Author's response to reviews

Title: Do adverse childhood experiences increase the risk of postdeployment posttraumatic stress disorder in US Marines?

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Author's response to reviews: see over
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Melissa Norton, MD
Editor-in-Chief
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Dear Editor Norton,

Enclosed please find our revised manuscript titled, “Do adverse childhood experiences increase the risk of postdeployment posttraumatic stress disorder in US Marines?”

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

**Reviewer 1:**

1. Despite the obvious interest of PTSD and the size of the sample (8391) the report has significant limitations that in the end represents a small advance to deserve publication. Understandably only males are included, the follow up period is not long enough. Being deployed is considered equivalent to combat exposure. Adverse childhood experiences are self reported at the beginning of recruit training (a more directive search might have changed the results) Even though information from medical records are a objective outcome measure it seems very surprising that ICD-9 is the classification used having available ICD-10 for more then 15 years.

The reviewer notes important points regarding the limitations of our paper, however we have stated these limitations in the appropriate section in the discussion and feel that our results are still valid. As for the codes for medical diagnoses, the US military healthcare system still uses ICD-9 codes. From our understanding, most US military systems will start using ICD-10 codes in 2013.

**Reviewer 2:**

1. It appears as though the authors used data collected as part of a larger effort to examine predeployment factors predicting post deployment mental disorders (in the discussion, the authors cite a previously published study of ACE in relation to alcohol misuse). If so, this might be better clarified earlier in the text (i.e., describe the goals of the over-arching study).
The reviewers make a good point and we understand that we did not clearly describe the Recruit Assessment Program (RAP). We have expanded the introduction to explain the main goals of RAP.

2. The introduction may benefit from several additional sentences elaborating on the context of the current study relative to the larger literature on ACE and adult onset PTSD. The authors do not immediately introduce the term 'Adverse childhood event' (not until they describe the findings of Sacco et al. paper) and readers may benefit from a brief sentence to operationalize the term (i.e., defining what constitutes - and does not constitute - ace). Also, it may help to clarify for the reader the theoretical reasons why ACE might predict adult onset problems (e.g., associated with childhood emotion dysregulation) and to state clearly the specific hypotheses of the current study.

This observation is appreciated. We have added more text to the introduction to address these points.

3. The investigators use months deployed as a proxy for combat exposure. Is it likely that Marines deployed at different times and different locations experienced differing degrees of combat exposure (the authors note in the discussion that combat exposure is homogenous in Marines, but might this differ according to timeline and location of deployment?).

The reviewer does address an important issue. However, all the Marines in this study deployed between September 2001 and June 2004 in support of the operations in Iraq and Afghanistan. While it is true that Marines may have been exposed to varying levels of combat or trauma based on specific occupation, location, or timing, in general, the exposure would have been similar as they all had recently finished recruit training and were deployed for the first time.

4. The authors explain that the AFQT score is an intelligence test in the discussion - this might be better explained for the reader when first introducing the term.

We agree and have provided additional text in the methods to address this concern.

5. Regarding the implications for the findings: the authors note that Marines with a history of ACE may be targeted for PTSD prevention programs, early interventions, or protection of stressful exposures. Is this feasible? That is, given that combat exposure tends to be homogenous among Marines, how feasible is it to try and limit combat exposure in a subpopulation that may constitute approximately 11% of Marines deployed?

This is a good question. Obviously, the most feasible option would be to offer the Marines most vulnerable for a PTSD diagnosis some type of prevention or early intervention program. This might include some extra training prior to deployment or informing unit leaders of Marines who might especially benefit from early intervention after a traumatic event. Furthermore, there are some jobs in the Marine Corps in which Marines are less likely to be exposed to combat, such as a helicopter mechanic. Marines who report high levels of ACE during recruit training could be encouraged to train for jobs that are likely to experience less combat.
Reviewer 3:

1. I couldn't see in the manuscript how many individuals had been excluded due to 'missing data', or having a mental health problem prior or during deployment. I think we need to know this when we read it. I can quite see why the authors have chosen to exclude those with a prior mental health problem before they deployed as this makes the analysis much cleaner, but I would still like to know how many people were excluded. It is more problematic for me that they have excluded those who developed mental health problems in theatre, and I would like to understand how many people this removed, and a bit further justification as to why this was done, as it seems to me that this is a potential source of bias. If the numbers were tiny, then it doesn't matter.

We recognize and agree with the need to clearly state the number of participants excluded from our study. We have added these numbers to the methods to address this issue. To answer the reviewer’s other concern, only 6 RAP responders had a PTSD diagnosis prior to their return from deployment. Furthermore, a sentence discussing the reason as to why those with a diagnosis during deployment were excluded was already present in the methods (“Diagnoses during deployment often do not appear in electronic medical records…”).

2. Another thing which I feel is worthy of further comment is the fact that the study did not have any measures of combat exposure. They do comment on this briefly but they don’t say why it matters. It is well established that having these pre-enlistment vulnerabilities is associated with risk taking behaviour, and it could well be that those who have these prior experiences are more likely to end up with mental health problems because they get themselves into situations during deployment which expose them to more combat. I think this possibility needs to be considered. Using duration of combat as a proxy measure of exposure doesn't deal with this.

We appreciate the comment from the reviewer. And while it is possible that those with pre-enlistment vulnerabilities are at increased risk for a PTSD diagnosis partly due to their behaviors during deployment, it is not feasible to examine this hypothetical question using data from RAP. Other longitudinal studies, such as the Millennium Cohort Study, may be better equipped to answer these types of questions.

3. Another limitation which is unavoidable and common to most of the work in this area is that the study doesn't include women, and in this case, is all Marines. This should be mentioned, as in the UK at least, Marines are not representative of the rest of the Armed Forces, and I would guess the same is true in the US.

We agree with the reviewer that it is very important to mention these types of limitations, however we feel that we have sufficiently stated these limitations of our study in the discussion (“Since this study was conducted among male Marines, the results may not be generalizable across all service branches or to female personnel.”).

4. I think it is interesting that the authors have decided to only examine outcomes in those who deployed, especially as they were looking at mental health outcomes which were not confined to PTSD, and they had the potential to examine outcomes in a much bigger sample of recruits. Another potentially powerful study design would have been to compare
mental health outcomes for those who had these vulnerabilities but did not deploy, with those who did, so that the impact of deployment could be untangled somewhat from these other pre-enlistment risk factors. It is not essential that the authors justify why they have chosen not to do this, but I did wonder this as a reader. In the aims, the authors made clear that they are interested in PTSD, and as such, I can understand why they then confined their sample to those deployed, but in fact their remit is somewhat broader than this in that they examine other mental disorders as well. It might be one sentence to justify their approach would satisfy the curiosity of other readers.

We appreciate this thoughtful comment from the reviewer. While we examined mental health outcomes, we were focusing on PTSD and therefore decided to limit the population to Marines who had deployed in support of the operations in Iraq and Afghanistan. As noted in the paper, deployed Marines are likely to experience similar traumatic experiences during deployment. Marines who do not deploy are exposed to differing amounts of trauma and, for almost all, fewer traumatic events than those deployed to a combat operation. Furthermore, it was not possible to assess which nondeployed Marines were exposed to a traumatic event, a necessary precursor to the development of PTSD. Finally, nondeploying Marines may be fundamentally different from their peers since all Marines should be readily deployable; reasons for nondeployment were not possible to discern in this study. We have expanded the explanation in the methods as to why nondeployed Marines were excluded from all analyses.

We appreciate the opportunity to respond to the comments and suggestions provided by the reviewers. Thank you again for considering our work for potential publication in the BMC Public Health. As corresponding author, please contact me if I can provide any additional information.

Very Respectfully,

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