Reviewer's report

**Title:** Traumatic events, other operational stressors and physical and mental health reported by Australian Defence Force personnel following peace-keeping and war-like deployments

**Version:** 1 Date: 14 November 2011

**Reviewer:** Danny Horesh

**Reviewer's report:**

This paper deals with a very important subject, i.e., peacekeepers' exposure to traumatic stress, and its implications in mental and physical health. Peacekeeping is often a highly taxing experience, and its implications have yet to be fully understood. The study has several strengths, most notably a large sample and 2 comparable samples of peacekeepers. However, it also suffers from several weaknesses, which should be addressed:

**Introduction**

1. In the discussion, the authors mention the term "dose-response". I believe this term, and the literature surrounding it, also deserves mentioning in the introduction, as it is highly relevant to the subject of this paper. There are mixed results regarding the connection between exposure and mental health, and this should be noted in the beginning of the text.

2. On p.6, the authors rightfully mention that peacekeepers might also be exposed to warlike stressors. This is a very important point, the complexity of which deserves more elaborate discussion.

3. One of the major flaws of this paper has to do with the lack of distinction between objective (e.g., "I handled dead bodies") and subjective (e.g., "I felt threatened by the experience") exposure. There is plenty of literature distinguishing between the two, as their implications are known to be different in term of mental health. This lack of distinction begins in the introduction, and continues throughout the paper. It is, as I have stated, a significant shortcoming of this paper.

4. I suggest that the authors re-arrange the last paragraph of the introduction (p. 7), so that (1) several separate hypotheses will be presented, so that one will better understand which refer to exposure and which refer to the association between exposure and mental/physical health, and (2) the aim of comparison between the two samples will be more clearly stated.

**Methods**
1. When I saw that "gender" was a variable here, I was wondering: are peacekeepers here both male and female? If so, I strongly suggest that gender differences be addressed in the paper.

2. I understand one sample was in fact the entire population of peacekeepers, while the other was samples from a much larger population. The authors should consider whether or not this has implications that may have escaped their eyes.

3. Generally speaking, the measurements sub-section should be revised, for 2 main reasons: 1. no psychometric properties (reliability, validity) are mentioned for any scale. 2. for most scales, the descriptions are very short and lack sufficient information on the measurements' structure. Also, authors should note that the first measurement has no name, and therefore we cannot identify its source or aim. In addition, it is very difficult to understand the rational for including several scales that measure similar constructs (e.g., how is the K10 different from the scale above it? Both measure non-specific symptoms).

Other specific problems in this section include: 1. the first scale taps severity of symptoms (mild, severe, etc.). What does this serve, if later on they are all counted as "yes" for reporting a symptom? 2. Also, at the end of the first measurements paragraph several questions are described, in which participants are asked to rate their own condition. If this is not an integral part of the measurement (and it seems not to be), then this description should be separated in the text also. 3. The TSES R2 taps both objective and subjective exposure, and again – these are completely 2 different types of exposures.

4. Statistical analysis – before I read this part, it was not clear to me that some peacekeepers participated in both missions. This is important information, and should be considered when writing the discussion.

Results
1. The response rate (p. 13) seems rather low. The authors explain this at the end, and attribute this RR to lack of response to one of the scales. If this is the case, they may want to consider more flexible treatment of this scale (i.e., that not all questions must be answered), or even better – implementing one of the modern methods for handling missing data, such as Multiple Imputation. This way, they will arrive at a larger, less biased, sample.

2. It would be nice to read some important descriptive information, such as – what was the most frequent stressor reported?

3. p.14 – 2nd paragraph: it is difficult to understand. Try re-phrasing by writing that "an association was found between X and Y", instead of writing about "a pattern of increased scores".

4. p.14 – final paragraph – Try avoiding mentioning the names of scales. Variable name are much easier to follow.

5. In general – a major shortcoming of the results section stems from the fact that most of the information is aggregated in tables, and so the text remains uninformative. The readers must run back and forth to the tables.
Discussion

1. Overall, the discussion is, to my opinion, not sufficiently deep in its contents. It mostly describes the results, but hardly offers any explanations to the findings (which are very interesting, and deserve explanations). I suggest the authors go over their findings, and attempt to explain them in light of previous studies.

2. Paragraph 2 (p.15) – what does it mean that "the association was consistent"? was the correlation size identical?

3. I suggest that the authors re-arrange the discussion so that they will first discuss findings regarding exposure, and then move on to those pertaining to the association between exposure and outcomes. Also, I suggest that the first paragraph of the discussion briefly summarize the findings.

4. p. 16, 2nd paragraph: what does "reported symptoms" refer to? What symptoms?

5. p. 16, 3rd paragraph: I would suggest another explanation for this finding, i.e., that peacekeepers were "inoculated" due to repeated exposure to stress (see Meichenbaum’s inoculation perspective).

6. p. 16 final paragraph: while the practical implications of these findings are very important, I would suggest moving them to the end of the ms.

7. The limitations paragraph is very well-written!

To sum, this paper deals with a very important topic and is based on large peacekeeping samples. The data here is of great potential, but I suggest the authors work on further refining the manuscript, and significantly deepen the discussion. I believe these efforts require a major compulsory revision of the ms.

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:

I declare that I have no competing interests'