Author's response to reviews

Title: Prevalence rate, predictors and long-term course of probable posttraumatic stress disorder after major trauma: a prospective cohort study

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Author's response to reviews:

BMC Psychiatry
Thomas Ehring
Associate Editor

Concerning: 1463566928741724 (previous 1352939829737490)

Original title: Prevalence rate, predictors and long-term course of probable posttraumatic stress disorder after major trauma: a prospective cohort study

Dear Associate Editor,

We thank you for accepting our manuscript for publication in BMC Psychiatry pending additional revisions.

The article is revised on the BMC Psychiatry web page. The specific revisions due to the review comments are highlighted with track changes. In case of further information or adjustments are required, we are most willing to help. We hope the amendments will be sufficient for acceptance.

Kind regards,
Dr. Juanita Haagsma, on behalf of all authors

REVIEWER COMMENTS

We would like to thank the reviewers again for the valuable comments to the manuscript. Below you will find our response, including the changes we have made in the manuscript. We hope that we have clarified the questions and that we have provided satisfactory changes in our manuscript.
Reviewer 1
Overall the paper is substantially improved with revisions responsive to the prior review. My only further comment is that where the responders are compared to the non-responders, it would be useful to know if the two groups differed (or not) in terms of TBI.
The responders and non-responders did not differ in terms of TBI.

Reviewer 2
This is a revised version of a previously submitted article titled „Prevalence rate, predictors and long-term course of posttraumatic stress disorder after major trauma: a prospective cohort study“. The study question is timely and of interest to BMC Psychiatry readers although the study is limited by the lack of baseline PTSD diagnosis, absence of clinical interviews and reliance on subjective symptom severity scores based on IES cut-off scores. The following critical comments remain:
(1) please specify in the introduction what you refer to by major trauma, i.e., traumatic stress, traumatic injury.
We have specified what we refer to by major trauma in the introduction.

(2) „PTSD symptoms (...) and may either disappear immediately after the event or have a delayed onset“- please provide a reference.
We have provided a reference.

(3) The authors should spell out what exactly they are investigating in terms of pre-trauma hospital care, i.e., they should specify that they index the presence versus absence of pre-hospital trauma care via helicopter or other emergency teams, rather than any other medical, interpersonal or information-processing aspects with regards to pre-hospital care. This information should also be added to the table (Table 3). They may also want to spell out hypotheses relating to this proposed predictor and its association with chronic PTSD and speculate further in the discussion why this was not significant. Might it be that HEMS and EMS attended to the most severe cases and that some of them may have developed PTSD in the absence of this care?
We have included all the suggestions that the reviewer makes in point 3 in the manuscript.

(4) Relatedly, the abstract mentions the „advancement of prehospital trauma care“- what is meant by „advancement“?
We have removed “advancement” from the abstract and manuscript, since the term is confusing.

(5) In the limitations, the authors should state that other predictors were not indexed in this study, such as peritraumatic processing, social support or other predictors suggested by recent meta-analyses or other empirical studies on injury survivors.
We have included this limitation in the discussion section.

(6) Additional: please check punctuation throughout the manuscript
p.5 admitted*to* ICU
We have checked punctuation.

Editors comment

In their reports, the reviewers point out a number of issues that need further attention. Please follow all of these suggestions when revising your manuscript. In addition, I would like to draw your attention to the following issue: In the first round of reviews, the reviewers and I highlighted the use of an outdated version of the IES as a major limitation of your study. Although you briefly mention this in your discussion, I think that this issue requires more careful consideration. There are two critical aspects to this issue, which I would like you to attend to in a more careful way in your revision. First, the IES used in your study only assesses two of the three symptom clusters of PTSD according to the DSM-IV. You should acknowledge this fact more clearly (a) in the Method section when describing the IES and (b) in the discussion section when attending to possible implications of this limitation.

We have acknowledged the fact that PTSD assesses two of the main PTSD symptoms according to the DSM more clearly in the methods section and in the discussion section.

Second, the authors of the IES explicitly state that the instrument cannot and should not be used to assess PTSD. Although I think that the use of a validated cutoff on this scale is defensible, I agree with one of the original reviewers that scoring above this cutoff must not be confused with a diagnosis of PTSD. In addition to the general problems related to such an interpretation of the cutoff scores, the fact that one of the three symptom clusters of PTSD is not even assessed in the IES makes this problem even more serious. Therefore, please follow the original reviewer's suggestion to replace "PTSD" by "probable PTSD" throughout your manuscript, including the title, abstract, method, results and discussion sections.

We have replaced “PTSD” by “probable PTSD” throughout the manuscript, including the title, abstract, method, results and discussion sections. Also, we have included an explanation of the use of the term probable PTSD.