Author’s response to reviews

Title: Posttraumatic Stress Disorder and Risk of Selected Autoimmune Diseases among US Military Personnel

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Author’s response to reviews:

Samuel Harris, PhD
Editor
BMC Psychiatry

Dear Dr. Harris,

We appreciate the feedback from the reviewers and are resubmitting a revised manuscript addressing their feedback and concerns. Below, we individually addressed each reviewer’s concern in detail (bolded). Enclosed please find our updated manuscript “Posttraumatic Stress Disorder and Risk of Selected Autoimmune Diseases among US Military Personnel,” in which the updates are tracked. Page and line numbers mentioned in the below responses to reviewers are based on the Tracked Version.

We have also removed "Millennium Cohort" from the author line.

Thank you for the opportunity to publish our paper in BMC Psychiatry. We appreciate your consideration.

Very sincerely,

Cynthia LeardMann, MPH
Senior Epidemiologist
Naval Health Research Center
Amit Shrira (Reviewer 1): I think that in view of the extensive literature in the field the introduction can end with specific hypotheses.

We agree and have updated the introduction to include specific hypotheses (pg 6, lines 104-107).

In the limitation section it should be added that PTSD was assessed by DSM-IV criteria and future studies should establish the association between PTSD with DSM-5 criteria and autoimmune diseases.

We appreciate the suggestions and have added a statement in the limitations about this point (pg. 15, lines 324-327).

Lisa M. James (Reviewer 2): While the PCL is widely used to screen for PTSD, it is also often criticized for reflecting general distress. This is of particular relevance in the present study given that PTSD is the focus of the study and that the majority of the "PTSD" sample also had other mental health conditions that may raise the distress aspects of the PCL. Limitations of using the PCL to define study groups should be acknowledged at the very least. What was the breakdown of provider diagnosed PTSD vs PCL-C positive screens?

The reviewer raises an important point, so we have added a statement in the limitations to address this concern (pg. 15, lines 322-328). However, prior studies indicate that PTSD assessed using the PCL-C based on the DSM-IV criteria has very high sensitivity along with high specificity (Brewin CR. Systematic review of screening instruments for adults at risk of PTSD. J Trauma Stress. 2005 Feb;18(1):53-62). In addition, evidence indicates that a large percent of service members with PTSD do not seek care (Quartana PJ, Wilk JE, Thomas JL, Bray RM, Rae Olmsted KL, Brown JM, Williams J, Kim PY, Clarke-Walper K, Hoge CW. Trends in mental health services utilization and stigma in US soldiers from 2002 to 2011. Am J Public Health. 2014 Sep;104(9):1671-9). Based on these observations, studies using Millennium Cohort data typically do not use ICD codes to identify those with PTSD.

The definition of combat is somewhat restricted and does not include more common combat-related experiences (e.g., firing a weapon at enemy, being fired at, going on patrols) that are typically assessed in standard measures of combat experiences (e.g, CES). The definition used here seems to conflate combat with Criterion A traumatic events.

While it is true that the definition of combat is somewhat restricted in this study, a recent study found that these 5 combat items perform similarly to more detailed measures (Porter B, Hoge CW, Tobin LE, Donoho CJ, Castro CA, Luxton DD, Faix DJ. Measuring Aggregated and Specific Combat Exposures: Associations Between Combat Exposure Measures and Posttraumatic Stress Disorder, Depression, and Alcohol-Related Problems. Journal of Traumatic Stress. 2018 Apr;31(2):296-306). We have added a similar statement in the manuscript (pg. 16, lines 326-327)
It would be helpful to know a little more about the items used to assess for physical assault, sexual assault, and sexual harassment.

We agree and have added more detail to the methods about these items (pg. 9, lines 178-182).

The authors themselves correctly point out the imprecision of the estimates for autoimmune diseases at several points throughout the manuscript. Relatedly, the number of cases is incredibly small for some diseases (e.g., 3 men and 9 women in the PTSD group with SLE); not surprisingly, many of the findings for specific diseases are non-significant.

We agree that some of these diseases are rare and therefore the estimates are quite imprecise. However, we still feel it is important to report these estimates and allow the readers to consider this imprecision in interpreting the effect estimates.

The authors refer to CD and UC in the discussion without prior mention of these IBD subtypes.

We have revised this sentence (pgs. 13, lines 275-277).