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<tr><td colspan="2">Article details: 2017-0153</td></tr>
<tr><td>Title</td><td>The association between child abuse and suicidal ideation, plans, and attempts in a sample of Canadian public safety personnel: a cross-sectional survey</td></tr>
<tr><td>Authors</td><td>Sarah Turner MSc, Tamara Taillieu, R. Nicholas Carleton PhD, Jitender Sareen MD, Tracie O. Afifi PhD</td></tr>
<tr><td>Reviewer 1</td><td>Dr. L. Reifels</td></tr>
<tr><td>Institution</td><td>Centre for Mental Health, University of Melbourne, Australia</td></tr>
<tr><td>General comments (author response in bold)</td><td>This is an interesting and well-written manuscript which outlines a clearly delineated study rationale that is embedded within the wider literature. It presents data from a cross-sectional study involving a large sample of Canadian public safety workers, which are examined through logistic regression analysis. Key study findings highlighting both the prevalence and role of child abuse histories in relation to lifetime suicidality among this cohort are of relevance to informing future research and public health measures.

My comments to further improve the manuscript are relatively minor and as follows:

ABSTRACT
1. Results: could indicate that no additive or interaction effects were detected.

We have added that no cumulative or interaction effects were found to the abstract on page 2.

INTRODUCTION
2. First paragraph: the acronym ‘RCMP’ should be spelled out in full at first use

We have made this correction in the Introduction.

METHODS
3. Data and sample: please indicate how long it took participants to complete the survey

The median time participants took to complete the survey was 1.3 hours. We are reporting the median because some respondents did not log out and back in when they left the computer to complete the survey at a different time and, therefore, their clock did not pause. There are some values that are very large (longest time to complete the survey was 1,941 hours), which is not an accurate reflection of the duration of the survey. We did not exclude outliers because there was not a clear gap in the data that could signify those who did not log out when they were away from the computer.

4. Child abuse: I understand that child abuse measures have previously been validated, but find it a bit bizarre that physical abuse questions 2 and 3 contain similar content (and even one overlapping/identical term ‘shove’) – a minor point, but could you comment

We thank the reviewer for noticing this error on our part. The third child abuse question does not include the word ‘shove’. We have now made this correction in the manuscript.

5. Child abuse: the fact that child ‘exposure’ to interpersonal violence involved the child ‘witnessing’ IPV rather than IPV being directed at the child could be made more overt in the item description (whereas the actual question is clear on this).

We use the term “exposure” rather than “witnessing IPV” in the description because it more accurately describes a child experience of witnessing or hearing IPV in the home. If we used the term “witnessing IPV” instead of “exposure”, we would be excluding those who did not physically see IPV, but heard it happening. This is still a harmful experience and is included in the actual question itself. Importantly, the term “witnessing” is no longer used in the child maltreatment literature. It is considered a less accurate and dated term. Therefore, we maintained our use of the term exposure to IPV in the paper.

6. Statistical analysis: please indicate the statistical analysis software used
We have now included this on page 8.

RESULTS
7. It would be useful to include some basic information on the prevalence and frequency of CRTs in the PSP sample. Given that CRTs essentially constitute ‘professional hazards’ for PSP, one would imagine that the CRT prevalence would be rather high.

Thank you for this comment. We have added the prevalence of any career-related trauma among each of the public safety personnel types to table 2. The reviewer is correct that the prevalence is high: 94% of public safety personnel have experienced one or more career-related trauma.

DISCUSSION
8. Study limitations: should include a brief critical reflection on: a) the reliability of retrospective (lifetime) self-report data; and b) the fact that lifetime data did not permit temporal inferences regarding the timing of suicidal behaviour (the Discussion refers to evidence of suicidality in ‘adulthood’, which whilst plausible, may also be slightly overstepping the conclusions that can be reached based on lifetime data)

We agree with the reviewer that these details are important. We have revised the limitations section on page 12 and have removed the mention of adulthood suicidality in the interpretation and conclusion.</td></tr>
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	9. To better gauge the likelihood of possible sample bias due to selective drop out it would be useful to compare the demographic profiles of those who only partially and fully completed the survey. **Thank you for this comment. To test if there are differences between those who completed the survey and those who dropped out, we conducted chi square analysis and separate logistic regression analysis for sex, age, education, marital status, ethnicity and provincial region as the independent variables, and missing data (yes/no) as the dependent variable. The results of these sensitivity analyses show no differences in missing data based on sex, marital status, and ethnicity. Those with older age and more education had decreased odds of having missing data. Living in Atlantic or Eastern Canada compared to Western Canada resulted in increased odds of missing data. Otherwise no differences were noted.** TABLES 10. Tables 2, 3, 4: could omit '%' symbol from cells (as it features in column headings) **We have removed the % from tables 2, 3, and 4.** 11. Tables 2 and 4: the use of 'different lettered superscripts' to indicate significant differences varies between Tables 2 and 4, which creates some confusion. Further clarification may be required to facilitate interpretation. While its use in Table 2 is intuitively clear, the related footnote underneath the Table does not seem to accurately reflect its use. By contrast, the Table 4 footnote seems to be correct, but the varying use of 'different lettered superscripts' in the table does not become immediately clear to the reader. A likely reason being that superscript letters in Table 2 refer to actual categories, whereas in Table 4 they do not (and it is their mere difference that matters). **We have added further clarification to the footnotes for both Table 2 and 4. For clarity, we want to explain that the same analysis to test for differences between categories was used for Table 2 and 4. The only difference is that in Table 2 the odds ratios are not presented because there is no meaningful reference group. In Table 4, the odds ratios are displayed with the reference group of no child abuse/ career related trauma**.
Reviewer 2	Dr. Paul D. Hasselback
Institution	Island Health Medical Health Officer, Nanaimo, BC
General comments (author response in bold)	This is an important Canadian study with international implications and it will be a good publication for CMAJ to consider. I have more praise for the paper, the style of writing and the approach that was used which might not seem apparent in this review. While there are issues associated with on-line surveys, the main issue for this paper is the need to revisit and correct some of the analytic methods and how they are presented. It is predominately a matter of language for words and how the results are interpreted. A significant concern that must be addressed before publication is the supposed logistic regression. Fundamentally the study would benefit from the input of an epidemiologist or statistician to review the methodology of analysis and how it is presented. Regrettably, when about 90% completed this review, a significant power failure may have lost a few final comments. Specific issues: 1. The communicating author has a good track history in work on measurement of childhood abuse and subsequent adult maladaptive behaviours including suicidal ideation. This work shows in the background, methods and approach to the study. **We thank the reviewer for this positive comment. Of note, the communicating author is also an epidemiologist. The methodological approach used in this paper is similar to dozens of her publications in journals to date.** 2. Page 6 line 40-53: The progression to completion of the survey should not be considered relevant. What would be more helpful here is some sense through the partnering agencies of the numbers of active employs nationally from whom the survey would be available. Eg Stats Can reports on numbers of police officers at http://www.statcan.gc.ca/tables-tableaux/sum-som/l01/cst01/legal05a-eng.htm at some 68,000 and firefighters are in the range of 25,000 career firefighters (and unclear from the survey is volunteer firefighters were eligible for the survey). I would expect other estimates of numbers reached would be more beneficial to include. **We have now included the approximate number of public safety personnel in Canada that may have been exposed to the solicitation for response to the survey in the methods on page 5.** 3. The relatively low proportion of active career members responding to the survey does not negate the excellent work, but does should be reported as such, and the potential for bias in overreporting by those who wished to disclose childhood abuse should be considered. Nonetheless the richness of the 4200 respondent survey provides very valuable information. **Although our sample includes a small proportion of all public safety personnel in Canada, the sample**

	size was reasonably large. We have included the limitation of a convenience sample on page 12. We are not able to determine if over or under reporting of child abuse histories occurred; therefore, we have also added the limitation of the self-report, retrospective measure of child abuse. 4. Page 7 the use of "type" of abuse is relevant to the study. Physical abuse is measured through any of three items. It is unclear after reading this what were "types": physical, sexual, EIPV, and not clear if exposure to sexual abuse was coded as seperate "type" or as a component of EIPV. **We would like to thank the reviewer for pointing out that this was not clear in the manuscript. We have now added clarification to the child abuse measurement description on page 5-6. Physical abuse, sexual abuse and exposure to intimate partner violence as a child were all coded as separate child abuse variables.** 5. Page 8 line 23 CRT – the statement that there were no missing data cannot be verified. It is reasonable to note that respondents were coded for only responses indicating they had experienced one of the events. **Based on the reviewer's comment, we have now removed this statement from the manuscript.** 6. Sociodemographic variables – these are listed as "covariates". Technically a covariate is something that may be predictive of the outcome of interest, in a cross sectional survey there is no "outcome" but there are associations between variables. This study is predicated on an assumption of temporal relationship from child to adult experiences and the variables provided are of interest (eg persons with child abuse history are more likely to have relationship problems later in life). **We agree with the reviewer that cross sectional analyses measure associations between variables and cannot determine causation. However, when computing statistical models, independent and dependent variables must be assigned in the modelling statements. Based on the above logic noted by the reviewer we agree that there is an assumption of the temporal relationship wherein childhood experience may be associated with the likelihood of experiences in adulthood. The use of the word 'covariates' in epidemiological cross-sectional studies is commonly used to describe additional independent variables that may confound the relationship between the main independent variable and the dependent variable of interest. We are simply interested in adjusting for the effects of these variables. That being said, upon the reviewer's request, we have removed the word 'covariate' from the methods section of our paper and replaced it with 'variable'.** 7. Statistical analysis: It is here that the need for more rigorous statistical review is recommended. At times as the reviewer I could not determine what analysis actually was undertaken or how it was performed. The word stratification does not appear to have been used correctly. For example the 2 by 2 table of gender by abuse history – there is no stratification here merely the odds ratio calculation that females are 1.12 times more likely to report abuse than males. Rather than state the odds ratio is 1.0, this can sometimes be called the referrant group, and is of importance of calculations were limited to 2 by 2 analysis (eg for variables with 3 categories). Another term that is something used is "odds" and not correctly utilized. **We would like to thank the reviewer for bringing to our attention that we did not clearly communicate the methodological approach used in the manuscript. We have now reviewed the manuscript to improve clarity. The reviewer is correct we did not stratify the analyses by gender. This was an error in our communication of the findings. We have now removed the word stratified from our statistical analysis and results sections and have added numbered steps that correspond to the analyses for each table in the statistical analysis section on pages 7-8. In addition, we have added a footnote to clarify that an odds ratio of 1.00 is the 'reference group' in tables 1 and 4.** 8. Discussion – The authors appropriately are conservative in their interpretation of the rich data set they are analyzing. The important relationship between suicidal behaviours as the dependent variable and the CRT and abuse histories as the independent variables should be presented as the key finding. In this scenario, the other variables are covariates. I am appreciative of the effort to determine if there is any interaction in the independent variables as this would have clinical implications – however, reversion to more plain language that "an interactive effect of abuse and CRT experiences was tested for and not found to be significant" might suffice. **Based on the reviewer's comment, we have now simplified the language in the results and the interpretation sections. We did not mention the non-significant interaction effect in the interpretation section due to a limited word count and our effort to highlight the most important findings; however, it is stated in the results section on page 10.** 9. Conclusion – buried in this manuscript is the key finding that any suicidal behaviour in PSPs is most closely associated with past child abuse and not on professional trauma experience. This is a critical finding in relation to employee programming for these professions as it is in child abuse prevention programming. Unfortunately the key finding here is lost in the jumble of analytic procedures. **We would like to thank the reviewer for this important comment. We have now revised the conclusions accordingly to highlight the key finding of this work on pages 12-13.**

	10. Table 2 – this is a complex table and might benefit from simplification and re-presentation. I suspect a statistician would recommend using other than a basic chi-square analysis for comparison across profession. Note the analysis of the number of types of abuse are non-mutually exclusive and it is unclear what is the referent group here. **The reviewer is correct in noting that a basic chi-square analyses would not be the best for analyzing the data in Table 2. The challenge with this manuscript is that we have so much data to present we have tried to present the data with richness, yet without being unnecessarily complex. That being said, the data in Table 2 focuses on the prevalence of child abuse by public safety personnel category. To determine statistically significant differences, we used logistic regression models rather than chi square. We have added a footnote and made adjustments to the headings of the table to clarify the analysis that was conducted in table 2. For each child abuse type, we conducted different six logistic regression analyses, one with each public safety personnel category as the referent group. Only significant differences at p<=0.05 were noted in the table. We would like to clarify also that the 'number of types' child abuse variable is mutually exclusive. If a respondent had two or more types of abuse, they were not included in the 1 type category and vice versa.** 11.Table 3 – This table would also benefit form simplification. The use of the word "adjustment" is not explained, was this through true stratification, ANOVA/ANCOVA, or using a form of logistic regression – if so, how was the regression undertaken? It is not clear who the referent group to which the odds ratio is developed. I can only assume the progression of suicidal behaviour is not exclusive and there might be interest is analyzing the "severity" component. **Based on the reviewer's comment we have now added more detail to clarify these analyses in the statistical analysis section on pages 7-8. We used logistic regression models to calculate odds ratios with each child abuse type as the independent variable and each suicidal behaviour (ideation, plans and attempts) as the dependent variable. The odds ratios were adjusted for sex, age, marital status, province of residence, ethnicity, education, years of service and public safety personnel category. These variables were entered into the model as additional independent variables. The referent group is no suicidal ideation, no suicidal plans or no suicidal attempts. The reference groups with an odds ratio of 1.00 were not added to the table in an effort to make the table less dense. The reviewer is correct that a respondent could be in all the lifetime suicidal ideation, plans and attempt categories and these are not mutually exclusive. An analysis comparing those who endorsed suicidal ideation, plans and attempts, compared to those who only endorsed suicidal ideation and/or plans is an interesting question to address severity of suicidal behaviours, however, beyond the scope of this paper. We have added to the limitations section on page 12 that we were unable to address the persistence or progression of suicidal behaviour, as we only are able to determine that suicidal ideation, plans and attempts occurred at least one time in the lifetime.** 12. Table 4 again suffers from the same questions. Of note in this table is that only about 1500 of the respondents are accounted for and we are not presented with the information on the distribution of those with no suicidal actions. **The reviewer is correct that many respondents are removed from this table that examines child abuse and career-related trauma in relation to suicidal behaviour. This is because of the low prevalence of suicidal behaviour in this sample. In this table, we only presented the results for those who endorsed each suicidal behaviour because this is the research question of interest. Suicidal ideation, plans and attempts are each separate variables, with the reference category for the odds ratios being 'no' to each behaviour. Clarified information for the statistical analysis used for this table has been added to the methods section on pages 7-8. If the editor would prefer, we can include a column for the prevalence of those with no suicidal ideation, plans and attempts, however, we think this would clutter the table with non-pertinent information. Therefore, we have not included this information in the current revision.** 13. Table 5 are presumably the results of a logistic regression. As noted we are not provided with the methodology nor is this extractable from the table. Given the suicidal behaviour is not mutually exclusive, running multiple models may not be appropriate. In written terms, the interest here is in the outcome of suicidal behaviour (as four categories (none, ideation, plans or attempts) and given we have four outcomes of interest, logistic regression may not be the right choice either, or a better dichotomization of the outcome category. **We would like to clarify that logistic regression (dichotomous outcome) was used for this table and has been revised for clarity in our statistical analysis section on pages 7-8. Three separate dichotomous variables were used to measure suicidal ideation, plans and attempts, therefore, each odds ratio uses the 'no' category as the reference group. For simplicity and enhanced readability, we did not include the referent column in the table. A cumulative severity variable that categorizes those who only endorse, ideation, ideation and plans, or all three (ideation, plans and attempts) was beyond the scope of this paper. Further, we were unable to determine the frequency or persistence of suicidal behaviour over time. This has been highlighted in the limitations on page 12. We have added additional description of how the suicidal behaviour variables were coded to the methods on page 7.**
Reviewer 3	Dr. Kelly K. Anderson
Institution	University of Western Ontario, Epidemiology & Biostatistics, London, Ont.
General comments (author response in	This is a comprehensive and well-written paper examining an important and timely topic, namely the mental health of public safety personnel in Canada. Using a large cross-sectional survey, the authors examine the

bold)	impact of childhood abuse and career-related trauma on the prevalence of suicidal behaviours in this sample. I have a number of suggestions regarding the study methodology that I believe will greatly improve the quality of the manuscript. (A) My primary comment on this paper is regarding the unclear and incorrect use of epidemiological terminology for the models examining an interaction between child abuse and career-related trauma on suicidal behaviours. Throughout the paper, the authors refer to the "additive and interactive effects…"; however, interaction can be present on an additive scale or multiplicative scale, so referring to additive and interactive effects as though they were separate concepts is confusing. Additionally, the presence of additive interaction is generally assessed using absolute measures (ex. risk difference) whereas multiplicative interaction is assessed using relative measures (ex. odds ratios, risk ratios). Based on the results (which report adjusted odds ratios), it appears as though both models are assessing multiplicative interaction, so referring to the model from Table 4 as "additive effects" is incorrect. The model presented in Table 4 is assessing multiplicative interaction based on the joint effects definition of interaction, whereas the model in Table 5 is assessing statistical interaction (although also on the multiplicative scale). I will refer the authors to the relevant chapter of Szklo & Nieto's text "Epidemiology: Beyond the Basics" for a comprehensive overview of these concepts. **We appreciate the reviewer's careful explanation of interaction effects. We have now read the chapter as recommended by the reviewer and appreciate this recommendation. After reading the chapter and reflecting on our manuscript, we believe that we were not clear with the statistical tests we used to derive what we have referred to as our additive and interactive models. Our "additive effects model", was not an additive interaction compute. Rather, it was simply the compute of determining if the additive experiences of child abuse and career related trauma had different effects compared to those without experience either, those experiencing only child abuse, and those experiencing only career related trauma. This was done by creating one nominal variable with 4 categories/ levels (i.e., 1) no experience of child abuse or career related trauma, 2) experience of child abuse without any career-related trauma, 3) experience of career-related trauma without child abuse and 4) experience of both child abuse and career-related trauma.) We called this an "additive model" because the levels progressed from no experiences of trauma, to one experience (child abuse OR career-related trauma), to two experiences. This analysis produces odds ratios, that were statistically compared using logistic regression and changing the reference group, to determine if child abuse histories plus career-related trauma produced greater associations with suicidal behaviour than experiencing either type of trauma alone. To not confuse the reader with other statistical tests, we have replaced the word additive with cumulative throughout the paper and have provided more detail in the statistical analysis section on pages 7-8 for clarification. In table 5, a formal interaction (statistical interaction) analyses was conducted and therefore, we have maintained the use of the term interaction for describing the methods and results from this table.** (B) The authors use odds ratios to report measures of effect – however, odds ratios will be overestimated when the prevalence of the outcome is common, in this case nearly 60%. Prevalence ratios would be a more appropriate measure of effect for this analysis. **We appreciate the reviewer's suggestion to use prevalence ratios rather than odds ratios; however, we would like to clarify that our outcome variable in the bivariate and multivariate models (tables 3,4, and 5) does not have a prevalence is 60%. The outcome variables in these tables are suicidal ideation, plans and attempts with prevalence of 26.6%, 12.7% and 4.4%, respectively in this sample. The reviewer states that overestimation is a problem when using odds ratios when the prevalence of the outcome is high. This is correct, however, in other large epidemiology samples, an odds ratio between 2 and 3 with a p-value of 0.001 is commonly reported. Both odds ratios and prevalence ratios are appropriate ways of reporting associations between variables (Tamhane, 2016). We prefer odds ratios because they are more commonly used in large epidemiological studies and are the preferred method in cross-sectional designs. As well, it is the method used in our previously published paper in JAMA Psychiatry looking at a similar set of analyses among a sample of Canadian Armed Forces (Afifi, 2016).** (C) The online survey methodology resulted in a loss of half the sample between the initiation and completion of the survey. I understand that the recruitment methods do not allow for an estimation of the size of the sampling frame, however it would be useful to try and estimate the impact that this loss had on the findings. Is it possible to conduct an attrition analysis to see whether there were differences between those who did and did not complete the survey? Are sensitivity analyses possible to assess the impact of this loss on your findings? Additionally, the authors should report how many people were lost through the exclusion of those with missing data (ie. total n for complete case analysis) **Thank you for this comment. We conducted a sensitivity analysis using a chi square test of significance to test for differences between those who completed the survey and those who dropped out on several sociodemographic variables including: sex, age, marital status, ethnicity, education and province of residence. The results of these sensitivity analyses show no differences in missing data based on sex, marital status, and ethnicity. Those with older age and more education had decreased odds of having missing data. Living in Atlantic or Eastern Canada compared to Western Canada resulted in increased odds of missing data. We have stated the completion rate of 49.3% in the methods on page 5 and added to the limitations on page 12 that the long questionnaire may have contributed to respondent burnout and reduced a completion rate.**

	(D) Given that suicidality is the primary outcome, more information on the suicide questions should be provided. Is this a validated tool? **The three questions used to assess suicidal behaviour have been used previously in several Statistics Canada surveys including the 2012 Canadian Community Health Survey- Mental Health, and the 2013 Canadian Armed Forces Mental Health Supplement. These items have been widely used and published in several high-impact journals such as the Canadian Medical Association Journal (Sareen et al, 2016) and the Journal of the American Medical Association (Afifi et al, 2016).** (E) I am unclear on the relevance of the findings reported in Table 2 (prevalence of child abuse by public safety personnel category). Were differences between categories hypothesized a priori? Is significance testing appropriate in this circumstance? How would these results be interpreted or used in practice? **Prior to this analysis, the prevalence of child abuse among public safety personnel as a whole in Canada or among specific categories was unknown. Therefore, we did not have an a priori hypothesis regarding differences among public safety personnel categories. Having an estimation of the prevalence of this experience is important to give weight to the importance of preventing child abuse in Canada. Individual public safety bodies have separate management and administration and therefore implement screening, prevention and training differently based on their population's needs. It is important to analyze data at the public safety personnel category level so that tailored strategies can be put in place to address the unique needs of that specific population. We have added a sentence to the interpretation on page 11 to articulate this point.**