Author's response to reviews

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

Authors:

Christopher J Phillips (chris.phillips@med.navy.mil)
Cynthia A LeardMann (cynthia.leardmann@med.navy.mil)
Gia R Gumbs (gia.gumbs@med.navy.mil)
Besa Smith (besa.smith@med.navy.mil)

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Author's response to reviews: see over
Dear Dr. Norton & Editors,

Please see our attached document, a second revision of the manuscript, “Risk Factors for Posttraumatic Stress Disorder Among Deployed US Male Marines”. We have addressed each of the reviewers’ and Associate Editor’s additional comments and incorporated revisions into the manuscript.

The authors appreciate this follow up opportunity to respond to the reviewers’ comments and suggestions. Our responses to each of the reviewers’ comments (in italics) follow below:

Reviewer 1

(1) Most predictor variables were assessed with single items of unknown psychometric properties (see also my second comment to the original manuscript). I still think that this is a serious limitation of the study that should be clearly acknowledged and receive some attention in the discussion. Although the authors write in their response to the reviewers that they have added this limitation to the discussion, I can’t find a passage about this issue in the manuscript.

Author’s Response:

We have added these sentences below to the discussion and hope it will satisfy the reviewer and be more transparent.

“Another limitation of this study is that we did not have the available data to develop measurement scales that meet the standards of psychometric principles, nor were our deployment exposure variables based on an established instrument with proven psychometric properties. For example, we did not have (1) multiple waves of data to test for the reliability of our measures over time (test-retest reliability); (2) the benefit of a large number of measures tapping the same latent construct to develop scales that were internally consistent; nor (3) any data to validate our exposure measures against established instruments with proven psychometric properties. However, our use of single
item exposure variables did reflect systematic variation across the study cohort and yielded meaningful results."

(2) I still think that the term ‘PTSD symptoms’ in the title, tables and throughout the manuscript is misleading. For example, in the abstract the authors write that “10.8% screened positive for postdeployment PTSD symptoms”. I assume that more than 10% of the sample had PTSD symptoms of some sort. In addition, the term does not accurately capture the dependent variable in the regression analyses, which was not a continuous PTSD symptom severity score but PTSD group status established by a PCL cutoff. I appreciate the authors’ argument that the PCL does not allow to formally diagnose PTSD. However, I am wondering whether a term such as ‘probable PTSD’ or ‘clinically-relevant PTSD symptoms’ would be more appropriate than simply ‘PTSD symptoms’.

Author’s Response:

We realize that the wording “PTSD symptoms” may create confusion, but as the reviewer acknowledged this study screens for ‘probable PTSD’ and does not diagnosis PTSD. It is true, more than 10% of the study cohort had at least one PTSD symptom. We have wrestled with this nomenclature in other papers we read and write as well. We have changed some of the language to indicate that the study screens participants for PTSD and after the initial mention we have used the wording “PTSD”. Furthermore, since we go into great detail describing the PCL-C scoring system, we have added the phrase “potentially clinically relevant PTSD symptoms” to page 7 under the Posttraumatic Stress Disorder Assessment section.

To be consistent, we have changed the title as well to read: “Risk Factors for Posttraumatic Stress Disorder Among Deployed US Male Marines”.

(3) Responding to my earlier suggestion to report goodness-of-fit data, the authors provide this information in their response to reviewers. Please also add this information to the manuscript.

Author’s Response:

The Hosmer-Lemeshow results are available from PROC LOGISTIC in SAS, but we used PROC SURVEYLOGISTIC with non response weighting and Hosmer-Lemeshow test is not available. Some readers may not concur with using Hosmer-Lemeshow from PROC LOGISTIC in this setting, so model fit as measured by R2 (0.851 for the reduced model) has been added as a note at the bottom of Table 3, the reduced model results. We have seen this method of citation for the goodness-of-fit tests in other BMC published articles and believe this to be an appropriate location.

(4) As detailed in my first comment on the original manuscript, I think that the current study mainly replicates well-known findings rather than adding a truly new piece of information to the literature. The authors’ response to my earlier
Author’s Response:

While we agree that some of our results support previous findings, we strongly feel that this study does add new information to the current literature. This study is one of the first prospective studies that evaluates some potential risk factors for PTSD during recruit training, prior to any exposure to combat. This study specifically examines young male Marines who have completed a survey prior to their first deployment (in support of the operations in Iraq and Afghanistan) and another survey following deployment.

This penultimate sentence has been added to the Background Section:

“Rarely have young Marines early in their military career been available for detailed study with the ascertainment of preservice and predeployment risk factors and post deployment exposures.”

This thought is tied to these new sentences in the Discussion Section:

“We are unaware of any recent studies with young Marines, at the earliest phases of the military experience, able to examine the relationship among prior violence exposures as young adults prior to military induction and their post recruit exposures to combat violence in their first or second deployments…synergistically enhancing the risk to develop PTSD, and for some, the clinically relevant disorder.”

For those clinicians and therapists reading this article, we still feel strongly that the last sentence is the most relevant, and encourages them to elicit histories for the experiences prior to combat exposure.

Reviewer 3

Odds ratios are reported for each of the variables, but significance levels are missing in Table 1 and 2.

Author’s Response:

Table 1 contains only counts and percentages from the study cohort by baseline and follow-up characteristics

In Table 2, for those confidence intervals which include 1 (and are bold in table), they have achieved significance at p values less than 0.05.
I wonder whether the interaction of predictors has been considered in the regression analyses predicting PTSD, i.e., the interaction between experience of prior violence and exposure to specific combat experiences.

Author’s Response:

It has been suggested that early trauma does interact with level of combat exposure to increase risk for PTSD (King, King, Foy, and Gudanowski 1996). We are interested in this as well and plan to explore in future analyses; however, when we evaluated these six additional interaction terms with the Bonferroni correction for multiple comparisons, the interaction terms were not significant at the start with the saturated model.

Associate Editor’s report

Right now, the reader has two different models to choose from, one summarized in Table 2 and one in Table 3, but basically no way to decide which is better (in addition, it is unclear to me exactly where in the "Statistical Analysis" section we reach the model that is reported in Table 2). I suggest the authors select a single model, detail the rationale leading to this model, and only present and discuss the findings from this model.

Author’s Response:

Table 2 is a collection of the univariate results for each characteristic listed and the outcome, a positive screen for PTSD symptoms. We did describe this twice in the manuscript, page 11, first full paragraph:

“…in the univariate analysis,...” - and - “In additional univariate analysis…”

I have changed the first phrase to read “…in the univariate analysis (Table 2),…” to help remind the reader that Table 2 contains the univariate results.

We described in detail that a saturated model started with all the characteristics listed in table 2, plus an interaction term we believed had plausibility. See page 9, Statistical Analysis “The saturated model included age,…”

From this saturated model we derived the final model described in Table 3 and this final model with adjustments for all the remaining covariates was the basis for the discussion section.

In addition, the sentence "The final multivariable model was adjusted for the remaining deployment exposures and other covariates that were significantly associated with the outcome, or confounded the relationship between 'felt in great danger of being killed' and PTSD by more than 15%." is unclear to me.
Author’s Response:

We concur, it is awkward. Our method is a manual backward regression. Once a covariate is removed, if the newly adjusted odds ratio for the primary or lead variable (in this case “felt in great danger of being killed”) changes by more than 15%, we consider the covariate just removed a potential confounder of the relationship between the lead variable and outcome; we then replace that suspect covariate before we resume the manual backward regression modeling. For this analysis in this paper, we did not find any confounders, so it would be an improvement to replace the sentence with:

“Nonsignificant variables were manually removed one at a time from the multivariable model if they did not confound the relationship between “felt in great danger of being killed” and PTSD by more than 15%. The final multivariable model was adjusted for the remaining deployment exposures and included only those covariates that were significantly associated with the outcome.”

Finally, the authors might want to include the following article for the building block effect, which summarizes the effects of traumatic load on PTSD development, PTSD severity and remission from PTSD even better than Kolassa & Elbert (2007).


Author’s Response:

We concur and have replaced Kolassa & Ebert (2007) with the latest Kolassa article as suggested.

All the authors wish to thank the effort and the reviewers for their efforts to improve the manuscript. It is truly our best effort to date.

Respectfully,

Lt Col Christopher J. Phillips, MD, MPH

DoD Center for Deployment Health Research