Reviewer's report

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

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Reviewer: Birgit Kleim

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The current study explores an interesting and timely topic in the field of traumatic stress that could be of interest to readers of BMC Psychiatry. However, I have some conceptual and methodological criticism that could be included in a revised manuscript.

(1) A general issue that should be discussed is how trauma exposure, specifically exposure to particular events, such as those including greatest fear of death lead to more PTSD. Why did subgroups of the sample, who were also exposed to such events NOT develop PTSD. The role of cognitive factors, including different ways of peritraumatic processing, including dissociation, for instance, which are likely to mediate the association, could be discussed.

(2) Relatedly, I wondered whether wearing a gas mask, using nerve agent antidote, or taking PB pills to protect against nerve agent exposure, may have influenced peritraumatic processing, and may have led to increased/decreased attention, or a greater likelihood of perceptual processing. Such processing, in turn, may have influenced on subsequent rates of PTSD.

(3) The authors mention a selection bias; this may indeed be the case: those with symptoms may be less forthcoming in filling in questionnaires (fear of stigma may play a role in this population), people with lower intelligence levels may be less likely to return a questionnaire, and they specifically included those who successfully completed boot camp. All this may have led to an underestimation of PTSD prevalence. I realise that selection bias is noted as a limitation in the discussion, but it should be highlighted further that the survey was only returned by 13% of all who were deployed initially.

(4) A limitation in the methods may be the lack of assessing exposure frequency. If I understand correctly, the authors indexed whether or not marines were exposed to each of the given combat exposures, but did not assess the frequency of which exposure took place. Moreover, from the present data, we do not know how distressing each event was to participant. The same incident could have been perceived as more or less distressing in different individuals, and in the same individuals at different times.

(5) How long after deployment were participants assessed, and had the same amount of time elapsed between deployment and study assessment? Differences in elapsed time may influence the results.

(6) On p.5., the authors mention “Dillman procedures”; I am not familiar with this procedure and wondered whether it would be worth including a brief explanation.
(7) Can the authors spell out the direction of the significant interaction term between number of friends and combat exposure severity means and how they interpret this result?

(8) It should be highlighted that PTSD diagnoses were based on self-report measures. Which cut-off was used to determine diagnostic status, were interference ratings included?

(9) On p. 11, it is stated that, for each dimension, a dose-response relationship to PTSD exists, with greater exposure leading to higher PTSD symptoms. It should be noted whether this would also be true for the sum of combat exposures assessed.

(10) Could there be a floor effect with respect to seniority and rank of marines? All of them were fairly young, and the results may be different if more experienced marines, who presumably had more exposure then, are included. On the other hand, higher rank marines may be less exposed as they are less in the combat frontline and junior marines of lower rank may be more forthcoming about PTSD symptoms?

(11) The fact that social support may play a protective role is highlighted and is indeed an important finding, which is in line with recent meta-analyses. However, from the current assessment, some points remain untouched. In particular, how did people answer the question: were supportive people available at the time of the incidence? How was social support effective and protective, i.e., were people in touch with their friends and family? Is social support by colleagues and fellow marines at the time of exposure or shortly after exposure important? I realise that these questions can not be answered from the current data, but the authors may want to include additional literature and speculate further about this relationship.

(12) Some more practical and clinical implications would be useful, it could be highlighted that the assessment of combat exposure was brief, but the upshot of this is that, if replicated in further samples, it could potentially be used to screen those at risk who may benefit from prevention programs of early intervention programs.

Level of interest: An article of importance in its field

Quality of written English: Acceptable

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

Declaration of competing interests: NA