Reviewer's report

Title: Gender differences among Indigenous Canadians experiencing Homelessness and Mental Illness

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Reviewer: Niranjan S. Karnik

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Review of PSYO-D-18-00170R1

This paper reports on gender differences in mental disorders and substance misuse among Indigenous homeless in Canada. Females were more likely to meet criteria for PTSD, multiple diagnoses, current suicidality, and current substance dependence than males. Being female also predicted self-reported physical and sexual violence. The study is worthwhile and helps address several gaps in the literature.

The introduction contains a great deal of historical and contextual information. The history presented is somewhat selective and in the confines of this paper there is inadequate room to thoroughly review the many elements of gendered and Indigenous discrimination that likely form the framework behind much of the experience for the populations included in this report. I would therefore advise that the paragraph beginning on Page 3 with "Historically…" be further truncated and the examples removed. I would advise the authors reference some key historical works or reviews, and keep the focus of the introduction on the current experiences of this population.

While much of what the authors describe may be due to the historical forces, this paper does not prove this association. The last statement they make in this wide-ranging paragraph that "the distinct historical and gender experiences of Indigenous men and women much be considered in the examination of Indigenous marginalization, street involvement, trauma and homelessness" strikes me as unsupported. While I am certain sympathetic to the sentiment overall, the current situation of the Indigenous homeless stands independent of the history because of the suffering that is happening now. I believe that the authors are trying to point to a process of structural violence that places indigenous populations at risk, but this is not what this paper is about and nothing in this paper examines the structural forces that brought this circumstance about.

The paragraph on Page 4 beginning with Menzies is a very solid beginning. It sets on the current history and the dire circumstances faced by Indigenous women. From here through the end of Page 6, I think the authors have done an excellent job in summarizing the key factors at play and of relevance to their study.

On page 7 the Ethics Statement would benefit from clarification if any of the universities involved have formal mechanisms for soliciting Indigenous input or have boards with representation. Or did the Community Engagement process involve some consideration of ethics and human subject protections? Please include a brief statement of clarification in the Ethics section.
On page 10: shouldn't the MINI and MAP be listed as measures? These are only mentioned later on page 11 as the tool for psychiatric evaluation and substance assessment. Or did only a subset who scored high on the MCAS get this full battery? This is unclear and should be specified. If different groups within the study received difference assessments, then a CONSORT diagram might be help to show the various numbers and breakdown of how the overall sample was handled.

The specification beginning on Page 11 of the variables is very confusing as a number of measures are outlined that are not included in the data collection section. Thus, it is unclear to this reader when the measures were done and on which groups. The structural description of the various variables is fine, I simply don't understand how these measures that were not previously mentioned are popping up.

The final paragraph in the discussion section beginning on Page 16 and continuing on to Page 17 is confusing. It seems to be a discussion about what was not done in this research. I think it belongs in the Study Limitations.

In the discussion section, it will be good to review some of the significant difference in Table 1. (1) Why is there a major difference between the 2 sites in terms of "high need", gender, aboriginal composition, education, children, hospitalization, arrest, and housing status. All of these differences point to two very different contexts or recruitment sites. Is this truly the same base population across these two sites? These differences should be contextualized and explained in the discussion. The differences should not be ignored. The site descriptions that are included earlier in the paper do not give the reader sufficient context to make sense of all of these differences - some of which are quite marked.

I would also like the authors to comment on the number of comparisons being undertaken here and controls for false positives.

Finally, I think the authors need to have some theory guiding the construction of the regression model. It appears that they selected some variables that were positive in their t-tests or chi-squares, and then simply put them into a model. This may not be the best process for structuring these regressions. Consultation with a biostatistician would help.

Overall, I think the study is worthwhile and hope that the authors can address the issues that I have outlined.

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

No

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes
Are the conclusions drawn adequately supported by the data shown?
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