PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Differences between the Canadian Military's Regular and Reserve Forces in Perceived Need for Care, Mental Health Services Use, and Perceived Sufficiency of Care: A Cross-Sectional Survey
AUTHORS	Boulos, David; Fikretoglu, Deniz

VERSION 1 - REVIEW

REVIEWER	Alexander Millner Harvard University U.S.A.
REVIEW RETURNED	23-Jan-2019

GENERAL COMMENTS	This paper explored perceived need for mental health services as well as perceived sufficiency of care among Regular (RegF) and Reserve (ResF) Forces within the Canadian Armed Forces. The paper is fairly straightforward, with the finding that ResF show lower perceived need for medication, lower mental health service use and lower sufficiency of care. I have only a few comments. 1) Based on the introduction, it seems like the authors suspected that the ResF would have lower levels of MHSU and perceived need? Why not state a hypothesis? 2) In the Methods the section called "Mental Health Services Use" is repeated in the next section called Perceived Need for Care and Perceived Sufficiency of Care. The same language doesn't need to be repeated twice. 3) It's hard for me to understand if and if so, how deployment plays a role in this study. Why focus only on those with a deployment instead of all personnel with a mental health disorder regardless of deployment? 4) Can the authors just provide one or two additional sentences regarding why they are running models 2-6 with all the covariates. Presumably to test whether there are effects of moderation or effects persist above and beyond other covariates. 5) I've not seen the variable reduction strategy retaining variables with a p-value of less than .025. Is that a typical approach? Why not sure a statistical learning technique such as lasso, particularly given the large sample, for variable reduction. 6) The authors explain that lower MHSU use among ResF after controlling for perceived need suggests that barriers to mental health care likely exist. Can the authors explain this little more? Shouldn't the PNC fully explain the reduced MHSU among ResF - barriers

	would be explained by response “4) Need not met: perceived a need but did not receive any.”? What other factors – either measurement issues or other explanations might explain why PNC doesn’t fully explain MHSU? 7) I wonder if there are any additional variables or question the authors might suggest that would advance our understanding of why ResF personnel have lower MHSU – questions about use of private facilities for example.

REVIEWER	Anna Torrens Armstrong University of South Florida, USA
REVIEW RETURNED	19-Feb-2019

GENERAL COMMENTS	This is a well-written and excellent article exploring the differences in various aspects of mental health needs/access among active and reserved military populations in CAF. It confirms existing trends and directs the need for further research at a more detailed level to explore why these differences exist in an effort to develop programs/policy to support troops who have deployed and experiencing mental health disorders.

REVIEWER	Laura Goodwin University of Liverpool, UK
REVIEW RETURNED	15-Apr-2019

GENERAL COMMENTS	This is an important research paper which addresses and identifies a problem regarding whether reservist personnel are less likely to seek help than regulars in the Canadian forces. However, I felt that in the current form there is a lot of information and data incorporated into the manuscript and that the author needs to think more carefully about the readability. Lots of acronyms are additionally used, which additionally makes the findings more difficult to digest. I think that once these issues have been addressed, in addition to the more specific points I raise below, that the paper should be acceptable for publication. I should note that I completely agree with the authors that it is most appropriate to conduct this investigation in individuals with a self-identified problem. Abstract: The issue of acronyms was particularly apparent in the abstract in which the results section was more challenging to read than it could have been. It may be worth considering presenting less results, but make the text clearer for those which are discussed. Introduction: I have two main comments regarding the introduction. One is that the background and very literature is very focused on a Canadian context, which is obviously relevant to the content of this paper, but given this is a British/international journal then it could be worth including a wider range of sources. The second comment relates to the justification of all the covariates which have been studied in this paper and if this could be more explicit in the introduction. For example, in paragraph 1 of page 6 I thought that the point around why childhood adversity may impact of help seeking could be developed further. Could it not be that those who have experienced adversity in childhood are more likely to have previous experiences of different health services and that this previous care and satisfaction with services could then impact on

	current use? Methods: There is a lot of description of the measures selected, and whilst this is needed to explain the different measures currently included, I wondered if some of them are necessary, particularly given not all are particularly discussed in the results section. It could also have been clearer that the mental health measures are assessed in individuals who are already self-identifying as having a mental health problem (I think that is the case, but I did have to go back and check a couple of times). One of the outcomes which I wasn't particularly clear on was the medication variable, so this should possibly be expanded on. Regarding the model selection, then how were the parameters for exclusion chosen? i.e. the p>0.25 cut off. The approach for model 6 using marginal standardisation should be described more clearly, as it is not completely obvious from the text how this modelling was conducted. Results: My main issue with the results is that there are multiple vast results tables, yet I didn't feel that the findings were adequately discussed in the text of the results, and so many readers may miss some of the key findings. I wondered if the tables can be reduced somewhat and more focus given to a restricted set of analyses. One issue which relates to this is regarding the differences in the mental health of these populations, described in earlier tables, but I didn't feel this was adequately discussed and how it could impact on the outcomes. In the adjusted models I wanted to know more about how this could have explained the difference in whether help was sought and whether those with poorer functioning were more likely to be the regulars and so this could have (to some extent) explained the difference. Discussion: I thought that the discussion was generally well written and informative, but wondered if a bit more could be said about the reasons why reservists may be less likely to seek care, beyond perceived need for care.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1:

This paper explored perceived need for mental health services as well as perceived sufficiency of care among Regular (RegF) and Reserve (ResF) Forces within the Canadian Armed Forces.

The paper is fairly straightforward, with the finding that ResF show lower perceived need for medication, lower mental health service use and lower sufficiency of care. I have only a few comments.

REVIEWER 1, COMMENT 1:

1) Based on the introduction, it seems like the authors suspected that the ResF would have lower levels of MHSU and perceived need? Why not state a hypothesis?

RESPONSE 1:

Thank you and yes, that was the hypothesis. I've added a sentence following the study objective to indicate this: "We hypothesize that ResF personnel will have a lower PNC, MHSU, and PSC relative to their RegF counterparts and attempt to identify factors influencing these outcomes."

REVIEWER 1, COMMENT 2:

2) In the Methods the section called "Mental Health Services Use" is repeated in the next section called Perceived Need for Care and Perceived Sufficiency of Care. The same language doesn't need to be repeated twice.

RESPONSE 2:

Thank you, I agree that it doesn't need to be repeated. I've removed the repeated text from the "Perceived Need for Care and Perceived Sufficiency of Care" subsection and made reference to the previous use.

REVIEWER 1, COMMENT 3:

3) It's hard for me to understand if and if so, how deployment plays a role in this study. Why focus only on those with a deployment instead of all personnel with a mental health disorder regardless of deployment?

RESPONSE 3:

There are a few layers to the rationale. Reserve Force members with a prior deployment would have potentially had exposure to experiences that are associated with an increased probability of developing a mental health problem. Additionally, Reserve Force members' access to free military mental health services is only available during active service or when a health problem is identified as 'service-related'. So this group would be most relevant to service provision (i.e., possibly a higher need and a service provision obligation); and it was expected that a lower proportion of non-deployed Reserve Force members would have a mental disorder. For this reason, only Reserve Force members with a prior 'Afghanistan' deployment who were in service at the time of the survey implementation were included in the sampling frame. In addition to this, there has been some suggestion, without evidence, that the experiences or preparedness of deployed Reserve Force members may not be similar to comparably deployed Regular Force members and that this difference may translate into a difference in the perception of a need for care and subsequent mental health services use and perceived sufficiency of received care. Hence the choice to compare Regular and Reserve Force members that had a prior Afghanistan deployment.

Moreover, some research has also indicated that deployment experiences and associated deployment characteristics can potentially influence individuals' perception of barriers to care (e.g., perceiving a need for care) and others have found that the perception of barriers to care can vary around the deployment cycle and be higher among those with combat exposure. So it was felt that deployment experiences and characteristics were important items to consider in this analysis when comparing Reserve and Regular Force personnel on the study outcomes.

This information is largely contained in the report already, most of this rationale can be found in the 'Introduction' section and some additional items can be found in the 'Methods' section. I've made a small addition in the 'Methods' section highlighting the fact that only Reserve Force personnel with a prior Afghanistan deployment were included in the sampling strategy and that for comparability, only Regular Force personnel with a similar deployment exposure were additionally included in the analyses.

REVIEWER 1, COMMENT 4:

4) Can the authors just provide one or two additional sentences regarding why they are running models 2-6 with all the covariates. Presumably to test whether there are effects of moderation or effects persist above and beyond other covariates.

RESPONSE 4:

Thank you, yes that was part of the reason.

I have made some additions to the text in the methods section outlining the reasoning. It is now indicated that models 2 to 6 were used to assess for the incremental influence of the variable groups (i.e., military and sociodemographic, health-related, LTE and child abuse, pre- and post-deployment mental health training, and deployment-related variable groups) on the hypothesized association between military component and the outcomes. Each of these variable groups have been suggested to have an influence on the outcomes being assessed and this approach would, presumably, offer some suggestion as to how and whether each explains some of the observed unadjusted outcome differences between military components. It would also provide some indication as to whether subsequently included variable groups moderate this unadjusted association (component and outcome) and whether variable groups included later mediate the influence of variable groups added earlier.

REVIEWER 1, COMMENT 5:

5) I've not seen the variable reduction strategy retaining variables with a p-value of less than .025. Is that a typical approach? Why not sure a statistical learning technique such as lasso, particularly given the large sample, for variable reduction.

RESPONSE 5:

The variable-reduction strategy was devised to assist with a concern. We had a large sample size but this was reduced when we focused the analysis on those with an identified mental disorder and additionally, we wanted to assess a large number of covariates (and variable groups). So the variable-reduction strategy had a dual purpose. We wanted to assess a fairly large number of variables and still retain each variable group's identity but we wanted to ensure that we didn't diminish the level of power to detect differences. As such, we chose to use propensity scores with one variable group and within the remaining variable groups we chose a conservative p-value approach ($p \geq 0.25$) to exclude the most non-significant variables in the variable group. I've added this reasoning to the methods section.

Although we didn't consider using a statistical learning technique, it's felt that its use would not have been ideal given the objective as outlined in 'Response 4'.

REVIEWER 1, COMMENT 6:

6) The authors explain that lower MHSU use among ResF after controlling for perceived need suggests that barriers to mental health care likely exist. Can the authors explain this little more?

Shouldn't the PNC fully explain the reduced MHSU among ResF - barriers would be explained by response "4) Need not met: perceived a need but did not receive any."? What other factors – either measurement issues or other explanations might explain why PNC doesn't fully explain MHSU?

RESPONSE 6:

Thank you, this appears to be a general comment on items from the 'Discussion' section. The Reserve and Regular Force comparison was among those who had an objective need for care (i.e., a past-year mental disorder). After adjusting for covariate groups that can potentially influence the level of perceived need (i.e., military and sociodemographic, health-related, LTE and child abuse, pre- and post-deployment mental health training, and deployment-related variable groups), we observed that perceived need for care only remained higher among Reserve Force members with respect to medication services – but we observed that the MHSU among Reserve Force members remained lower for any service, medication services (as expected from the lower perceived need) and marginally for counselling services. While differences were modest, this suggests a deficit in expected MHSU by Reserve Force members (i.e. overall comparable perceived need but lower MHSU) and this deficit is attributable to all barriers, perceived need included. It's suggested that this may be due to structural barriers to care (which have been reported) but there are other possible barriers that could explain this, such as perceived need and stigma, career concerns, etc. Additionally, it's possible that structural barriers may manifest as a result of a differential in facilitators to care between Regular and Reserve Force members.

Yes, among participants who perceived a need for care, those indicating item "4) Need not met: perceived a need but did not receive any" would represent service members who were experiencing a barrier other than perceiving the need. But also, those with only a partially met need are likely experiencing a barrier to care other than perceiving the need as well. Generally speaking, service members receive mental health training around deployments to increase their awareness of the possibility that mental health concerns can occur during/ following deployment. In an ideal situation, service members with a need for mental health services should perceive a need for services and then should seek this out. Barriers to this connection have been documented and perceiving this need was often indicated as the most prevalent but other barriers have been documented; these are touched on with references in the Introduction section (e.g., stigma, negative beliefs about mental disorders and associated treatments, a concern over potential negative career consequences, and systemic issues such as lengthy wait times and poor accessibility).

I added some text in the 'Discussion' section to highlight that Reserve Force members' lower perceived sufficiency of care was suggestive of barriers to care other than perceiving a need. It was indicated that a need for care not fully met would suggest that barriers other than perceiving a need for care were present and this would be more indicative when assessing whether or not a need was at least partially met.

REVIEWER 1, COMMENT 7:

7) I wonder if there are any additional variables or question the authors might suggest that would advance our understanding of why ResF personnel have lower MHSU – questions about use of private facilities for example.

RESPONSE 7:

Further work is warranted to investigate why Reserve Force personnel have a lower MHSU. We found that there was a differential in the barriers to care between Regular and Reserve Force personnel and while a small difference in perceived need for care was observed, other barriers in addition to perceiving a need, were implied by the results. Previous work on this study population noted that Reserve Force personnel had more past-year civilian MHSU relative to Regular Force members and 29% reported that not being eligible for CAF health services was their justification for using civilian services. Based on this, the next step might be to investigate the reported barriers to care as this would help to update information services and awareness programs with a goal of offsetting any identified barriers. A small adjustment was made to the last sentence in the conclusion to further highlight this statement.

Reviewer 2:

This is a well-written and excellent article exploring the differences in various aspects of mental health needs/access among active and reserved military populations in CAF. It confirms existing trends and directs the need for further research at a more detailed level to explore why these differences exist in an effort to develop programs/policy to support troops who have deployed and experiencing mental health disorders.

RESPONSE:

Thank you for your comments.

Reviewer 3:

This is an important research paper which addresses and identifies a problem regarding whether reservist personnel are less likely to seek help than regulars in the Canadian forces. However, I felt that in the current form there is a lot of information and data incorporated into the manuscript and that the author needs to think more carefully about the readability. Lots of acronyms are additionally used, which additionally makes the findings more difficult to digest. I think that once these issues have been addressed, in addition to the more specific points I raise below, that the paper should be acceptable for publication. I should note that I completely agree with the authors that it is most appropriate to conduct this investigation in individuals with a self-identified problem.

REVIEWER 3, COMMENT 1:

Abstract: The issue of acronyms was particularly apparent in the abstract in which the results section was more challenging to read than it could have been. It may be worth considering presenting less results, but make the text clearer for those which are discussed.

RESPONSE 1:

Thank you. I revised the abstract, slightly rephrasing the text in the results and conclusion sections and reduced the use of acronyms. I retained acronyms for the main outcomes and component as these phrases are often repeated and there is a need to conserve word usage. I retained PNC = perceived need for care, MHSU = mental health services use, PSC = perceived sufficiency of care, RegF =Regular Force personnel, ResF = Reserve Force personnel, and CI = confidence interval.

REVIEWER 3, COMMENT 2:

Introduction: I have two main comments regarding the introduction. One is that the background and very literature is very focused on a Canadian context, which is obviously relevant to the content of this paper, but given this is a British/international journal then it could be worth including a wider range of sources. The second comment relates to the justification of all the covariates which have been studied in this paper and if this could be more explicit in the introduction. For example, in paragraph 1 of page 6 I thought that the point around why childhood adversity may impact of help seeking could be developed further. Could it not be that those who have experienced adversity in childhood are more likely to have previous experiences of different health services and that this previous care and satisfaction with services could then impact on current use?

RESPONSE 2:

Thank you for the comment. I have made some adjustments to the text in the ‘Introduction’ section to associate some of the concepts to countries other than Canada.

REVIEWER 3, COMMENT 3:

Methods: There is a lot of description of the measures selected, and whilst this is needed to explain the different measures currently included, I wondered if some of them are necessary, particularly given not all are particularly discussed in the results section. It could also have been clearer that the mental health measures are assessed in individuals who are already self-identifying as having a mental health problem (I think that is the case, but I did have to go back and check a couple of times).

One of the outcomes which I wasn’t particularly clear on was the medication variable, so this should possibly be expanded on.

Regarding the model selection, then how were the parameters for exclusion chosen? i.e. the $p>0.25$ cut off.

The approach for model 6 using marginal standardisation should be described more clearly, as it is not completely obvious from the text how this modelling was conducted.

RESPONSE 3:

Thank you for your comment. I have made some minor adjustments to the description of the different measures. Unfortunately, the study was ambitious in its attempt to assess or control for a large number of measures and, as per reporting criteria, these do all need to be identified sufficiently. The 'medication' service type was prescription medication.

It was indicated in the 'Study Population and Sampling' sub-section of the 'Methods' section that the analyses in the paper were restricted to participants with an Afghanistan deployment that had at least one of six measured past-year mental disorders. I have additionally restated this at the start of the 'Statistical Analysis' subsection. It was also already included at the start of the 'Results' section that the individuals with an identified disorder comprised the study population but I have added a phrase to emphasize the point.

A little more detail has been provided in the 'Statistical Analysis' subsection. More description is provided to describe what is meant by marginal prevalence differences (i.e., covariate/model adjusted effect estimates) and a reference had already been provided for the interested reader. A little more description is provided around the model selection strategy, including its motivation. Within each variable group (i.e., military and sociodemographic, health-related, LTE and child abuse, pre- and post-deployment mental health training, and deployment-related variable groups), variables were sequentially dropped if their covariate-adjusted Wald Chiquare p-value was ≥ 0.25, starting with the variable having the largest p-value. Variables retained in a given model (variable group) were not assessed for exclusion in subsequent models (variable group assessments).

REVIEWER 3, COMMENT 4:

Results:

My main issue with the results is that there are multiple vast results tables, yet I didn't feel that the findings were adequately discussed in the text of the results, and so many readers may miss some of the key findings. I wondered if the tables can be reduced somewhat and more focus given to a restricted set of analyses.

One issue which relates to this is regarding the differences in the mental health of these populations, described in earlier tables, but I didn't feel this was adequately discussed and how it could impact on the outcomes. In the adjusted models I wanted to know more about how this could have explained the difference in whether help was sought and whether those with poorer functioning were more likely to be the regulars and so this could have (to some extent) explained the difference.

RESPONSE 4:

Thank you. It may not be ideal to reduce the tables containing the logistic regression analyses but I reduced them to just show the adjusted odds ratio for the 'component' covariate (Reserve Force vs. Regular Force) with each subsequent model and the marginal prevalence estimates which are based on the final model (model 6). I will propose that the original Tables 3, 4 and 5 be provided as supplementary material. Additionally, more description was added in the 'Results' section and the covariate influences on the outcomes that were identified through logistic regression are touched on with a little more detail.

The covariates for mental disorders and other health-related characteristics were included in the models to control for the influence that these covariates may have on each outcome. Together these variables characterize many different constructs that could differ between Regular and Reserve Forces personnel and that could partly explain any observed differences. There have been some studies that have reported differences in our outcomes with differing mental disorders, perhaps due to differences in level of distress or perhaps in individual perceptions of the disorder, treatment, etc. Although this is not fully discussed in the 'Introduction' section it is referred to and references were provided to support the idea for the interested reader. Some adjustments were made to the text in the 'Introduction' section to indicate the association of clinical characteristics and past experiences with psychological distress levels and the notion that past experiences may be a proxy for past encounters with mental health services.

REVIEWER 3, COMMENT 5:

Discussion:

I thought that the discussion was generally well written and informative, but wondered if a bit more could be said about the reasons why reservists may be less likely to seek care, beyond perceived need for care.

RESPONSE 5:

I have included additional text in the 'Implication' subsection of the 'Discussion' section to provide a brief overview of the barriers to care.

VERSION 2 – REVIEW

REVIEWER	Alexander Millner Harvard University
REVIEW RETURNED	25-Jun-2019

GENERAL COMMENTS	The authors addressed my concerns. I have no further comments.

REVIEWER	Laura Goodwin University of Liverpool, UK
REVIEW RETURNED	09-Jul-2019

GENERAL COMMENTS	This paper is much improved and I think the tables are now easier to interpret in the condensed version. The data analysis section has been expanded, which provides clarity, but I wondered if numbering or sub headings could help the readability of this section. It is worth thoroughly proof reading the article as there are some grammatical errors within some of the new paragraphs/sub sections. This includes the abstract which is clearer but would be improved with some minor wording changes.

VERSION 2 – AUTHOR RESPONSE

Reviewer 1 Comment:

The authors addressed my concerns. I have no further comments.

RESPONSE:

Thank you.

Reviewer 3 Comment:

This paper is much improved and I think the tables are now easier to interpret in the condensed version.

The data analysis section has been expanded, which provides clarity, but I wondered if numbering or sub headings could help the readability of this section.

It is worth thoroughly proof reading the article as there are some grammatical errors within some of the new paragraphs/sub sections. This includes the abstract which is clearer but would be improved with some minor wording changes.

RESPONSE:

Thank you, I have now added numbers to the subheadings that fall under the 'Outcome Variables' and 'Potential Confounders and Covariates of Interest' headings in the 'Methods' section. I have also made some adjustments to the wording in various sections of the paper.