Reviewer's report

Title: Risk Factors for PTSD Symptoms Among Deployed US Male Marines

Version: 1 Date: 16 September 2009

Reviewer: Thomas Ehring

Reviewer's report:

The manuscript describes results of a two-wave prospective study investigating predictors of PTSD in US Marines deployed to Iraq. The manuscript is generally well-written. In addition, the relatively large sample size is a clear strength. However, the use of potentially unreliable measurements complicates the interpretation of the findings. In addition, I think that the authors should try to make a stronger case for the relevance of the study and its contribution to the literature.

Major compulsory revisions
(1) The three main predictors of PTSD investigated in the ms are severity of trauma exposure, perceived threat to life and social support. All three are well-known predictors of the disorder as shown by a large number of studies in military and civilian populations and confirmed in meta-analyses (Brewin et al., 2000; Ozer et al., 2002). I therefore think that the current study mainly replicates well-known findings rather than adding a truly new piece of information to the literature. I am wondering whether the authors would agree with this statement. If they do agree, I suggest that they acknowledge this more clearly in the abstract and throughout the manuscript. If they don’t, it would be good to provide some more information on how this study truly extends earlier research.
(2) I see the fact that most predictor variables (e.g., social support, perceived life threat) were assessed with single items of unknown psychometric properties as the main limitation of the study. In my opinion, this should be clearly acknowledged as a limitation and receive some attention in the discussion.
(3) I am wondering whether data was collected anonymously. If not, some discussion on how this may have influenced participants’ responses to the different measures appears warranted.
(4) Results of past studies have suggested that retrospective self-report of combat exposure may be rather unreliable and influenced by current PTSD symptom levels (e.g., Roemer et al., 1998; Southwick et al., 1997). The authors may want to take these findings into consideration when discussing the results of their study.

Minor compulsory revision
(5) The dependent variable is a diagnosis of PTSD established on the basis of participants’ responses to the PCL-C. I therefore think that the term ‘PTSD symptoms’ in the title, tables and throughout the manuscript is misleading and
should be replaced with the term ‘diagnosis of PTSD’. On a side note, I am wondering whether the results stay the same if a multiple regression analysis with the continuous PTSD symptom scores was used instead of a logistic regression with a categorical outcome variable.

(6) In the abstract, the study aim stated ("determine significant exposures associated with PTSD") is not entirely consistent with the results reported later in the abstract (perceived life threat and social support as predictors). Please make this more consistent.

(7) In some participants, PTSD symptoms assessed may be related to traumatic events experienced before deployment. Were these events assessed at all? If yes, please include this variable in the analyses.

(8) It would be good to have some indication as to how well the final model predicts PTSD. Please provide some goodness-of-fit data and/or percent of participants correctly classified.

References


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

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I declare that I have no competing interests.