Author's response to reviews

Title: Risk Factors for Posttraumatic Stress Disorder Symptoms Among Deployed US Male Marines

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Version: 2 Date: 22 January 2010

Author's response to reviews: see over
22 January 2010

Melissa Norton, MD
BMC Psychiatry, Editor-in-Chief
236 Gray's Inn Road
London WC1X 8HL, UK

Dear Dr. Norton & Editors,

Thank you for your patience. Enclosed please find our revised manuscript, “Risk Factors for Posttraumatic Stress Disorder Symptoms Among Deployed US Male Marines”. We have addressed each of the reviewers’ comments and incorporated revisions into the manuscript.

The authors appreciate the opportunity to respond to the reviewers’ comments, and all agree that this revision has improved the clarity and content compared to the original manuscript. The revised version of the manuscript has been submitted. Our responses to each of the reviewers’ comments (in italics) follow below:

**Reviewer's report**

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

**Version:** 1 **Date:** 16 September 2009

**Reviewer:** Thomas Ehring

**Reviewer's report:**

*The manuscript describes results of a two-wave prospective study investigating predictors of PTSD in US Marines deployed to Iraq. The manuscript is generally well-written. In addition, the relatively large sample size is a clear strength. However, the use of potentially unreliable measurements complicates the interpretation of the findings. In addition, I think that the authors should try to make a stronger case for the relevance of the study and its contribution to the literature.*

**Major compulsory revisions**

(1) *The three main predictors of PTSD investigated in the ms are severity of trauma exposure, perceived threat to life and social support. All three are well-known*
predictors of the disorder as shown by a large number of studies in military and civilian populations and confirmed in meta-analyses (Brewin et al., 2000; Ozer et al., 2002). I therefore think that the current study mainly replicates well-known findings rather than adding a truly new piece of information to the literature. I am wondering whether the authors would agree with this statement. If they do agree, I suggest that they acknowledge this more clearly in the abstract and throughout the manuscript. If they don’t, it would be good to provide some more information on how this study truly extends earlier research.

Author’s Response:

This was the first of three articles we wanted to write with our study cohort, but based on the insightful reviews, we have enlarged the current effort and included more covariates.

We thought it was courageous for Brewin et al. to proceed with the meta-analysis given the extreme heterogeneity among the available studies, even after inclusion criteria were applied. As Brewin discussed “…the meta-analysis succeeded in identifying weak effects of pretrauma risk factors…” and “Very few studies have examined other predictive factors measured pretrauma.”. Only two studies were cited, from 1993 and 1998. With the expanded scope of this article we now include many pretrauma assessments from RAP I. This work confirms Brewin’s alternate interpretation of his own model, i.e., “The data may be regarded as consistent …with a model in which pretrauma factors interact with trauma severity or trauma responses to increase the risk of PTSD” and expands the literature for examination of pretrauma risk factors. We have extended/enhanced earlier research (now) by the revision of the analysis with pretrauma factors.

The addition of prior violence episodes to the analysis and their contribution in the reduced regression model, we are to support and discuss the concept of traumatic load as well.

(2) I see the fact that most predictor variables (e.g., social support, perceived life threat) were assessed with single items of unknown psychometric properties as the main limitation of the study. In my opinion, this should be clearly acknowledged as a limitation and receive some attention in the discussion.

Author’s Response:

We acknowledge this limitation and have added it to the discussion.

(3) I am wondering whether data was collected anonymously. If not, some discussion on how this may have influenced participants’ responses to the different measures appears warranted.

Author’s Response:

The data was not collected anonymously. Participants provided written consent prior to participation. Although, as described in the consent form, survey responses were stored in conjunction with a subject identification number known only to the subject and study staff, we understand that survey questions were sensitive and participants could be less forthcoming, particularly in regards to questions about home life, to include adverse
childhood experiences and prior trauma, and PTSD assessment. This limitation has been added to the discussion.

(4) Results of past studies have suggested that retrospective self-report of combat exposure may be rather unreliable and influenced by current PTSD symptom levels (e.g., Roemer et al., 1998; Southwick et al., 1997). The authors may want to take these findings into consideration when discussing the results of their study.

Author’s Response:

Our median gap time (time between return from last deployment and follow-up survey) was approximately 2 ½ months (76 days). Roemer’s average “gap” between the surveys was 21 months, almost 10 times longer. Roemer’s Time 1 survey was the gold standard for comparison to Time 2 and the change in recall of combat exposures were associated with the posttraumatic symptomatology. The timing of PTSD assessment for this study aligns with Roemer’s Time 1 survey. Roemer’s findings appear valid, but we do not believe apply as strongly for this study cohort.

Minor compulsory revision

(5) The dependent variable is a diagnosis of PTSD established on the basis of participants’ responses to the PCL-C. I therefore think that the term ‘PTSD symptoms’ in the title, tables and throughout the manuscript is misleading and should be replaced with the term ‘diagnosis of PTSD’. On a side note, I am wondering whether the results stay the same if a multiple regression analysis with the continuous PTSD symptom scores was used instead of a logistic regression with a categorical outcome variable.
Author's Response:

The PCL-C is a screening instrument for PTSD symptoms and used alone cannot diagnose PTSD. Those with a positive screen have a higher likelihood of being diagnosed by a credentialed mental health provider after a clinical encounter. Within the military, anyone returning from deployment, (usually assessed 3-6 months after return) who has a positive PCL-C screen is referred for further mental health evaluation.

When scoring the PCL-C, participants met the criteria for PTSD symptoms if they had a total score of 50 or more on a scale of 17 to 85 points, and reported a moderate or above level of at least one intrusion symptom, three avoidance symptoms, and two hyperarousal symptoms. From the original paper below by Weathers et al, the sensitivity and specificity for the probability of a PTSD diagnosis was 0.82 and 0.83 respectively.


Multiple regression analyses are planned for future studies, but are not a component of this paper.

(6) In the abstract, the study aim stated (“determine significant exposures associated with PTSD”) is not entirely consistent with the results reported later in the abstract (perceived life threat and social support as predictors). Please make this more consistent.

Author's Response:

We have rewritten the abstract to improve internal consistency.

(7) In some participants, PTSD symptoms assessed may be related to traumatic events experienced before deployment. Were these events assessed at all? If yes, please include this variable in the analyses.

Author's Response:

The initial survey instrument assessed adverse childhood experiences (ACE) and trauma history questions, both of which provide insight into predeployment traumas. We agree with the reviewer on the importance of assessing predeployment traumas, so we have revised the manuscript and re-run the model including an ACE score and a trauma history score (see text).

The prior violence exposure score remained as a significant risk factor in the final reduced model.
(8) It would be good to have some indication as to how well the final model predicts PTSD. Please provide some goodness-of-fit data and/or percent of participants correctly classified.

Author’s Response:

A predictive model was not our analysis goal, however, the sensitivity and specificity for the final model were both 75%. The c-statistic or “area under the curve” was 0.835.

SAS Summary:

The SURVEYLOGISTIC Procedure

Association of Predicted Probabilities and Observed Responses

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References


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.

Author’s Response:

The reviewer brings up some good points. There are many identical questions/question sets in the baseline RAP and follow-up RAPII questionnaires. For example, the CAGE questions were part of both questionnaires, but many questions, i.e. the PCL-C, were not included in both instruments.

The same social support question, however, is included in both questionnaires (“How many close friends or relatives do you have you can call on for help or talk to about personal problems?”). We had planned to do further research studying the change in questions/measures, but agree that the inclusion of the social support question from RAP would be helpful to more thoroughly examine the relationship between social support and PTSD. We have included social support from the baseline questionnaire in the full saturated model.

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.

Author’s Response:

Seven of the questions described by Felliti et al {Relationship of childhood abuse and household dysfunction to many of the leading causes of death in adults: the Adverse Childhood Experiences (ACE) Study. Am J Prev Med. 1998;14:245-258} for the ACE score are included as part of the RAP questionnaire. Based on these 7 questions, we created our own ACE score (see text) and included it in the full saturated model.

Some prior violence questions are asked in the RAP questionnaire, but since the RAP/RAPII instruments are only administered to Marine recruits at the point of entering military service, by design trauma questions specific to combat are not included in the baseline assessment. We created our own prior violence score (see text) and included it in the full saturated model. It remained as a significant risk factor in the final reduced model.
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?

Author’s Response:

Earlier in our analysis we did include an exposure combat score, a simple sum of the positive endorsements. However, we were concerned about adding “apples to oranges”, i.e. could the sum be valid when the questions included life threatening scenarios vs. taking PB pills which were supposed to be taken preventively and periodically. For this reason, we decided not to include the combat summary score in the original manuscript.

Based on these reviews, we have revisited a combat exposure summary score as a sum of the first six questions omitting, wearing a gas mask/MOPP gear, using a Mark-1 Kit, and taking PB pills. Neither method for these summed combat exposure scores were significant in the fully saturated models and were eliminated early in the manual backward stepwise regression. In univariate analysis there is a graded, positive association for three categories of the combat summary score, but we believe that this summary score is a deficient measure as it does not truly represent the actual number of lifetime traumatic exposures/events or as the reviewer has pointed out, or a severity associated with each distinct event.

Unfortunately, our data does not have the granularity to sum the entire number of exposure events, only the number of different types of traumatic events, however, the number of different types, as they increase, is in line with the concept of a “building block” fear network, which we suggest in the discussion.

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)?

Author’s Response:

This was a manual backward stepwise logistic regression. We have added the word “stepwise” to the methods. Covariates were removed one at a time starting with the least significant. If the lead covariate (“did you feel in great danger of being killed”) changed by more then 15% (the odds ratio or beta coefficient) the recently removed covariate returned to the model as a likely confounder and the next least significant covariate was challenged.

The regression included the weights developed as we described to adjust for non-response bias.

A predictive model was not part of our analysis goal, however, the sensitivity and specificity for the final model were both 75%. The c-statistic or “area under the curve” was 0.835

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.

Author’s Response:

We concur that these are interesting discussion points but we believe this level of in depth discussion is beyond the scope of the current study and better suited to a systematic review. Based on the major revisions above, we have added the social support assessment, measures of ACE, and prior violence exposures from the baseline RAP questionnaire, which were mentioned in the introduction and controlled for in the regression model.
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.

Author’s Response:

We appreciate the Reviewer’s suggestion. This comparison, as a discussion point, has been added to the revised manuscript. Those Marines with two deployments did indeed have a greater “sum” of combat exposure types.

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?

Author’s Response:

There are other issues regarding how well combat exposures are documented in our current study. Since the odds for a positive PCL-C screen are relatively high for combat exposures, we would like to see better documentation and screening for combat exposures as part of the assessment when a Marine returns from deployment. We have added this to the discussion.

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.

Author’s Response:

We have deleted what the reviewer described as speculation re external validity from the discussion section and thus considerably shortened the paragraph. The discussion point, that a dose response may be present, we would still prefer to leave for the reader to consider.
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.

Author’s Response:

With the addition of the social support assessment from RAP, we realize, and attempted to point out in the original paper, a potential “chicken first or egg first” concept here. But it’s more likely that the prior association found merely reflects the expression of PTSD, i.e. avoidance. In the updated analysis and revised manuscript, 0-2 number of close friends as a covariate is no longer significant. More detailed work with an assessment of the social support network at pre, during, and immediate post deployment would be required to explore the notion of a causal relationship, or if a stable large support system helps mitigate development of PTSD.

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.

Author’s Response:

We appreciate the Reviewer’s comment and have added the median PCL-C scores to the results section.

6. The responding rate (13%) is relatively low, might there be any reasons for that?

Author’s Response:

There are several factors that contributed to the relatively low response rate for this study. Marines, especially young Marines, have the greatest rate of deployment compared to all other service branches, which make this population highly mobile and difficult to track. Aside from deployments, Marines rarely can afford to set down roots and frequently do not have up to date mail addresses on file. We were aware of this and included weighting to compensate for any potential non-response bias. The survey instruments were not anonymous, possibly inducing a reticence to respond. We have added this limitation to the discussion.
7. **Check the manuscript for abbreviations, e.g. I was not able to find an explanation for the abbreviation NCO in the manuscript**

**Author’s Response:**

Although we used “Noncommissioned officer” in the tables without the acronym, we neglected to do so in the body of the manuscript. We have corrected this and appreciate the Reviewer identifying this omission.

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).**

**Author’s Response:**

We have added the phrase “…higher ranking noncommissioned officers…” to the discussion to help avoid confusion regarding group status.
Reviewer's report

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

Version: 1 Date: 5 October 2009

Reviewer: Birgit Kleim

Reviewer's report:

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.

Author's Response (1 & 2): Similar to the reviewer, we question whether wearing a gas mask, using nerve agent antidote, or taking PB pills to protect against nerve agent exposure may influence peritraumatic processing. However, the follow-up survey did not contain quantitative measures, such as the Peritraumatic Dissociative Experiences Questionnaire or the Dissociative Experiences Scale, making such an assessment beyond the scope of this study. From other projects/research we are engaged in, we believe there are likely racial biases regarding compliance with taking PB pills, and that the three exposures may not be perceived as life threatening compared to engagement in combat or encounters with improvised explosive devices (IEDs). In the revised analyses, we created an exposure summary score based on six combat exposures, without using wearing a gas mask, using nerve agent antidote, or taking PB pills questions. The summary of combat exposure types had a graded, positive univariate association, but it did not persist in the final reduced logistic model.

The reviewer notes an important point regarding peritraumatic factors and processing, but we cannot adequately discuss peritraumatic processing; we have no objective measures on our current survey instruments that measure such factors. Interest in resilience or resistance to PTSD development is quite high especially within the DoD. From this study cohort, however, we can only report that noncommissioned officer rank, fewer
deployments and those without prior history of violence exposure were less likely to develop PTSD symptoms. These three concepts are included in the discussion.

(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 realize 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.

Author’s Response:

We agree, in part, regarding fear of stigma and lower intelligence. Survey questions were sensitive and subjects could have been hesitant to positively endorse questions about home life, to include adverse childhood experiences and prior trauma, and PTSD symptoms. This limitation has been added to the discussion.

As the population for this study was Marines with deployment experience, by definition it could only include RAP participants who completed boot camp, because only those who successfully complete boot camp can became Marines, thus ensuring they had an opportunity to deploy.

(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.

Author’s Response:

The reviewer is correct, we did not assess for the degree of stress. Different individuals could have experienced the same combat exposure and yet have had varied perceptions of distress to the event. We have added this limitation to the discussion.

Nearly all the combat exposure questions were dichotomous, however the “great danger of being killed” question did have frequency assessment, which was collapsed to a yes/no response. We plan to evaluate this in later work with generalized estimating equations to fit an ordinal model for multinomial data.
(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.

Author’s Response:

The reviewer makes a good point, and we attempted to address this point in the original version of the manuscript by including gap time (page 5). Gap time was calculated as the number of days between the last deployment and completion of the follow-up survey (RAPII). The median gap time was 76 days. Gap time was included in the fully saturated model but was not significant. Gap time can be found in Tables 1 and 2.

(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.

Author’s Response:

We agree and have added text to briefly describe the Dillman procedures (page 5). Briefly, the Dillman et al. described a survey procedure that includes at least four separate mailings, timed for maximum response effect.

First: To all members of the sample—a personalized, advance notice letter. Its purpose is to tell people they have been selected for the survey and they will be receiving a questionnaire

Second: About one week later, again to all members of the sample—a personalized cover letter with more detail on the survey, a questionnaire, and stamped return envelope

Third: Four to eight days after the questionnaire goes out, again to all members of the sample—a follow-up letter thanking those who have responded and requesting a response from those who have not

Fourth: Three weeks after the first questionnaire goes out—a new personalized cover letter contacting people who have not yet responded, with a replacement questionnaire and stamped return envelope.

(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?
Author’s Response:

The interaction term was nonsignificant and was removed from the saturated model during stepwise regression. See original manuscript, page 8.

(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?

Author’s Response:

We do describe that the PCL-C is “self report” in the first sentence, page 6 of the original manuscript.

The PCL-C is a screening instrument for PTSD symptoms. Those with a positive screen have a higher likelihood of being diagnosed by a credentialed mental health provider after a clinical encounter, and there are those with positive PCL-C screenings who do not warrant a clinical diagnosis. Within the military, anyone returning from deployment, (usually assessed 3-6 months after return) who has a positive PCL-C screen is referred for further mental health evaluation.

When scoring the PCL-C, participants met the criteria for PTSD symptoms if they had a total score of 50 or more on a scale of 17 to 85 points, and participants had to report a moderate or above level of at least one intrusion symptom, three avoidance symptoms, and two hyperarousal symptoms. From the original paper by Weathers et al, using only the 50 point cut off score, the sensitivity and specificity for the probability of a PTSD diagnosis was 0.82 and 0.83 respectively.


We are not familiar with interference ratings – if this was a reference to work with pain interference scales, we did not measure that in our RAP surveys.

(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.
Author’s Response:

We appreciate that the reviewer noted our discussion of this dose-response for a given dimension.

Earlier in our analysis we did include an exposure combat score, a simple sum of the positive endorsements. However, we were concerned about adding “apples to oranges”, i.e. could the sum be valid when the questions included life threatening scenarios vs. taking PB pills which were supposed to be taken preventively and periodically. We decided not to include it.

Based on these reviews, we have revisited a combat exposure summary score as a sum of the first six questions omitting, wearing a gas mask/MOPP gear, using a Mark-1 Kit, and taking PB pills. Neither method for these summed combat exposure scores were significant in the fully saturated models and were eliminated early in the manual backward stepwise regression. In univariate analysis there is a graded, positive association for three categories of the combat summary score, but we still believe that this summary score is a deficient measure as it does not truly represent the actual number of traumatic exposures/events or as the reviewer has pointed out, a severity associated with each distinct event.

(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?

Author’s Response:

This is a good question, but we do not believe that this is an issue. Advancement to higher rank is more rapid within the lower rank tier, E1-E3.

In the last 10 years, Marines were deployed at least 50% of their careers. Higher ranking Marines are likely to have had multiple and recent deployments, but only be a few years older than those in the ranks below, who are also deploying.

(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.
Author’s Response:

We agree that further dissection of social support would be valuable, but as the reviewer realized, we have no data for these specific questions. With our addition of baseline social support to the analysis, we believe it is more likely that the change in social support as measured by number of close friends at follow-up is more likely a consequence PTSD and by definition, exhibition of avoidance.

(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.

Author’s Response:

Again, as with a similar suggestion from Reviewer 2, we agree. There are other issues regarding how well combat exposures are documented. Since the odds for a positive PCL-C screen are relatively high for combat exposures, we’d like to see better documentation and screening for combat exposures and pre deployment exposures to violence as part of the assessment when a soldier returns from deployment. We have added this to the discussion.

Respectfully,

Lt Col Christopher J. Phillips, MD, MPH

DoD Center for Deployment Health Research