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Title	Military-Related Sexual Assault in Canada: Prevalence, Circumstances, Correlates, and Associations with Mental Disorders
Authors	Kimberley Watkins, MA; Rachel Bennett, MSc; Mark A. Zamorski, MD, MHSA; Isabelle Richer, PhD
Reviewer 1	Dr. Carolyn Joy Sachs [permission granted but wants to correct typos. Will provide corrected version]
Institution	UCLA Emergency Medicine Center, Los Angeles, CA, USA
General comments (author response in bold)	This paper addresses an important topic and gives new information on the prevalence of sexual assault in the Canadian Military. The paper is well written and the methods seem appropriately executed. I applaud the Canadian Government for supporting this research and exploring the issue of military personnel well being and the authors for producing this excellent and important work. I have several general concerns that if addressed have the potential to greatly increase the impact of the information. 1) The main issues are how do medical providers and the military use this information? I submit you can make much broader conclusions and recommendations to immediately institute a sexual assault prevention program among ALL military personnel. **We share this sentiment, but we feel that this goes beyond what is appropriate in this paper, which did not address the effectiveness of prevention programs. Such arguments are better suited for the planned editorial that is to accompany the paper. As well, it is worth pointing out that such action has already been undertaken—we now allude to this at the very end of the paper, and we have added specific mention about prevention activities…" establishment of an independent Sexual Misconduct Response Centre and initiation of prevention initiatives." Additional detail on the specifics of the CAF's response might be worked into the proposed editorial.** 2) The most astounding finding from the paper was not the prevalence of sexual assault among the female military was the fact that the 16% of women were assaulted BY OTHER MILITARY PERSONNEL. This is a call to action. These perpetrators of sexual assault are not "the general population" they are paid representatives of the Canadian government who for the most part are paid to protect the population and serve the Nation. They need to be held to a higher standard. At least one in 6 women joining in the Canadian Military can expect to be sexually assaulted by another military member. Canada (and from the literature review the United States) must to more to stop their own government representatives from this misogyny. The authors explore some of these ideas in the supplemental part of the paper but they need to be in the main publication. **We share the reviewer's indignation, but we do not feel that our research paper is the right place to express it. Indignation may motivate and catalyze prevention and control efforts, but it does not provide a reliable roadmap to these ends. This is why we have alluded instead to the unique opportunities for prevention and control afforded by the military environment. Again, the proposed editorial will provide ample opportunity for discussion along these lines.** Specific Comments: 3) Methods: well done face to face interviews involving much work and funds. Limited questions about sexual assault, in particular I would have liked to know at what point in the military service the assault took place to be able to risk adjust by years of service the yearly risk of suffering sexual assault. **We would have loved to do so—but this valuable information was not captured in the survey. This is very hard content to cover in asurvey as there are often multiple events over time, with some of them occurring long in the past.** 4) The face to face method is likely to under estimate sexual assault as it was administered by military representatives and subject are in general less likely to reveal mental health issues and victimization that may appear to make them seem "weak" given they choose to join the military. Please review the literature on face to face disclosure of assault by military versus other methods and list as a potential limitation. **This issue pervades all research on sexual assault victimization. We have added in some content along these lines to the Limitations section of the Discussion: "On the other hand, face-to-face interviews offer advantages in terms of response rate (80% in the case of the data collected for this paper); this response rate is much higher than that reported in population-based surveys in the US military, 24 – 34%). The WMH-CIDI PTSD model is constructed and administered in ways that facilitate disclosure of sensitive events. It is possible, conceptually, that the military environment may be associated with greater reluctance to report trauma and mental health concerns. However, previous analysis of the present data in conjunction with data from Canadian civilians strongly refute this concern: the prevalence of common mental disorders and of child abuse victimization were both significantly higher in military personnel." We have added in additional references to these points. We would also point out that the interviewers were employees of Statistics Canada and hence not "military representatives." We believe that the exceptional response rate speaks to the importance the respondents attached to the subject of the survey and to the confidence that the respondents had in the confidentiality of their responses.** 5) I have a question pertaining to the methods of the broader study: when subjects disclosed sexual assault and other mental health issues were services offered to treat these? I suspect with a face to face interview services were offered immediately after disclosure and that institutional IRB would mandate this. Please discuss. **These are legitimate concerns. Statistics Canada has a series of entities that fulfill the function of an IRB, ensuring that participation is voluntary, that risks of participation are managed, and that those with critical responses are dealt with appropriately. StatCan has policies, practices, and procedures in place to manage these issues, which arise in many of their health surveys. For our survey, it was made clear to respondent at the time of consent that no action would be taken as a consequence of their responses. All participants were provided a handout that identified key local resources for getting help if needed. While this issue (that is, how survey researchers address the disclosureof information that might merit clinical intervention) is important, we do not feel it is central enough to the thrust of the present paper that it needs to be elaborated upon within it.** 6) Page 13 and throughout the paper: you refer to the PTSD, mood and anxiety disorders as "mental disorders". I find this somewhat pejorative as many other conditions may be conjured up in readers under "mental disorders" and some readers may think that those without "mental disorders" would not be sexually assaulted in the military thereby perpetuating a sore of "victim blaming". **We understand that some object to the use of the term "mental disorder" in this context (for the reason the reviewer points out). Others object to the use of these terms in any context, for related reasons. It is for this reason that the CAF uses the term "Operational Stress Injury" as opposed to PTSD when communicating with our members and with veterans. This term is viewed as less stigmatizing.** **The present paper targets a clinical and scientific audience, for whom the use of precise terms (as documented in standard clinical references such as the International Classification of Diseases and the Diagnostic and Statistical Manual of the American Psychiatric Association) is absolutely essential. We have therefore retained our initial terminology.** Please clarify exactly what categories of mental health were studied (I suspect this is depression, anxiety, and PTSD) and not psychosis or personality disorders. **This is already addressed in the first line of the Measures subsection of the Methods section: "We assessed lifetime and past-year prevalence of mood and anxiety disorders (an aggregate variable of major depression, PTSD, generalized anxiety disorder, or panic disorder).**

	Also what is the presence of these same disorders in the general public? Since you mentioned that the overall prevalence of SA in the general public does not differ much from those in the military you should do the same for these specific disorders. **We did not set out to compare, in this paper, the prevalence of mental disorders between military personnel and civilians, so this feels off-topic to us. This is a complicated issue that we have published on in the past, and it really cannot be done justice in the confines of the present paper.** 7) Please don't dismiss "unwanted touching" with statements like "although the majority of it was unwanted touching"; this is sexual assault and is a crime with tolls on the victim. In keeping with the comments earlier this should not be minimized, tolerated, or perpetrated by paid government representatives against other CAF employees. **We share the substance of the reviewer's concern, but we were unable to locate the offending line in our text. Instead, we say in the Results: "MWSA consisting of unwanted sexual touching was more prevalent than that of forced sexual activity among both men and women. " And in the Discussion: "The majority of this sexual assault was unwanted sexual touching, as opposed to forced sexual activity." These are simple statements of findings, and we were careful to attach the word "sexual assault" to them to emphasize that unwanted touching is a form of sexual assault. We did, however, feel it important to distinguish between these two forms of sexual assault because they are treated differently under the law in Canada and because they have differential health impacts.** 8) Page 17; you present interesting data and report the P value as less than 0.01 for Rank Category and risk of SA yet there is no discussion. Why do you think there was a difference and did you expect one a priori in one direction or the other? **We did not go into this finding because we measured rank category at the time of the survey, not (necessarily) at the time of MWSA. As such, it has a complicated and ultimately unsatisfying interpretation. The rationale for its inclusion was that it was a potential confounder of the relationship between sexual assault and mental health problems.**
Reviewer 2	Jodi Marie Samantha Gatley
Institution	University of Northern British Columbia, Northern Medical Program, Prince George, BC; and Centre for Addiction and Mental Health, Toronto, ON
General comments (author response in bold)	This study makes an important contribution to the relatively small literature on sexual assault in the Canadian Armed Forces and also importantly stratifies analyses of sexual assault and sexual harassment outcomes by gender. It also provides striking estimates of prevalence of lifetime sexual assault during military service, especially among women. Some minor structural edits have been suggested, as well as more substantial critical commentary on the analyses. 1) P. 4 Paragraph 1: The prevalence estimates for sexual assault and the evidence to support the association to PTSD are based on a study specific to the CAF population rather than the general Canadian population. The population that these findings are based on should be identified for the reader to clarify this. It would also be informative to contrast estimates for the CAF population to that of the general population here if possible. **We believe this comment pertains to our Reference #1, a 2008 article by Van Ameringen et al. The reviewer appears to be under the impression that that study targeted a military population, but that is not the case: It is a general population study and we identify it as such. We have reinforced this message by clarifying: "….in the Canadian general population" in the first sentence of the introduction.** 2) P.5 paragraph 4: The response rate by gender to the CFMHS should be presented. **This is a relevant question, but unfortunately StatCan did not provide gender-specific response rates for the 2013 survey. It may reassure the reviewer to know that the very similar 2002 Canadian Forces Supplement to the Canadian Community Health Survey Cycle 1.2 showed identical stratum-specific response rates for men (75.9 – 87.3%) and women (75.3 - 87.1%) and that the weights in both surveys were, in any case, adjusted for non-response.** Data source: 3) Given that providing estimates of the prevalence of lifetime sexual assault among CAF members is a central aim of the study, more detailed information should be provided about the anonymity of the CFMHS. It is stated that some descriptive information such as rank and deployment history were also collected, is it possible that respondents would be considered about the risk of identification because of these details? **We acknowledge self-report as a key limitation, and we have beefed up this content in response to Reviewer 1's related concerns. It might interest the reviewer to know that there was a separate consent step forlinkage of the administrative data that was most likely to identify an individual; 99.9% of respondents gave their consent. As noted earlier, the high response rate and significant rates of various problems reported provide some reassurance that respondents had faith in the confidentiality protections.** Design and analyses: 4) The primary research question regarding the reason for examining relationship between MWSA and non-MWSA to PTSD and mood disorders is not clearly stated. As it is a cross-sectional study, a compelling case is not made for the importance of these results, given that temporality and directionality are unknown. **We believe that the justification for the exploration of differential associations between MWSA and non-MWSA is clearly articulated in the Introduction: "Some aspects unique to MWSA, such as the feelings of betrayal and continuous exposure to the perpetrator when he or she is a unit member, may have a disproportionate influence on mental health." We provide some references in which these issues are explored in more detail. We do acknowledge in the Discussion that the cross-sectional nature of the data is a key limitation: "The study was cross-sectional in design, so we could not determine the direction of the association between sexual assault and mental disorders."** 5) A major concern with the analyses is that the authors do not consider the potential role of alcohol consumption in sexual assault. Alcohol consumption is strongly associated to both sexual assault victimization and perpetration, see (Abbey A, Zawacki T, Buck PO, Clinton AM, McAuslan P. Sexual assault and alcohol consumption: what do we know about their relationship and what types of research are still needed? Aggression and Violent Behavior 2004;9(3):271-303.) Alcohol abuse likely has a bi-directional association with PTSD, traumatic life events, and mood disorders. Thus alcohol abuse and dependence may confound the associations between sexual assault and PTSD, traumatic life events, and mood disorders, and should be included as an independent variable for logistic regression models. **The point about the role of alcohol use in sexual assaults in an important one—it certainly has a complex relationship with the outcome and with the other co-variates, as the reviewer points out. But for that reason, it might be hard to interpret the effect of alcohol problems, as we measured them, on the model—they are more than a simple confounder, and their inclusion in our analysis could easily amount toover-controlling for a factor that lies on the casual path between the exposure and outcome of interest. We have added (to the Limitations subsection) an allusion to the relevance use of alcohol and drugs at the time of the event as being useful for prevention and control efforts: "For example, we did not assess the use of alcohol or drugs (including the surreptitious use of incapacitating "date rape" drugs) around the time of MWSA events."** 6) The CFMHS collects measures of past year alcohol abuse and dependence, and if possible the authors should conduct analyses for MWSA and non-MWSA outcomes stratified by victims' alcohol abuse and dependence status. **Given that we have not measured alcohol use or dependence at the time of the MWSA or other non-MWSA events, such results would have an unclear interpretation. As well, the cell sizes would quickly get untenably small, given the low prevalence of alcohol use disorders in the women**

	in the sample. 7) A key limitation of the study that the authors should acknowledge is the inability to measure incapacitated sexual assault, where the victim was unable to resist unwanted sexual advances due to incapacitation by alcohol, drugs or otherwise. About 1/10th of sexual assaults in Canada in 2014 were incapacitated sexual assault, see (Perreault S. Criminal victimization in Canada, 2014. 2015;Juristat. Catalogue no. 85-002-X) and it may account for up to half of sexual assaults among young adults, see (Carey KB, Durney SE, Shepardson RL, Carey MP. Incapacitated and forcible rape of college women: prevalence across the first year. J Adolesc Health 2015 Jun;56(6):678-680.) **This is an interesting and important issue, and it is likely at least as relevant to phenomenology of sexual assault in military women as it is in other women. However, we have absolutely no data in our study that shed any light on this, nor do we have any data from other sources on its incidence or prevalence in the CAF. As such, discussion of it feels outside of the scope of our paper. We have, however, alluded to this issue in the Limitations subsection, as noted above.** 8) The authors find an elevated risk of MWSA among women who have been deployed but do not discuss the relationship of deployment to hazardous drinking. Studies suggest high levels of hazardous drinking by US military personnel during deployment and afterwards. See [Thomas JL, Wilk JE, Riviere LA, McGurk D, Castro CA, Hoge CW: Prevalence of mental health problems and functional impairment among active component and National Guard soldiers 3 and 12 months following combat in Iraq. Arch Gen Psychiatry. 2010, 67 (6): 614-623.; Bray et al. Substance Use and Mental Health Trends Among U.S. Military Active Duty Personnel: Key Findings From the 2008 DoD Health Behavior Survey. Military Medicine 2010, 175 (6): 330-339.] Also, the lack of information from the CFMHS about respondents' alcohol use during deployment is a limitation that should be discussed as it may have contributed to the association of deployment history to MWSA victimization. **As noted above, we have nuanced our interpretation of the linkages between deployment and MWSA, given the somewhat divergent findings showing that a surprising number of women with MWSA reported that one or more events during deployment but that women who had deployed did not have an elevated risk for MWSA, after adjustment for confounding factors.** **Certainly US research has clearly pointed to a relationship between deployment (specifically combat deployments) and post-deployment high-risk drinking. Canadian findings have differed, for reasons we still do not understand. In 2014, Pearson, Zamorski, and Janz (http://www.statcan.gc.ca/pub/82-624-x/2014001/article/14121-eng.htm) noted that CAF personnel who deployed in support of the mission in Afghanistan had a lower risk of alcohol use disorders than those who had not so deployed. Boulos and Zamorski (http://www.cmaj.ca/content/185/11/E545.long) found a surprisingly low prevalence of alcohol use disorders among personnel with deployment-related mental disorders (largely PTSD).** **Also, for the vast majority of CAF personnel who participated in the survey and had deployed (largely in support of the mission in Afghanistan), access to alcohol during deployment was extremely limited—these deployments were largely "dry." So while we acknowledge the linkages between deployment and later drinking behaviour in some military populations, we do not believe that this likely accounts for the potentially elevated risk for MWSA during deployment.** 9) It would be informative to provide odds ratios for MWSA and non-MWSA among those who had been deployed compared to those who had never been deployed, given the prominence of MWSA during deployment found among women. **This is an interesting line of inquiry. Unfortunately, the small number of women with MWSA in the sample and Statistics Canada's stringent standards for release of findings pertaining to small numbers of respondents precluded this comparison.** 10) It is also important to address the significance of the limitation that those who had experienced both MWSA and non-MWSA could not be identified. Prior sexual assault victimization is a known risk factor for subsequent sexual assault victimization, and more research is needed to identify risk factors for this high risk sub-group of the population with the goal of informing prevention. **We highlight this limitation in both the Methods section and in the Limitations subsection of the Discussion. We have added in an additional mention of its significance in the Limitations subsection: "We could also not determine, among those with MWSA, how much (if any) non-MWSA they had experienced; this is especially important given the role that prior sexual assault may play in subsequent events."**