Reviewer’s report

Title: Association Between Baseline Psychological Attributes and Mental Health Outcomes After Soldiers Returned from Deployment

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Reviewer: James Naifeh

Reviewer’s report:

This study examined whether the baseline scores of new soldiers on the Global Assessment Tool (GAT) can predict postdeployment screening results for PTSD and depression. The findings indicate that soldiers with GAT scores in the bottom 5% may be at elevated risk for postdeployment mental health problems. The manuscript is well written and addresses the important issue of identifying soldiers who may be vulnerable to significant distress. Longitudinal analysis of soldiers who have just entered the Army is critical for understanding adverse mental health outcomes in this population. Below I have suggested several areas for improvement. Of greatest concern are the seemingly contradictory statements about whether the GAT should be used as a screening tool to exclude high-risk soldiers, as this has important policy implications.

1) Page 2, Line 43: To avoid confusion, the Abstract results for postdeployment PTSD should specify that it is associated with "baseline depression," rather than just "depression."

2) Page 3, Lines 50-58: The manuscript states "One important hypothesis is that while the increasing trend in mental health service needs may be influenced by the stressors involved with the protracted war on terrorism and military life in general, the upward trend in mental health service needs may also be reflective of poor psychological health of soldiers entering the military." Although pre-military mental health is likely associated with risk for post-deployment outcomes, it does not explain upward trends in psychiatric morbidity over time unless the proportion of new soldiers with pre-military mental health problems has increased. Perhaps the authors can clarify the point they are trying to make.

3) There appear to be conflicting statements about potential uses of the GAT. On one hand, the manuscript states "Knowing how well these psychological attributes can predict future mental health outcomes can potentially aid the DoD in identifying a workforce that is better suited for the stresses associated with its unique environment…” (Page 4, Lines 14-29). But then a couple paragraphs later the manuscript states "The GAT is not designed to be used as a screening tool and to do so in high stakes settings where employment decisions are made would be a mistake" (Page 5, Line 9-14). Then in the Discussion, the manuscript states that a composite risk score could be "useful in screening recruits" (Page 12, Line 6-9). The authors should clarify their stance on how the GAT should be used. Is it a screening instrument, and if so, what is the purpose of the screening (e.g., to intervene with high risk recruits vs. exclude them from service)?
4) Page 7, Lines 7-9: The manuscript states, "In an alternate analysis exercise, we included all soldiers." The manuscript should explain why this alternate analysis was conducted. This is revealed in the Results section but it would be helpful to explain the rationale up front.

5) Page 7, Line 48: It should be specified that that these PC-PTSD questions are based on the DSM-IV version of PTSD. Additionally, DSM-IV PTSD officially has only 3 symptom clusters, not 4 clusters as reported in the manuscript -- the avoidance and detachment questions in the PC-PTSD are assessing two aspects of the same cluster (Criterion B). Although confirmatory factor analytic studies support 4-factor models of DSM-IV PTSD, the official DSM-IV version is still 3 clusters. DSM-5 PTSD does have 4 clusters, but this is assessed using a revised version of the PC-PTSD that has 5 questions (https://www.ptsd.va.gov/professional/assessment/screens/pc-ptsd.asp).

6) Page 8, Lines 6-12: The manuscript appears to be saying that the GAT has both binary and Likert-type responses. If some of the GAT responses are binary, how were they standardized to a scale of 1-5? Perhaps I have misunderstood the description of the GAT?

7) Page 8, Line 48: The manuscript should clarify whether time-varying demographics (e.g., age, rank) are based on their value at the time of the GAT assessment or the post-deployment assessment.

8) Did the multivariate analyses of each GAT scale also adjust for the other GAT scales, or was each scale examined separately?

9) Page 11, Lines 36-44: Were ROC curves calculated for the full sample or the restricted sample? The results are described following the results for the full sample, so I assume that is the sample that was used. Is there a reason these analyses were not conducted on the restricted sample, which seems to be the primary analytic sample in the study?

10) Page 11, Lines 46-58: Similar to the previous comment, it is not clear if the interactions were tested in using the restricted sample, the full sample, or both. It would be helpful if the manuscript clearly specified which sample is being used for the ROC and interaction analyses. If only the full sample is being used then the manuscript should explain why the focus was switched to the full sample.

11) Page 11, Lines 50-52: The manuscripts states that "in general" the interactions between GAT variables and combat exposure/MOS were not significant, with the results available upon request. The language is a little vague. Does this mean that some of interactions were significant? If any of the interactions were significant, the authors might consider reporting them to enhance understanding of how GAT scores influence postdeployment risk. In addition, the conclusion that "the relationship between the baseline psychological attributes and post-deployment health conditions do not appear to be modified by the stress level of the work environment" may be too broad given the limited assessment of work-related stressors in this study. The 3 deployment stressors are on the more severe end, capturing direct exposure to combat and death (only 18-31% of the sample had experienced these stressors). The authors might consider using more specific language to describe these stressors rather than suggesting
they capture work-related stress, which has many dimensions in addition to combat and death/injury.

12) Page 12, Lines 4-34: Examining concentration of risk was a particularly useful step. I'm curious how the model would have performed if only the GAT was included. In other words, how useful is the GAT, on its own, in identifying concentration of risk? This is important if the GAT is going to be used as an early indicator of soldiers who may have subsequent problems.

13) Page 13, Lines 9-14: This is a very strong conclusion given the limitations of the current study. The brief screening scales in the PDHA (2 depression items, 4 PTSD items) provide a limited assessment of postdeployment mental health. It is impossible to know how soldiers who screened positive would have been classified based on a more thorough assessment that includes all symptoms and, importantly, an assessment of clinical distress and/or functional impairment. Further, this again raises the issue of how the GAT should be used. In the Introduction, the manuscript states that the GAT should not be used for employment screening, and yet that seems to the implication of describing low-scoring soldiers as a "poor fit for military service."

14) Page 15, Lines 7-19: The PDHRA analyses are a nice addition, but they should be included, and more thoroughly described in the Method and Results section. The Discussion is not the place to introduce new major analyses.

15) Page 15, Line 58: A citation should be included for the TAPAS.

16) Please clarify if the tables and figures are using the full or restricted sample.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes

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