Author's response to reviews

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

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Author's response to reviews: see over
Dear Dr Odenwald

Thank you for providing the reviewer comments to the manuscript ‘Traumatic events, other operational stressors and physical and mental health reported by Australian Defence Force personnel’. Please find a point by point response to the reviewers’ and editor’s comments appended to this letter. The reviewers’ comments are in bold.

I also provide an amended manuscript. If you have any questions about any of our responses please let me know.

Yours sincerely

Mr Michael Waller
Traumatic events, other operational stressors and physical and mental health reported by Australian Defence Force personnel following peacekeeping and war-like deployments
Michael Waller, Susan A Treloar, Malcolm R Sim, Alexander C McFarlane, Annabel C L McGuire, Jonathan Bleier, Annette J Dobson

Reviewer 1

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.

Response: The term ‘dose-response’ is not used in the amended manuscript due to the new set of results and the restructured discussion. The introduction has been changed to raise the possible connections between exposures and mental health outcomes (paragraph 4 of the introduction).

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.

Response: The introduction now includes a more detailed discussion of warlike stressors (paragraph 3 introduction)

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.

Response: The paper now considers subjective and objective traumatic exposures separately.

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.

Response: The last section of the introduction section has been modified to comply with this request.

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.

Response: Males and females did deploy to East Timor and Bougainville, however the overall ratio of females to males was low. Accordingly, separate analyses for female ADF members have not been included in this paper but adjustments for gender have been made in the analyses.

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.
Response: The East Timor sample was a stratified sample drawn from the full nominal roll of those deployed to East Timor. The sample was representative of the entire deployed group with regard to service, age, gender and permanent/reserve status. Therefore we believe that the comparison of results between the Bougainville group (drawn for the full deployed list) and East Timor group (drawn from a sample) is appropriate.

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.

Response: The measurement section has been revised to include more detail about each measure and the justification for each items inclusion. Additional information has also been included on the psychometric properties of the scales where that information is available. The analysis has been rerun to assess the objective and subjective traumatic exposures separately.
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.

Response: We have included this number in the first paragraph of the results section. There were 136 persons included in the study who deployed to both locations.

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.

Response: Table 3 shows that those who completed the questionnaire but did not complete the TSES-R had similar demographic characteristics to those who completed both the TSES-R and the health outcome questions. Therefore we believe that multiple imputation is unlikely to alter the overall conclusions presented.

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

Response: Descriptions of the most frequent stressors and traumatic events have been added to the results section.

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".
Response: This sentence is no longer included in the manuscript.

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

Response: This sentence is no longer included in the manuscript.

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.

Response: An attempt has been made to include a more detailed description of the tables in the results section.

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.

Response: The discussion has been expanded to include more explanations for the findings and to compare the results to previous studies.

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

Response: This sentence is no longer in the manuscript.

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.

Response: These suggested changes have been made.

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

Response: This sentence is no longer in the manuscript.

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).

Response: The explanation presented for the finding (that 'there are many enduring effects of traumatic events that the participants in the studies do not apparently register' ) was used to highlight that although participants reported that they were no longer affected by the traumatic events that occurred on deployment, some association between traumatic exposures and health outcomes was still observed. We have however amended the discussion to now mentioned the ‘inoculation’ hypothesis in the discussion of the objective stressors which were not shown to be clearly associated with the health outcomes (Discussion paragraph 4).

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.

Response: This paragraph has since been restructured.

7. The limitations paragraph is very well-written!

Reviewer 2
Major Compulsory Revisions

1. State how many of the personnel deployed to Bougainville also deployed to East Timor, This is an important omission from the paper.

Response: We have included this number in the first paragraph of the results section. There were 136 persons included in the study who deployed to both locations.

2. Please give more detail of results to support the statement that both groups reported being less affected by deployment experiences now than at the time of the event (not just a p value). Similarly for PTSD and psychological distress can number of cases be reported please for each deployment?

Response: The numbers who reported psychological distress and PTSD have been added to the first paragraph of the results section. The median scores on the TSES-R2-A scale for Bougainville and East Timor participants have been added to the third paragraph of the results section.

3. In the discussion modify the sentence that states “although the level of traumatic exposure was different, the association between level of exposure and scores on PCL, K10 and number of symptoms was consistent in both groups” to not include K10 for the Bougainville group.

Response: After reanalysis this section was removed from the discussion section.

Minor Essential Revisions
4. A very minor point - Is it peace-keeping (as in title) or peacekeeping (as elsewhere in paper)?

**Response:** The text has been changed to use the term ‘peacekeeping’ consistently.

5. Check the sample size for the East Timor group - is it 4002 (in Method) or 3999 as in Table 2?

**Response:** The text has been corrected to report 3999 as the sample size in the East Timor group consistently.

6. PCA: state which were the two items removed from subsequent analysis.

**Response:** This has been added to paragraph 3 of the results section.

**Discretionary Revisions**

7. What was the period of recruitment – start and end date?

**Response:** The period of recruitment November 2007 to January 2009 has been included in the second paragraph of the methods.

8. State in the methods that for the Traumatic Stress Exposure Scale the items were dichotomized before being summed.

**Response:** This was not the case. The text in the paragraph which describes the TSES-R scale has been modified to make this clearer.
9. Please explain why the 12 events of the Traumatic Stress Exposure Scale were dichotomized into never/ever whereas (factor) scores are used for the non traumatic exposures.

**Response:** Scale scores were used for the TSES-R (split into objective and subjective exposures) and the scores of these scales (quartiles) were used to compare health outcomes. No principal components analysis was used here because this scale only contained 12 items (and splitting of the scale into objective and subjective items was subsequently suggested by reviewer 1). A principal components analysis was used to reduce the 36 non-traumatic stressors into smaller groups of stressors so that we could consider different types of non-traumatic stressor separately. Counts of the stressors reported in each of the results factors were used to compare health outcomes between those who reported different numbers of non-traumatic stressors. We have amended the methods section to try to include the rationale for using the PCA for non-traumatic stressors (Methods paragraph 11).

10. Using the same categories for traumatic exposures in the two deployments would aid comparison – is this possible?

**Response:** The analysis has been redone using the same quartile cut-offs for each deployment.

11. In the second paragraph of the Results – presumably the second sentence should read something like: “Each traumatic event was reported with higher frequency in the East Timor group with the exception of ....”

**Response:** This suggested change has been made.

12. Table 3 eliminate the never columns to make it easier to compare the frequencies of exposures between the two operations.

**Response:** This suggested change has been made.
13. Table 4 I recommend removing the factor loadings from the table and reporting the non traumatic stressors in the same way as the traumatic stressors in Table 3 giving the number as well as %, grouping the stressors by the 4 factors.

Response: These changes have been made.

14. Tables 5 and 6 I would recommend removing the results for General Health and just refer to these results in the narrative. The column of results for Symptoms quotes Ratio of means – I am not familiar with this way of expressing the results of negative binomial regression – should this be a Rate Ratio (IRR in stata)?

Response: The general health results have been removed from the table as I agree that the table was very busy. It was difficult to discuss these general health results without having the results presented in a table so this section of analysis has been removed from the paper. In the results presented for the negative binomial regression the ‘ratio of means’ is the equivalent of IRR in the STATA output. We would prefer to keep the headings as they are currently presented because in the unadjusted models the output corresponds with mean number of symptoms in group of interest divided by the mean number of symptoms in the reference group. Multiple linear regression was used to investigate the association between quartiles of traumatic experiences and score on the K10 or on the PCL. Why was this approach rather than logistic regression with ‘caseness’ on K10 or on the PCL-C as outcomes?

Response: The analysis has been modified to use logistic regression for the outcomes of ‘caseness’ using the K10 and PCL-C.

Editors Comments
Additional comments by Dr. Odenwald: Please report more information on the 67-item list of the Australian Gulf War Veteran Health Study, e.g. how it was developed, reliability and validity. Please describe in more detail and report reliability and validity of the instruments K10, PCL-C, TSES-R2, list of 36 non-traumatic stressors, etc. Tables 5 and 6: Please explain better what coefficients are reported. Table 5: K10 - what coefficients are reported?

Response: We have expanded the measurements section to include more detail on the development of each of the scales. Conbach’s alpha has been reported for both the PCL-C and K10 scales. The coefficients in Tables 5 and 6 have now been changed to odds ratios.