**Author's response to reviews**

**Title:** Cross-sectional Prevalence Survey of Intimate Partner Violence Perpetration and Victimization in Canadian Military Personnel

**Authors:**

- Mark A Zamorski (mark.zamorski@forces.gc.ca)
- Miriam E Wiens-Kinkaid (miriam.wiens@mail.mcgill.edu)

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**Author's response to reviews:** see over
RE: Cross-sectional Prevalence Survey of Intimate Partner Violence Perpetration and Victimization in Canadian Military Personnel

Dear Editor:

Thank you for the opportunity to revise our manuscript entitled “Cross-sectional Prevalence Survey of Intimate Partner Violence Perpetration and Victimization in Canadian Military Personnel.” We appreciate the thoughtful and helpful comments of both reviewers. We have been able to address almost all of these in our revision. The text on the following page indicates how each of the reviewer’s concerns were addressed.

We hope these revisions meet with your approval.

Very truly yours,

Mark A. Zamorski
Head, Deployment Health Section
Reviewer 1:

Major Essential Revisions:

1. Are there cultural factors that the authors expect will differentially affect the prevalence of partner violence victimization or perpetration in this Canadian sample? If not, what makes this sample unique or important to study?

   This is covered quite extensively in the second through fifth paragraphs of the introduction: The second paragraph addresses factors that could influence the prevalence in military families. The third paragraph addresses possible differences in the phenomenology of IPV in military families (severity, frequency, duration, barriers, etc.)—we specifically mention possible effects of military culture and provide a couple of references on this facet of the problem. The fourth paragraph highlights the operational demands facing Canadian military families over the past decade and the potential linkage between deployment-related mental disorders and IPV perpetration. The fifth paragraph highlights the CAF’s approach to the prevention of IPV, which could have an influence on prevalence and other features of IPV. We did add an additional clause in the final paragraph of the Introduction to better link these points to the purpose of the paper.

2. A goal of the manuscript is to examine correlates, including mental disorders, to aid in identifying risk groups and contributing factors; however, mental disorders and “high risk” drinking were assessed within the last year, not for the length of the relationship. A limitation of this approach is that partner violence could have occurred with their current partner 15 years prior to the assessment and thus been separated from past year symptoms by a significant amount of time. Do the results presented here really illuminate risk groups or are these associations tangential at best?

   The second and sixth paragraphs of the Limitations section acknowledge this very important point in depth. We have added a couple of sentences to reinforce this.

3. Some counterintuitive findings were observed and warrant more discussion. Specifically, IPV perpetration was lowest among those with more recent deployments compared to those with more temporally distant deployments, and PTSD and depression were differentially associated with PTSD and depression despite being highly correlated. Please elaborate on why these associations may have been observed.

   We were also surprised by the deployment timing finding. We chose not to go further into its interpretation in light of problems with the messy recall periods and uncertain temporality of the events of interest. What we have focused on instead is simply the need for research that addresses these limitations (in particular, the need to take into account the timing of deployment). We also have
pointed out that the issue of timing could account for the inconsistency of findings on the association between deployment and IPV.

We have included a description of the extent of co-morbidity of PTSD and depression in the Results section to highlight this important point. While there was substantial co-morbidity, we did have enough individuals without co-morbidity that we could explore their differential effects—this is a strength of our study relative to others. We have added a comment explaining these differential associations in the third paragraph of the Implications section.

4. Is there a way to know if those who declined participation differed from those who participated on partner violence? How was the study advertisement worded?

   In the Methods section, we added in a comment about potential response bias and a comment on how the survey was framed to potential participants.

Minor Essential Revisions:

5. In the “Data Source” section, 2315 surveys out of 4385 surveys were returned, but only 2157 were usable. Please more clearly articulate the reasons for the differences in numbers (e.g., why were 158 unusable?).

   A comment on the reasons for unusable surveys was added in the Methods section. To optimize comparability with all of the other work using this data set, we elected not to impute the weighting variables reasoning that the percentage of eliminated cases is relatively small.

6. Were respondents compensated for completing surveys?

   No, they were not: A comment to this effect was added to the Methods section.

7. Has the survey instrument been validated in previous studies? How were the 10 acts of violence selected?

   Some additional detail and two references on this were added to the Methods section.

8. Is there information on length of current relationship or stability of current relationship?

   Unfortunately, no—this is mentioned in the second paragraph of the Limitations section.

How was current “intimate relationship” defined for participants?

   The specific wording of the question was added to the Methods section.
9. Because mental health questions were asked on a survey, it might be more appropriate to use the term “probable” when referring to PTSD and depression.

   We believe that the definitions should be clear enough in the Methods section to the readership familiar with survey methods; we have already included information on the test characteristics in that section. We feel it would be unnecessarily cumbersome to reinforce this limitation each and every time. We have, however, emphasized this in the Limitations section.

10. In the analysis section, why was listwise deletion used to handle missing data instead of multiple imputation? This practice could bias estimates by not permitting full use of available data.

   We have changed the analysis approach to use multiple imputation to address this concern. We have elected to present most of the tables with unimputed values in order to give readers a sense of the extent of missing value for each variable—this is made clear in the footnotes to each table. We have used the imputed values where they matter the most, which is in the final regression models where listwise deletion had resulted in the elimination of hundreds of cases.

11. Explain the Archer and Lameshow criteria for goodness of fit.

   Additional detail was added to the Methods section to explain this.

12. What was the cutoff for the tolerance and variance inflation factor?

   This detail was added to the Methods section.

13. On pages 11-12, when describing prevalence estimates and using terms like “larger fraction” it is important to include a referent (e.g., “emotional or financial abuse was seen in a larger fraction” than what?).

   A comment was added to clarify this.

14. When describing correlates on pages 12-13, the term univariate is used; the proper term for relationships between only two variables is “bivariate.” “Univariate” should be used when describing a single variable (e.g., the mean for PTSD severity). If referring to odds ratios, the terms unadjusted and adjusted can be used.

   This terminology was changed throughout.

15. On page 18, citation 33 refers to the NISVS, which did NOT measure perpetration, but the cite is used to support that women perpetrated more violence. Please revise.
Reviewer 2:

Major Essential Revisions:

1. While the authors have included both men and women in the sample, there needs to be a more detailed discussion of gender and IPV, and how the study findings can be interpreted in light of the literature on this topic. Please see Langhinrichsen-Rohling (2010) and Johnson (2010) for discussion of the controversies in this literature. Specifically, the authors should address how the survey instrument, time period for reporting of IPV, and limiting the response to the current relationship may impact the study findings. The authors should also address issues related to common-couple violence versus intimate terrorism (Johnson 1995, 2010), bi-directional violence, and acts of self-defense. I would also suggest showing perpetration/victimization items shown in Table 3 and in figures 1 & 2 stratified by gender.

The requested gender stratifications have been included and are mentioned in the results and the discussion. The Limitations section was substantially reinforced to reflect the concerns above.

The issue of gender and IPV is obviously a complex and controversial one, and it is one that may have special salience in the military. We also accept that given that BMC Public Health has a broad readership, the average reader may not grasp the nuances of the issue. Nevertheless, we did not want to get too deep into this, given its complexity and given that the in-depth exploration of gender differences in IPV in the military was not an objective of our study. We added some additional commentary on this in several places. We also eliminated any mention of gender-related issues in the abstract, reasoning that these could be more readily taken out of context there.

We hope that we have struck the right balance and captured the most important point, which is that our findings should not be interpreted to mean that gender is irrelevant to IPV. It is equally important, though, that readers understand that male military personnel are victims of IPV as well, that not all of that is likely accounted for by defensive behaviours by their female intimate partners, that less physically injurious forms of IPV are also an important public health problem, and that male IPV victimization has important consequences of interest to the military employer.

2. In Table 8, the N varies across all models. If because of missing data, were there any significant differences in demographic characteristics between those included and those excluded from the model? Also in Table 8, the authors excluded variables from the model if they did not have a univariate relationship
with the outcome. This approach does not take into account possible mediating or moderating effects of these variables, appears to change the sample size across models, and limits the comparability of models, as different covariates are included in each of the four models. A suggested alternate model building approach would be to include the same set of variables in all four models, and include variables, such as marital status and high-risk drinking in final models for theoretical reasons, even if univariate relationships are not statistically significant.

We addressed this concern by using multiple imputation to address the missing data that was driving the different N's in the different models. However, even after imputation, two of the models had poor fit when all variables were included. We have judged that the advantage of consistency of variables included from model to model is not worth the disadvantage of drawing conclusions from a poorly fitting model. Hence, we elected to delete the extra variables contributing to poor fit in two of the four models. Of note, the pattern of significant associations did not change when the extraneous variables were excluded.

3. The first sentence in the final paragraph - “we did not find evidence of an epidemic of perpetration of severe forms of physical abuse by recently deployed male personnel on their civilian spouses” is problematic. The authors note the limitations to the study, particularly related to the relationship between IPV and deployment, and the potential for confounding. Under-reporting may have been possible due to self-report of perpetration, survey mode (paper), and the unspecified recall period. Also, this research did not interview partners of deployed male personnel about their experiences of violence. It is quite possible that recently deployed male personnel perpetrate severe forms of physical abuse against their intimate partners but the methodological limitations of the current study, as described by the authors, were limited their ability to address this particular research question. As Johnson (2010) notes, situational couple violence "dominates general survey data, because of the biases of so-called representative survey samples. These biases arise from the little-noted high rates of refusal in survey samples - 40 percent in the much-cited National Family Violence Surveys (Johnson 1995). Because intimate terrorists and their partners refuse to participate in such surveys, the former because they do not wish to implicate themselves, the latter because they fear retribution from their partner, general social survey data include almost not intimate terrorism or violence resistance (Johnson et al. 2008)." (p. 213).

We have greatly expanded the Limitations section to address these points, some of which were also made by Reviewer 1. We have also nuanced the Conclusion to reinforce these limitations.

Minor Essential Revisions:

4. How are the authors defining IPV? The term IPV is used interchangeably with the term “family violence” which is also not defined.
References to “family violence” were changed to IPV where needed to enhance precision. A working definition of IPV was added to the Methods section.

5. Please provide any information on the reliability and validity of the instrument used to measure IPV.

Additional material and references on the IPV measure (also requested by Reviewer 1) was included in the Methods section.

6. From the information provided in Table 3, there appear to be very few responses to the sexual IPV item (forced into unwanted sexual activity) but exact numbers are not provided because they were so few in number. While my assumption is that responses to this item were included in the construction of the any physical or sexual IPV variable, this is not clear from the text. This raises a related concern regarding the use of the terms “physical and sexual IPV,” “physical or sexual IPV” and “physical/sexual IPV” – all are used by the authors. If the sexual IPV item was included in the construction of this outcome, then “physical and/or sexual IPV” would be most appropriate. Considering the small number of positive responses to the sexual IPV item, however, I would suggest focusing only on physical IPV as an outcome, noting in the text the small number of responses to the sexual IPV item as a rationale for focusing only on the prevalence and correlates of physical IPV.

A clarification on the inclusion of sexual abuse items in the aggregate outcome was made in a footnote to Table 3. While we see the argument for excluding the low prevalence abuse items such as sexual abuse from the aggregate outcomes, we could make the same argument for other low-prevalence items, ending up with a messy outcome like “any higher prevalence physical and/or sexual abuse perpetration.” We also suspect that the exclusion of these cases would open us to criticisms of not including the most serious cases in the analysis. Regardless, this issue is essentially moot because very, very few individuals endorsed ONLY sexual abuse perpetration but no other forms of abuse perpetration. We have, however, beefed up the Limitations section to make it clear that the findings are driven by the higher prevalence/lower severity forms of abuse; this issue was also raised by Reviewer 1, underlining its importance. Finally, we have changed the terminology to physical and/or sexual and to emotional and/or financial abuse throughout.

7. Please include the total sample size in all tables. Also, please address more clearly how missing responses were handled.

Total N has been included in all tables. Analysis was revised to use multiple imputation for missing values.

8. Relationship between marriage and IPV. Since the questionnaire assessed
abuse over the life of the current relationship, it is possible that those who were
married had, on average, a longer length of the current relationship than those
who were not married. The (possible) longer exposure period for married, as
compared to unmarried, individuals may account for the finding of higher risk of
experiencing physical and/or sexual IPV

This is a useful interpretation that had not occurred to us—we have added a
comment to that effect in the Discussion.

2. 66% of sample is currently married, yet the authors refer to “understanding
their perpetrating spouses will aid . . .” (Implications, p. 25) and “we did not find
evidence of an epidemic of perpetration of severe forms of physical abuse by
recently deployed male personnel on their civilian spouses” (Conclusion, p. 27). In
both instances, the perpetrators and those experiencing violence could be
intimate partners as well as spouses.

We changed the terminology to “intimate partner” throughout.