Reviewer's report

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

Version: 1 Date: 18 September 2009

Reviewer: Cindy Eckart

Reviewer's report:

This study investigates the relationship between PTSD symptoms and potential promoting or protecting factors (as combat exposures, socio-demographics and social support) in a sample of Marines who were deployed in a current conflict area. Analyzing their survey results in a manual backward multivariable logistic regression the authors revealed several combat exposure variables and the size of the social support network as being associated with the development of PTSD symptoms. The authors argue that their results in line with previous reports support the significance of the experience of personal threat on the development of PTSD symptoms. Furthermore, they highlight the importance of a large social support network as a protective factor against PTSD.

The paper is interesting and well written. The argument is concise and well structured. As military soldiers are frequently exposed to combat experiences and PTSD prevalences among them are high, it is of special interest to investigate factors that contribute to the development of PTSD in this population. The RAP survey therefore offers a valuable opportunity, as pre- and post-deployment characteristics of soldiers might be compared. However I have some concerns regarding the analyses of survey data and the interpretation of results that might improve the impact of this work. For these reasons, which are detailed below, I recommend a major revision and a second review round before publication of this paper.

Major Comments:

1. Pre-deployment/post-deployment comparison: As far as I understood, the 706 responders participating in this RAP II survey already completed the RAP survey thus offering a valuable data pool of pre-deployment/post-deployment information (one of the major strengths of this work). However, I was surprised that no comparisons were made between the two time points. Were the survey instruments of RAP and RAP II identical or at least comparable in their questions regarding socio-demographics and drugs? Were there any mentionable changes in health, family status, health variables besides PTSD, tobacco, substances or alcohol use or social variables that might be attributable to the experience of combat exposure or the development of PTSD symptoms? Might data from the RAP be included in the regression model to identify risk or resilience factors for later PTSD symptom development? Was there any variable in the RAP survey that could operationalize social support before deployment? Because this might be of special relevance in the question whether a lack of social support
constitutes a risk factor for developing a PTSD or rather a consequence of avoidance symptoms.

2. Traumatization beyond deployment: Is there any information about traumatization that might have occurred before or beyond the deployment episodes? On page 7 the authors mention adverse childhood experiences. These should be further clarified – which kind of experiences were meant, how were they measured, were potential traumatic experiences included and how were adverse childhood experiences controlled for? I apologize for my unfamiliarity with the PCL-C, but is this inventory referring to a general event list of potential traumatic events (as e.g. the CAPS or the PDS does)? Or does it explicitly refer to particular experiences (in this case combat exposure) so that it can be inferred that PTSD symptoms were related to this event? As the present investigation especially aims to control for the influence of combat exposure on the development of PTSD symptoms, potential traumatization (beyond combat exposure) should be closely controlled for.

3. Building-block effect of traumatization: There is a large amount of literature, documenting a building-block effect of traumatization - the more traumatic events a person experiences the more probable is the diagnosis of PTSD and the more severe are the resulting symptoms (e.g. Neuner et al., 2004, BMC Psychiatry 4, 34; Dohrenwend et al., 2006, Science 313, 979-982). Furthermore, the authors suggest a similar relation in the discussion of their results (page 11). Surprisingly they did not test this hypothesis with their own data. Nine combat-related, potential traumatic experiences were questioned in the survey. These items might be used to create a sum score of traumatization. Is there any relationship between the number of combat-related potential traumatic events experienced by the subject and resulting PTSD symptoms? When a sum score of traumatization is included in the regression model, does this variable prove to be influential?

4. Statistical procedure: I apologize for my unfamiliarity with logistic regressions, but when the authors state that they performed a "manual" stepwise logistic regression, how were the non-significant variables eliminated - all together as a whole or stepwise? As the chi statistics may change with any change that is made in the compositions of variables in which order were the variables eliminated? Beginning from the smallest chi statistic proceeding to the highest that yielded non-significant results? Or were there any theory driven considerations and weightings? How were resulting models (with and without a challenged variable) compared to make a statistical decision if one model should be favored over the other? Likelihood ratio tests? Or comparison of information criteria, as AIC, BIC or SIC? Apart from the odds ratio and the confidence interval a depiction of p values and eventually chi square statistics of the respective variables (at least in the tables) might be valuable. How was the general model fit of the final model, was a goodness-of-fit test for the final mode performed (e.g. comparing it to the intercept-only model)?

5. Relation to other study populations: In their introduction the authors mention some previous work about risk factors for the development of PTSD. It could be interesting to mention on which population this research was conducted. This
thread should be continued in the discussion of results as well. How did populations, formerly investigated regarding risk and resilience factors of PTSD, differ from the Marines in the present study, how might these sample characteristics result in different risk factors between populations and how could the findings in this sample be generalized to other populations (e.g. the importance of social support)? In this context factors that did not proof to be influential in this sample (when they were associated to PTSD in other samples) should be mentioned as well (e.g. age, education, marital status, socioeconomic status) - maybe potential reasons for the lack of influence in this specific sample could be discussed.

Minor Comments:

1. On page 12 the authors quote, that it “is reasonable to speculate that additional deployment events increase the probability for exposure. Why is this speculation not tested? One could group the population according to have been deployed one or two times and just test, if one group experienced more adverse combat exposures than the other.

2. The authors discuss that combat exposure (that is in most cases inextricably linked to war deployments) is the most important predictor identified by their analyses. What might be implications from that knowledge?

3. In their discussion the similarities and differences to Hoge, 2004; however, the difference in PTSD prevalence between the two populations is 1.4% and thus eventually due to chance. The authors might eventually shorten this paragraph. The conclusion that these differences are an external validation for the thesis that more combat exposure (postulated in the Hogan population) might be associated with higher prevalence of PTSD symptoms, however seems highly speculative to me in this context and should be deleted.

4. In the abstract, the authors highlight the importance of a large support network as a potential protective factor against the development of PTSD symptoms. However, in their discussion, they rather emphasize a reciprocal interaction between PTSD symptoms and social contacts, with less social contacts favoring the development of PTSD and PTSD avoidance symptoms resulting in withdrawal from social contacts. As regression analyses are correlational analyses and the notion of a potential bidirectional influence might be better supported by this technique as the notion of a causal relationship, the conclusions in their abstract should be drawn more cautiously.

5. Nine individual combat exposures were assessed. PTSD symptoms scaled from 17 to 85. To improve the clearness of the population characteristics it might be helpful to mention ranges, means and standard deviations of the number of combat exposures and PTSD symptoms in the study sample as a whole and in the subsample of 11% showing clinically relevant PTSD symptoms.

6. The responding rate (13%) is relatively low, might there be any reasons for that?
7. Check the manuscript for abbreviations, e.g. I was not able to find an explanation for the abbreviation NCO in the manuscript.

8. The authors use some military terminology regarding the rank/status of participants. The non-military reader might profit from short descriptions what distinguishes these ranks (e.g. more or less combat exposure, more control over maneuvers etc).

**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.