

## PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Long-term responses to treatment in UK veterans with military-related PTSD: an observational study.
<b>AUTHORS</b>	Murphy, Dominic; Spencer-Harper, Lucy; Carson, Carron; Palmer, Emily; Hill, Kate; Sorfleet, Nicola; Wessely, Simon; Busuttil, Walter

### VERSION 1 - REVIEW

<b>REVIEWER</b>	Tony McHugh Melbourne University Melbourne Australia
<b>REVIEW RETURNED</b>	03-Apr-2016

<b>GENERAL COMMENTS</b>	<p>I think this is a highly worthwhile article that is very well-written. It would make a most welcome addition to the literature and it merits publication.</p> <p>I have no substantive criticism to make about it and only make small number of minor observations regarding each of the sections of the paper. On correction of these minor issues, the article is ready for publication.</p> <p>Introduction section</p> <p>There are no apparent theoretical or logical absences. Its arguments are well-made and easy to follow.</p> <p>p4</p> <p>para 2 sentences 3 and 4 need to be sharpened. They are a little unclear and potentially contradictory.</p> <p>para - line two (there is a superfluous "the" at the end of the sentence).</p> <p>p5</p> <p>para 2 - "treated for PTSD a year after treatment" makes no sense. I assume the former (treated) should be assessed.</p> <p>Method section</p> <p>Well described. Only minor revisions required.</p> <p>p5</p> <p>last para - verb confusion in 1st line ... are and were are used in</p>
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	<p>quick succession. In sentences later in para on that page there is verb inconsistency... were, would and were occur.</p> <p>p6</p> <p>para 1 - line 7 close cohort needs to be closed cohort.</p> <p>eight lines later "a four occupational therapist led session" could be better expressed.</p> <p>p7</p> <p>para 1 - line 11 psychiatry should be psychotropic. next line "on the same dose" would read better as that dose.</p> <p>p8</p> <p>"Interpreted more conservatively" I reckon I know what is meant, but to I suggest this is not the clearest way of putting it. Increasing the n in a denominator does not equate with a greater denominator score necessarily and the result could be a drop in conservatism. Could it not say interpreted by reference to the larger numbers of the initial population sample or something similar.</p> <p>Results section</p> <p>Well described. Only minor revisions required.</p> <p>p10</p> <p>para 3 - line 3. "As expected" is not appropriate surely - there has been no prior justification for it. Again I know what is meant (military folk tend to be male) - a better way of saying this needs to be found.</p> <p>In brackets four lines lower insert "they had" (they had been medically discharged).</p> <p>p11</p> <p>2nd last line re "any basis may only have had a modest impact". Curious grammar and bias is bias - can it have a causal impact (no need to assert this, only note the bias).</p> <p>p10</p> <p>Discussion section</p> <p>There are no apparent theoretical or logical absences in the discussion. Its arguments are well-made and easy to follow, however, more could be made of some (see below).</p> <p>p 12 Line 3 insert treatment before "programme".</p> <p>I think more could be made of the alcohol findings - these are not findings that many C-R PTSD treatment outcome studies necessarily show - and why they have occurred. This especially so in light of the low audit scores obtained.</p> <p>I also think the mood comorbidities non-result requires comment. There is a robust literature pointing to the impact of the same on</p>
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	<p>PTSD. That it was not found here needs some exploration.</p> <p>I think the last sentence of the para that begins on p 13 (i.e., that immediately pre the strengths and weaknesses section) is superfluous. I also think the matter addressed in last two sentence of the 1st para are neither an indication of a strength or weakness and should be mentioned in the methods section before this and its utility commented upon earlier in the discussion for its implications.</p> <p>The CAP-5 reference on p 16 should of course be altered to the CAPS-5</p> <p>The concluding para is fine, but the importance of its ideas for future research would be well emphasised.</p> <p>Conclusions section</p> <p>Line 2 word two should be than not that</p> <p>General comments</p> <p>There are a number of grammatical questions that may or may not require attention depending on the view of the journal editors. These include the use of:</p> <ul style="list-style-type: none"> <li>• personal pronouns</li> <li>• % symbols outside brackets</li> <li>• numbers less than ten requiring words &amp;</li> <li>• numbers and not words to commence sentences.</li> </ul> <p>Common usage suggest they should not be used.</p>
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<b>REVIEWER</b>	Mitzy Kennis Researcher at University Medical Center Utrecht / Teacher at Utrecht University, the Netherlands
<b>REVIEW RETURNED</b>	10-May-2016

<b>GENERAL COMMENTS</b>	<p>This is an interesting article describing long-term treatment outcomes (1 year follow up) of an intensive treatment program, offered to PTSD patients after 'treatment as usual'. Although it is not an RCT, the results are promising and it is shown that intensive treatment can further reduce symptom severity in (chronic) PTSD. The sample is large and the analyses are suitable. I have some minor suggestions for improvement.</p> <ol style="list-style-type: none"> <li>1. The first time Combat Stress (CS) is mentioned, it would be useful to add a brief explanation about the organisation, and when/how people are admitted.</li> <li>2. It is somewhat unclear that the term responders and non-responders are utilised (based on having filled out questionnaires at follow up), while "treatment response" is also investigated. It would be helpful to check the usage of these two "responders" and perhaps better clarify this throughout the manuscript.</li> <li>3. It would be helpful to give a clinical explanation of the effect (and effect size) of symptom reduction observed. Is 11 points on the PSS normally defined as treatment response for example (clinical</li> </ol>
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	<p>improvement) or is it a marginal symptom severity change?</p> <p>4. Although using a random slope non-linear growth model seems suitable, it is somewhat unclear why time squared is taken into account in the statistical model. Can the authors explain choosing this analysis, and add this to the method section?</p>
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## VERSION 1 – AUTHOR RESPONSE

Reviewer: One

We would like to start by thanking reviewer one for their comments and have incorporated all of their suggested changes to improve the clarity and flow of the manuscript. As suggested we have modified the text to take into account reviewer one's comments throughout the introduction, methods, results and discussion. The changes have been made in red. We have not gone through these minor changes individually within this letter for the sake of clarity.

Below we have discussed two of reviewer one's comments and their general comments in more detail.

I think more could be made of the alcohol findings - these are not findings that many C-R PTSD treatment outcome studies necessarily show - and why they have occurred. This is especially so in light of the low audit scores obtained.

One of our exclusion criteria was that participants could not be actively dependent on alcohol prior to starting treatment. As such, this may explain the lower than expected audit scores. We have added an extra sentence in the discussion to further discuss the role of alcohol difficulties post-treatment.

I also think the mood co-morbidities non-result requires comment. There is a robust literature pointing to the impact of the same on PTSD. That it was not found here needs some exploration.

We have added a sentence to the discussion on page 13 discuss this non-finding further and describe how we had previously report that pre-treatment co-morbidities predicted 6 month treatment outcomes in a similar way to previous findings.

General comments by reviewer one

There are a number of grammatical questions that may or may not require attention depending on the view of the journal editors. These include the use of:

- Personal pronouns

Whilst we agree with the reviewer about the common usage the use of personal pronouns fit with the authors writing style and would request to keep these in.

- % symbols outside brackets

We have added brackets to % symbols where possible and used the word 'percent' instead of the symbol where this was not possible

- numbers less than ten requiring words

We have reviewed the manuscript and removed any occurrences when numbers less than ten had not been written as words.

- numbers and not words to commence sentences.

We have reviewed the manuscript and modified any sentences where numbers were used instead of words to start sentences.

Reviewer: 2

We would also like to take this opportunity to thank reviewer two for their comments to improve the manuscript. Four comments were raised by reviewer two:

1. The first time Combat Stress (CS) is mentioned, it would be useful to add a brief explanation about the organisation, and when/how people are admitted.

An additional sentence has been added to the introduction to describe Combat Stress and there is information with the methods to further elaborate.

2. It is somewhat unclear that the term responders and non-responders are utilised (based on having filled out questionnaires at follow up), while "treatment response" is also investigated. It would be helpful to check the usage of these two "responders" and perhaps better clarify this throughout the manuscript.

As suggested the terms responder/non-responder have been removed from the manuscript and the terms 'followed up or not' or 'lost to follow-up' have been used instead to improve the clarity of the study.

3. It would be helpful to give a clinical explanation of the effect (and effect size) of symptom reduction observed. Is 11 points on the PSS normally defined as treatment response for example (clinical improvement) or is it a marginal symptom severity change?

As a clinician myself, the gains we are finding appear to be clinically significant, but it is hard to find published empirical evidence to back this up. As such, we do not feel able to comment the clinical significance of the gains found within this study. We have added a sentence to the discussion to compare the reported effect size to Australian and US studies of veterans with PTSD.

4. Although using a random slope non-linear growth model seems suitable, it is somewhat unclear why time squared is taken into account in the statistical model. Can the authors explain choosing this analysis, and add this to the method section?

An additional sentence has been added to the analysis section within the methods to provide a rationale for why a fixed coefficient of time squared was used.

We hope that we have been able to address the reviewers' comments with the changes we have made to the manuscript and that the paper is now ready to be accepted for publication.

## VERSION 2 – REVIEW

<b>REVIEWER</b>	Tony McHugh Melbourne University Melbourne Australia
<b>REVIEW RETURNED</b>	30-May-2016

<b>GENERAL COMMENTS</b>	<p>The authors have appropriately addressed the comments I made on the initial draft. As stated it is a commendable draft article and I can, therefore, see no reason why it should not be published in its amended form.</p> <p>Please note that this review template asked for comment on whether specialist statistical review is required. I am not in a position time or expertise-wise to consider this question. I have on this basis ticked no, but the editorial panel would do well to follow the comments of reviewer two who appears to have a greater level of knowledge about the primary analytical technique applied.</p>
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<b>REVIEWER</b>	Mitzy Kennis Teacher at Utrecht University / Researcher at University Medical Center Utrecht, the Netherlands
<b>REVIEW RETURNED</b>	13-May-2016

<b>GENERAL COMMENTS</b>	The authors have addressed my comments and I advise accepting the manuscript for publication in its current form in BMJ open.
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