer’s report

Title: Posttraumatic stress symptoms and health-related quality of life: a two year follow up study of injury treated at the Emergency Department

Version: 2 Date: 31 July 2011

Reviewer: Thomas Elbert

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The somewhat interesting part of this paper concerns the quantitative assessment of the health-related quality of life in relation to PTSD in victims injured by an accident. Prevalence rates of PTSD are of lesser interest, as they depend on the particular population and thus cannot be generalized. The fact that only 37% of patients had responded in the first assessment and only 21% in the final assessment makes these figures irrelevant. There is no way, one can adjust for these missing values. For instance, men may be less likely to respond when traumatized, while women may show an opposite behavior – the potential for such a response bias will hardly allow meaningful conclusions with respect to gender. Similarly, the patients with more severe injuries may be less likely to fill in a questionnaire etc. Moreover, as PTSD cannot be validly assessed by a self-report questionnaire, the figures would be questionable even for higher response rates. I would therefore suggest that the focus of the paper be shifted to the association between PTSD and HRQoL, although the results are not very surprising.

Major Compulsory Revisions

The introduction neglects that PTSD generally arises from cumulative exposure to traumatic stressors: the greater the number of various types of traumatic stressors experienced by an individual, the more likely the individual is to develop PTSD, and the more pronounced the symptoms will be – a cumulative effect, shown in many studies. In addition, higher trauma load has been found to reduce the probability of spontaneous remission from PTSD (e.g. Kolassa et al.). Therefore prevalence rates in response to a traumatic stressor such as a single accident bear little meaning, if the level of previous traumatic stressors in the population is not specified. Therefore, the rates of previous reports vary across a wide range.

The authors say “that missing IES values were imputed by substituting the median value of nearby 5 points”. The authors should give the procedure in somewhat more detail, a rationale should be provided (given that the IES was a measure of central importance) and it should be indicated for how many cases IES values have been missing.

The essential measures were assessed over time and it is thus not clear, why the authors present single t-tests instead of repeated-measures ANOVA.
Figure 1 and 2 give no additional information to the text. The relevant message is also presented in Figure 3. Figure 3 is useful, but not very well done – use e.g. dotted or dashed for no PTSS.

Minor
spell out ED-treated in abstract, give full name of EQ-5D

**Level of interest:** An article of insufficient interest to warrant publication in a scientific/medical journal

**Quality of written English:** Acceptable

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