PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (**http://bmjopen.bmj.com/site/about/resources/checklist.pdf**) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	The Influence of Military Component and Deployment-related Experiences on Mental Disorders among Canadian Military Personnel who deployed to Afghanistan: A Cross-Sectional Survey
AUTHORS	Boulos, David; Fikretoglu, Deniz

VERSION 1 – REVIEW

REVIEWER	Sunny J. Dutra, PhD Boston University School of Medicine and VA Boston Healthcare System, Boston MA, USA
REVIEW RETURNED	31-Aug-2017

GENERAL COMMENTS	This manuscript employs a large data set of psychiatric symptoms and service usage among Canadian service members deployed to Afghanistan. The statistics are sound and the results make a timely contribution to the literature on psychiatric symptoms and mental health care among deployed military service members. I believe some relatively minor changes to the framing of the results and their implications would improve the quality of the manuscript, as follows: 1) The assessment instrument employed, the Composite International Diagnostic Interview, has not been updated for DSM-5. As such, the authors cannot be certain that the findings related to specific diagnostic categories would translate to current (DSM-5) diagnoses. Related to this, two aspects of the present manuscript are particularly concerning: • The Alcohol Abuse finding specific to ResF personnel is concerning given that this diagnosis no longer exists (has been replaced by Alcohol Use Disorder, with altered diagnostic criteria) in DSM-5. • The 'any anxiety disorder' category includes PTSD, which is no longer categorized as an anxiety disorder in DSM-5. To address this, I recommend changing diagnostic category labels (e.g., PTSD) to 'symptoms of' labels (e.g., PTSD symptoms), and clarifying in the limitations section that the CIDI has not been updated for DSM-5 and as such the symptoms reported in the present study may not reliably map onto current diagnoses. 2) Related, in the 'Lifetime Traumatic Experiences' section, the authors describe the CIDI module on PTSD, which assesses lifetime exposure to a variety of potentially traumatic experiences. While this information is valuable, exposure is not the only criteria for a DSM-5-consistent Criterion A traumatic event.

	In fact, the CIDI does not assess these remaining criteria, and simple endorsement of exposure to an event does not qualify them as DSM-5 Criterion A traumatic events. The authors should clarify this distinction, and perhaps describe these events as exposure to 'adverse events' or 'potentially traumatic events' rather than 'lifetime traumatic experiences'. Minor Point: • In the Abstract, the abbreviation 'MHP' is first used in the Design section, but is not spelled out until the Primary Outcome Measure section.

REVIEWER	Susan Proctor US Army Research Institute of Environmental Medicine USA
REVIEW RETURNED	09-Oct-2017

GENERAL COMMENTS	Summary (from Abstract) The objective of the survey study of current Canadian Armed Forces was to examine differences in mental disorders between Regular and Reserve Forces with an Afghanistan deployment and assess the contribution of deployment-related experiences. Additionally, descriptive analyses were performed to examine use of mental health services at military or civilian facilities among those with a mental disorder. Results found that Reserve personnel were less likely to be identified with a past-year anxiety disorder but more likely to be identified with a past-year alcohol abuse disorder. Most all deployment-related experiences had some association with mental health problem. 'Ever felt responsible for the death of a Canadian or ally personnel' experience had the strongest association with mental health problem. Overall impression The manuscript provides a well-written description of the survey study, with detailed results tables. Specifics: - Abstract: The acronym 'MHP' is used in the Design paragraph, before it is defined (in Primary Outcome Measure paragraph). -Introduction (p 5, first para): The authors speculate that differences in MHP prevalence between component may be the result of differing MHSU but what about pre-existing, baseline factors as a possible reason for differences? -Introduction (p.5, last para.): Provide a reference for the statement, " It is notable that the CAF mental health system is arguably better resources and optimized to aid military personnel with MHP when compared to the Canadian civilian system." -Methods (p. 7): Even though the response rate was high, ~80%, were there any significant military service history differences in the characteristics of those persons who did not respond to the survey compared to those who did? Methods (p. 8): Are there more details about the LTEs asked on the CIDI module on PTSD? References pertaining to psychometrics and validity would be useful to include. -Methods (p.10): Were the list of DEXs 1-8 developed specifically for this study or are they based on deployment-related experiences that have been used in many other studies? Please include reference or description of the development of the experiences.

	-Abstract p 3 and Discussion p. 22: As acknowledged in the overview pertaining to the strengths and limitations of the study, the study was only conducted among currently serving personnel (excluding those released from service or who had left). And, a number of the participants recorded having Afghan deployment(s) many months/years (some greater than 7 years prior) prior to the survey. As such, the study results are prone to health warrior effect bias, in that the findings reflect those current persons healthy enough to remain in the Canadian service and not left or been released. Also, if is not evident that the survey has included detailed exposure to a variety of more recent/intervening experiences (i.e., social support, family circumstances) since deployment that may play more of an influence on current mental health problem status than past deployment experiences. Several papers from US studies and other military personnel deployed to recent Iraq and Afghanistan conflicts point to the importance of social support as factors predictive of mental health problems. Further acknowledgement of these limitations is warranted for the discussion.

REVIEWER	Alexander Millner Harvard University U.S.A.
REVIEW RETURNED	09-Oct-2017

GENERAL COMMENTS	The manuscript was focused on understanding differences in mental health problems between Canadian Armed Forces components – Regular Force vs Reserve Force – all of whom were deployed to Afghanistan. This paper has many strengths, most notably its use of a large military sample and the questions it pursues. However, first, overall, I found the paper to be fairly unorganized and difficult to follow, which undermined these strengths. If the authors can make a more organized and streamlined manuscript, it would greatly increase the potential impact of this paper. Second, I thought the authors could use more explanatory sentences throughout the manuscript, in the form of, “we examined x because” or longer explanations e.g. “We applied the final survey weights provided by Statistics Canada to determine descriptive and regression statistics” (I had a hard time understanding what this meant and what the purpose of the weighting was). Each section - the Abstract, Intro, Methods, Results and Discussion – were not clearly organized and/or did not necessary follow a similar pattern, which would greatly increase clarity. Examples: The Authors state that the main goal is to examine differences between components which they do throughout the paper but then there is a fairly important part that is dedicated to just understanding whether deployment experiences are related to MHP but the authors never clearly state this as one of their aims. The authors use fairly awkward language to describe this aim in the abstract (“to assess the contribution of deployment-related experiences”) and the last paragraph of the intro (“and to assess the contribution of deployment experiences after systematically adjusting for …”). It’s unclear what “contribution of deployment-related experiences"

	means and what it's contributing to. I would suggest that the authors more clearly state what their questions are and how they intended to test these questions. On the other hand, I understand that it's a little difficult to explain that the main goal of the paper is to compare component differences but then the goal deviates to assess overall association between deployment experiences/characteristics and MHP. The authors, I think, could make this work with a more organized and streamlined introduction. Overall, the authors need to more clearly and specifically state what the aims of the paper are so that the reader can follow those aims through the Methods, Results and Discussion. More specific items: -There is a long section on prior literature in the discussion. This, it seems, should be in the intro. -The intro very quickly moves to the question of MHSU as a possible factor (3rd paragraph) but this is the last aim and a secondary part of paper so it should take that place in the intro. -The following sentence is difficult to understand given the prior sentence - "This would be an initial step in assessing whether mental health services use (MHSU) was a contributing factor." - How are "differences are primarily a result of differing individual characteristics, past non-military traumatic life experiences, pre- and post-deployment training (e.g., mental health training), or deployment experiences" and MHSU related? What is the question here? Increased deployment-related MHP could be caused by differing individual characteristics, past non-military traumatic life experiences, pre- and post-deployment training or deployment experiences or lack of MHSU? This paragraph could be clearer and MHSU should probably be discussed later. - Three of the six paragraphs of the intro are in regards to MHSU. MHSU is a secondary aim and the primary aim was hardly discussed in any detail. I think the failure to set up the the important parts of the paper in the intro is part of the reason why I found the paper hard to follow. -The order of the Results section is as follows: (1) component differences on every single variable measured (2) assoc between MHP and component accounting for cov differences (3) assoc between deployment characteristics and MHP (4) assoc between deployment experiences and MHP (5) deployment-related trauma (LTE) (6) interactions between deployment characteristics, deployment experiences, deployment-related trauma and component on MHP. (7) difference in MHSU between components – measured, I guess, by Wald chi-sq. I do not think it is clear from the intro or the methods, how these analyses map on to the questions posed in the intro. I had to infer the goals of the paper from explicitly listing the analyses from the Results section. The intro and methods should do a better job leading the reading into this so that the reader knows what to expect when they arrive at the Results. Other comments/questions -The authors never define MHP. Is this just a stand in for mental

	disorders? What's the difference? It seems a little odd to say mental health problem and suicide ideation. If I heard the term mental health problem, I would think suicide ideation would be in that category. -In addition to MHP, there are other places where the authors use the acronyms without saying explaining what the acronym is in the text. MHP is mentioned in the abstract and PTSD, AUD. I know they are explained in the tables but should be in the main text as well. -Under types of traumatic experiences in the Methods section, the authors refer to an "Other" category. I would either capitalize or put quotes around it or both. That would clear up the subsequent sentence which says, "it was grouped with the other traumatic experiences" – ie "it was grouped with Other traumatic experiences" -It's unclear what this means – "Sample size for the Afghanistan-deployed population was determined using past-year PTSD prevalence estimates with a margin of error of no more than ± 0.7%." - Authors could give a rationale why sample was stratified by three military rank groupings and not some other factors? -One crucial limitation of this study is that I did not see any mention of age of onset of mental disorders. Thus, it is unclear whether disorders had their onset prior to or after deployment. This makes it difficult to imply much of anything in terms of what risk of mental disorders/MHP are due specifically to deployment.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1:
This manuscript employs a large data set of psychiatric symptoms and service usage among Canadian service members deployed to Afghanistan. The statistics are sound and the results make a timely contribution to the literature on psychiatric symptoms and mental health care among deployed military service members. I believe some relatively minor changes to the framing of the results and their implications would improve the quality of the manuscript, as follows:

REVIEWER 1, COMMENT 1:
1) The assessment instrument employed, the Composite International Diagnostic Interview, has not been updated for DSM-5. As such, the authors cannot be certain that the findings related to specific diagnostic categories would translate to current (DSM-5) diagnoses. Related to this, two aspects of the present manuscript are particularly concerning:
• The Alcohol Abuse finding specific to ResF personnel is concerning given that this diagnosis no longer exists (has been replaced by Alcohol Use Disorder, with altered diagnostic criteria) in DSM-5.
• The 'any anxiety disorder' category includes PTSD, which is no longer categorized as an anxiety disorder in DSM-5.
To address this, I recommend changing diagnostic category labels (e.g., PTSD) to 'symptoms of' labels (e.g., PTSD symptoms), and clarifying in the limitations section that the CIDI has not been updated for DSM-5 and as such the symptoms reported in the present study may not reliably map onto current diagnoses.

RESPONSE 1:

I agree, the CIDI version 3.0 uses DSM IV criteria for disorders and this is now more clearly indicated in the methods section. It was never indicated that the outcomes were DSM V but a reader may mistakenly think they are. Some description is now provided in the limitations section to remind the reader that some of the assessed disorders differ between DSM IV and DSM V. However, given that it was clearly stated that the CIDI was used to assess disorders, and it is now indicated that this uses DSM IV criteria, it is felt that use of the phrase 'symptoms of' would be misleading since the CIDI-assessed DSM IV criteria were satisfied.

REVIEWER 1, COMMENT 2:
2) Related, in the 'Lifetime Traumatic Experiences' section, the authors describe the CIDI module on PTSD, which assesses lifetime exposure to a variety of potentially traumatic experiences. While this information is valuable, exposure is not the only criteria for a DSM-5-consistent Criterion A traumatic event. In fact, the CIDI does not assess these remaining criteria, and simple endorsement of exposure to an event does not qualify them as DSM-5 Criterion A traumatic events. The authors should clarify this distinction, and perhaps describe these events as exposure to 'adverse events' or 'potentially traumatic events' rather than 'lifetime traumatic experiences'.

RESPONSE 2:
I have made some text changes in the methods and tables to indicate that the lifetime traumatic experiences were lifetime potentially traumatic experiences.

REVIEWER 1, COMMENT 3:
Minor Point:
• In the Abstract, the abbreviation 'MHP' is first used in the Design section, but is not spelled out until the Primary Outcome Measure section.

RESPONSE 3:
Text changes have been made to the abstract and MHP is now spelled out with its first use.

Reviewer 2:
Summary (from Abstract)
The objective of the survey study of current Canadian Armed Forces was to examine differences in mental disorders between Regular and Reserve Forces with an Afghanistan deployment and assess the contribution of deployment-related experiences. Additionally, descriptive analyses were performed to examine use of mental health services at military or civilian facilities among those with a mental disorder. Results found that Reserve personnel were less likely to be identified with a past-year anxiety disorder but more likely to be identified with a past-year alcohol abuse disorder. Most all deployment-related experiences had some association with mental health problem. 'Ever felt responsible for the death of a Canadian or ally personnel' experience had the strongest association with mental health problem.
Overall impression
The manuscript provides a well-written description of the survey study, with detailed results tables.

Specifics:

REVIEWER 2, COMMENT 1:
- Abstract: The acronym 'MHP' is used in the Design paragraph, before it is defined (in Primary Outcome Measure paragraph).

RESPONSE 1:
Text changes were made to the abstract and MHP is now spelled out with its first use.
REVIEWER 2, COMMENT 2:

-Introduction (p 5, first para): The authors speculate that differences in MHP prevalence between components may be the result of differing MHSU but what about pre-existing, baseline factors as a possible reason for differences?

RESPONSE 2:
Respectfully, differing individual characteristics between components was meant to include differences in baseline vulnerabilities (e.g., social support) as well. Some text changes were made in the introduction to further highlight this.

REVIEWER 2, COMMENT 3:
-Introduction (p.5, last para.): Provide a reference for the statement, " It is notable that the CAF mental health system is arguably better resources and optimized to aid military personnel with MHP when compared to the Canadian civilian system."

RESPONSE 3:
A reference has been included.

REVIEWER 2, COMMENT 4:
-Methods (p. 7): Even though the response rate was high, ~80%, were there any significant military service history differences in the characteristics of those persons who did not respond to the survey compared to those who did?

RESPONSE 4:
Unfortunately, there was limited information available from the non-responders. Additionally, due to privacy restrictions, administrative data on deployment information from non-responders was not identifiable for comparison.

REVIEWER 2, COMMENT 5:
Methods (p. 8): Are there more details about the LTEs asked on the CIDI module on PTSD? References pertaining to psychometrics and validity would be useful to include.

RESPONSE 5:
The LTE are lifetime potentially traumatic experiences. Two references have been included for the interested reader to review further:

Haro JM, Arbabzadeh-Bouchez S, Brugha TS et al. Concordance of the Composite International Diagnostic Interview Version 3.0 (CIDI 3.0) with standardized clinical assessments in the WHO World Mental Health surveys. Int J Methods Psychiatr Res 2006;15(4):167-180.

Kessler RC, Ustun TB. The World Mental Health (WMH) Survey Initiative Version of the World Health Organization (WHO) Composite International Diagnostic Interview (CIDI). Int J Methods Psychiatr Res 2004;13(2):93-121.

REVIEWER 2, COMMENT 6:
-Methods (p.10): Were the list of DEXs 1-8 developed specifically for this study or are they based on deployment-related experiences that have been used in many other studies? Please include reference or description of the development of the experiences.

RESPONSE 6:
These DEX were adapted from the Combat Experiences Scale (CES) that was developed by the Walter Reed Army Institute for Research. These items were previously assessed among CAF

personnel and after pilot testing for this survey, the 8 specified items were included. The text in the methods section has been updated to reflect this.

REVIEWER 2, COMMENT 7:
-Abstract p 3 and Discussion p. 22: As acknowledged in the overview pertaining to the strengths and limitations of the study, the study was only conducted among currently serving personnel (excluding those released from service or who had left). And, a number of the participants recorded having Afghan deployment(s) many months/years (some greater than 7 years prior) prior to the survey. As such, the study results are prone to health warrior effect bias, in that the findings reflect those current persons healthy enough to remain in the Canadian service and not left or been released. Also, if is not evident that the survey has included detailed exposure to a variety of more recent/intervening experiences (i.e., social support, family circumstances) since deployment that may play more of an influence on current mental health problem status than past deployment experiences. Several papers from US studies and other military personnel deployed to recent Iraq and Afghanistan conflicts point to the importance of social support as factors predictive of mental health problems. Further acknowledgement of these limitations is warranted for the discussion.

RESPONSE 7:
Respectfully, the study limitation pertaining to the exclusion of personnel who were released from service at the time the survey was implemented, possibly with mental health concerns, has already been discussed in the limitations section. However, some text changes have been made to identify social support and social environment as factors that were not fully assessed but potentially have an influence on mental health.

Reviewer 3:
REVIEWER 3, COMMENT 1:
The manuscript was focused on understanding differences in mental health problems between Canadian Armed Forces components – Regular Force vs Reserve Force – all of whom were deployed to Afghanistan.

This paper has many strengths, most notably its use of a large military sample and the questions it pursues. However, first, overall, I found the paper to be fairly unorganized and difficult to follow, which undermined these strengths. If the authors can make a more organized and streamlined manuscript, it would greatly increase the potential impact of this paper. Second, I thought the authors could use more explanatory sentences throughout the manuscript, in the form of, “we examined x because” or longer explanations e.g. “We applied the final survey weights provided by Statistics Canada to determine descriptive and regression statistics” (I had a hard time understanding what this meant and what the purpose of the weighting was).

RESPONSE 1:
Some text changes were made in the methods section.

REVIEWER 3, COMMENT 2:
Each section - the Abstract, Intro, Methods, Results and Discussion – were not clearly organized and/or did not necessary follow a similar pattern, which would greatly increase clarity.

Examples:
The Authors state that the main goal is to examine differences between components which they do throughout the paper but then there is a fairly important part that is dedicated to just understanding whether deployment experiences are related to MHP but the authors never clearly state this as one of their aims. The authors use fairly awkward language to describe this aim in the abstract (“to assess the contribution of deployment-related experiences”) and the last paragraph of the intro (“and to

assess the contribution of deployment experiences after systematically adjusting for …"). It's unclear what "contribution of deployment-related experiences" means and what it's contributing to. I would suggest that the authors more clearly state what their questions are and how they intended to test these questions.

On the other hand, I understand that it's a little difficult to explain that the main goal of the paper is to compare component differences but then the goal deviates to assess overall association between deployment experiences/characteristics and MHP. The authors, I think, could make this work with a more organized and streamlined introduction. Overall, the authors need to more clearly and specifically state what the aims of the paper are so that the reader can follow those aims through the Methods, Results and Discussion.

RESPONSE 2:
Some text changes were made to the abstract and introduction section.

REVIEWER 3, COMMENT 3:
More specific items:
-There is a long section on prior literature in the discussion. This, it seems, should be in the intro.

RESPONSE 3:
Respectfully, the literature cited in the discussion was used as comparators for the current findings and has a place in the discussion. Some of these references were also cited in the introduction when making statements about associations previously identified by other researchers.

REVIEWER 3, COMMENT 4:
-The intro very quickly moves to the question of MHSU as a possible factor (3rd paragraph) but this is the last aim and a secondary part of paper so it should take that place in the intro.

RESPONSE 4:
Respectfully, while I agree that the introduction section includes more description for the MHSU secondary objective then might be warranted, this item required a little more description to convey the interplay between MHSU and mental health problems and how MHSU could logically differ between components. However, some text changes have been made in the introduction section..

REVIEWER 3, COMMENT 5:
-The following sentence is difficult to understand given the prior sentence - "This would be an initial step in assessing whether mental health services use (MHSU) was a contributing factor."

RESPONSE 5:
The sentence has been removed.

REVIEWER 3, COMMENT 6:
- How are "differences are primarily a result of differing individual characteristics, past non-military traumatic life experiences, pre- and post-deployment training (e.g., mental health training), or deployment experiences" and MHSU related? What is the question here?

Increased deployment-related MHP could be caused by differing individual characteristics, past non-military traumatic life experiences, pre- and post-deployment training or deployment experiences or lack of MHSU? This paragraph could be clearer and MHSU should probably be discussed later.

RESPONSE 6:

Changes have been made to the text in the introduction section but respectfully, the sentence fragments are taken out of context.

The sentence “Unfortunately, studies that suggest that ResF personnel returning from deployment tend to have a greater prevalence of MHP have not systematically investigated whether such differences are primarily a result of differing individual characteristics, past non-military traumatic life experiences, pre- and post-deployment training (e.g., mental health training), or deployment experiences.”
suggests that component differences in MHP that others have identifies may be a result of uncontrolled differences between the two groups (RegF and ResF). This ultimately sets up part of the rationale for the detailed analysis needed to compare components (primary objective).

The sentence “It is also possible that the observed component differences in MHP prevalence are a result of differing MHSU among those with MHP; differences in the amount, timing (delay), and quality of care may manifest as symptoms being more persistent and ultimately, more prevalent in one component relative to the other.” suggests how MHP prevalence differences between components could also partially be a result of slower symptom resolution, possibly as a result of differing MHSU patterns in those with a mental disorder.

REVIEWER 3, COMMENT 7:
- Three of the six paragraphs of the intro are in regards to MHSU. MHSU is a secondary aim and the primary aim was hardly discussed in any detail. I think the failure to set up the the important parts of the paper in the intro is part of the reason why I found the paper hard to follow.

RESPONSE 7:
Some text changes have been made in the introduction section.

REVIEWER 3, COMMENT 8:
-The order of the Results section is as follows: (1) component differences on every single variable measured (2) assoc between MHP and component accounting for cov differences (3) assoc between deployment characteristics and MHP (4) assoc between deployment experiences and MHP (5) deployment-related trauma (LTE) (6) interactions between deployment characteristics, deployment experiences, deployment-related trauma and component on MHP. (7) difference in MHSU between components – measured, I guess, by Wald chi-sq. I do not think it is clear from the intro or the methods, how these analyses map on to the questions posed in the intro. I had to infer the goals of the paper from explicitly listing the analyses from the Results section. The intro and methods should do a better job leading the reading into this so that the reader knows what to expect when they arrive at the Results.

RESPONSE 8:
Some text changes have been made in the introduction and methods sections.

REVIEWER 3, COMMENT 9:
Other comments/questions

-The authors never define MHP. Is this just a stand in for mental disorders? What’s the difference? It seems a little odd to say mental health problem and suicide ideation. If I heard the term mental health problem, I would think suicide ideation would be in that category.

RESPONSE 9:
Some text changes have been made in an attempt to correct instances where there was ambiguity. Use of the phrase mental health problems (MHP) was meant to refer to any mental health problem

except when referring to the study outcomes where only 6 past-year mental disorders were assessed (via the WHO CIDI) as was past-year suicide ideation.

REVIEWER 3, COMMENT 10:
-In addition to MHP, there are other places where the authors use the acronyms without saying explaining what the acronym is in the text. MHP is mentioned in the abstract and PTSD, AUD. I know they are explained in the tables but should be in the main text as well.

RESPONSE 10:
Some text changes have been made.

REVIEWER 3, COMMENT 11:
-Under types of traumatic experiences in the Methods section, the authors refer to an “Other” category. I would either capitalize or put quotes around it or both. That would clear up the subsequent sentence which says, “it was grouped with the other traumatic experiences” – ie “it was grouped with Other traumatic experiences”

RESPONSE 11:
Changes have been made to the text in the methods section.

REVIEWER 3, COMMENT 12:
-It’s unclear what this means – “Sample size for the Afghanistan-deployed population was determined using past-year PTSD prevalence estimates with a margin of error of no more than ± 0.7%.”

RESPONSE 12:
The initial sample size identified for the survey was chosen by the survey implementation organization (Statistics Canada). They estimated that the sample size chosen would be sufficient to estimate a past-year PTSD prevalence in the study population with a margin of error of no more than ± 0.7%. The statement has been modified in the methods section to reflect this.

REVIEWER 3, COMMENT 13:
- Authors could give a rationale why sample was stratified by three military rank groupings and not some other factors?

RESPONSE 13:
Some text changes have been made in the methods section. These strata were chosen to ensure sufficient numbers were sampled with certain characteristics. For example, associations between rank and a number of outcomes have been noted in previous research among the CAF, perhaps because it can be a proxy for other things such as age, certain exposures, etc., and fewer individuals with higher ranks were in the study population. Hence the need for over-sampling some strata and the need to use sampling weights in the analyses in order to produce estimates that were representative of the population.

REVIEWER 3, COMMENT 14:
-One crucial limitation of this study is that I did not see any mention of age of onset of mental disorders. Thus, it is unclear whether disorders had their onset prior to or after deployment. This makes it difficult to imply much of anything in terms of what risk of mental disorders/MHP are due specifically to deployment.

RESPONSE 14:
Some text changes have been made in the discussion section. It was indicated that a precise date for onset of the measured disorders and the various deployment experiences relative to individuals’

Afghanistan-related deployment return prevented more detailed assessments that took temporality into account.

The revisions to the manuscript have been implemented with track changes enabled to facilitate their review but I have also enclosed a version with track changes removed. I hope that the responses and revisions meet your expectations.

VERSION 2 – REVIEW

REVIEWER	Sunny J. Dutra, PhD VA Boston Healthcare System, USA
REVIEW RETURNED	17-Nov-2017

GENERAL COMMENTS	My concerns have been addressed adequately.

REVIEWER	Susan Proctor US Army Research Institute of Environmental Medicine, USA
REVIEW RETURNED	11-Nov-2017

GENERAL COMMENTS	N/a

REVIEWER	Alexander Millner Harvard University USA
REVIEW RETURNED	03-Nov-2017

GENERAL COMMENTS	Overall, the authors made minimal changes. I understand the desire for a short introduction but I continue to believe that the current introduction leaves readers in the dark and does not invite them to further read or digest the findings. This could hurt the impact of the paper. That being said, that is the framing aspect of the paper and, of course taking the limitations into account, I think the paper provides some results that are worthwhile. The authors clearly disagree with me on the quality of their introduction and if the editor/action editor do not mind an intro that does not set up the paper well or disagree with me that that's the case, then that's fine. I will not recommend rejection on framing. As an aside, it is my experience as both a reviewer and as an author, that when one changes text in response to a review, they quote the changed text within the response to reviewers so that reviewers do not have to hunt for it. Along the same lines, this is probably as dismissive a response to reviews as I've seen - both in terms of lacking to engage the criticisms provided by the reviewers and not trying terrible hard to improve the manuscript in response to the criticisms.

VERSION 2 – AUTHOR RESPONSE

Reviewer 1:
My concerns have been addressed adequately.

RESPONSE:
Thank you.

Reviewer 2:
N/A

RESPONSE:
Thank you.

Reviewer 3:
Overall, the authors made minimal changes. I understand the desire for a short introduction but I continue to believe that the current introduction leaves readers in the dark and does not invite them to further read or digest the findings. This could hurt the impact of the paper. That being said, that is the framing aspect of the paper and, of course taking the limitations into account, I think the paper provides some results that are worthwhile. The authors clearly disagree with me on the quality of their introduction and if the editor/action editor do not mind an intro that does not set up the paper well or disagree with me that that's the case, then that's fine. I will not recommend rejection on framing.

RESPONSE:
Additional adjustments were made to the introduction. Deployment experiences and their influence on mental health in service members was discussed in more detail and some research on their association with mental health problems was further highlighted. The importance of deployment experiences as potentially mediating any identified mental health problem differences between Regular and Reserve Force personnel with prior difficult deployments was also further highlighted. The limited assessment of deployment experiences in prior research that compared the mental health of Regular and Reserve Force personnel with prior deployments was noted and it was indicated that the current study helps to address this limitation. More detail has also been included for the primary objective statement:

“The primary objectives of this study were to explore differences in prevalent MHP between active service RegF and ResF CAF personnel with a past Afghanistan deployment, to assess the influence of deployment experiences on identified MHP differences between components, and specifically, to quantify an estimate of the contribution of both component and deployment experiences to prevalent MHP (i.e., six measured past-year mental disorders and past-year suicide ideation) using covariate-adjusted prevalence difference estimates.”

The revisions to the manuscript have been implemented with track changes enabled to facilitate their review and I have also enclosed a version with track changes removed. I hope that the responses and revisions meet your expectations.