Reviewer’s report

Title: Suicidal ideation and attempts in population-based samples of women: temporal changes between 1989 and 2015

Version: 0 Date: 07 Dec 2018

Reviewer: Kirsten Marchand

Reviewer's report:

General comments:

Thank you for the opportunity to review this paper. The authors have provided a report of a population-based study in which repeat measures of the prevalence of suicidal ideation and attempt over a 26-year period. A secondary aim was to examine the association between socio-demographic characteristics (specifically, education, employment and relationship status) with these suicidal behaviours. In general, the paper is well written and makes an important contribution to those with an interest in this field. There are a few areas requiring further details or clarification, and this would strengthen the overall interpretation of the study's methodology, findings and implications.

This includes a more clear and consistent description of the main effect of time that is of interest in this study; it seems that period effects are of primary concern since analyses are done by wave of data collection (i.e., 1989-1991…2013-2015) for all birth cohorts. However, at other points in the report and analysis, the authors separate these findings by age category. It would strengthen the justification and discussion of the paper to know what is the main effect of interest and why this is the important measure of time for this particular health issue.

The second major limitation relates to the study's current report of the sampling procedure and potential sources of systematic sampling bias. Additional details are needed to support interpretation of the robustness and potential replication of the study. The study could be strengthened with additional supplemental sensitivity analyses, especially since the authors have included the 1993 cohort in Wave 4 analysis.

Section specific comments:

Background:

- The background would be strengthened by providing more context regarding prior research into temporal changes in self-reported suicidal ideation and attempts. Beyond the one US study cited, what have other prior studies shown? Why was this an important issue to measure over time at a population level? What are the implications for studying this over time? What was the author's hypothesis?
- "Temporal trends are likely to be dependent of time and place". The word "of" should be "on".

- Related to this and as stated above, it is unclear what the main effect of interest is in this study. The authors should consider stating at the outset what kind of "temporal trend" or measure of time they were interested in and then anchor the background in explaining why this is an important effect to study for suicidal behavior specifically.

Methods:

- Additional information regarding the two-stage sampling procedure is needed. I recognize the authors might be choosing to save words by citing their prior manuscripts, but some further details are needed here for interpreting the quality and potential sources of bias for the reported analysis. Specifically, the authors should clarify how stage 1 was stratified? In the study population section, I understood that "all women born" in each of the cohort years were invited to stage 2?

- Related to this, the authors state that the response rate for the 1993 birth cohort was low, but have kept this cohort in the analysis, including comparisons between W4 vs. W1 and W4 vs. W3. Could the authors comment on any potential systematic differences between this cohort and the others? They state that for this cohort they also included women from Northern and Eastern districts? How are these districts different from the others? Can sensitivity analyses be done to assure the reader that no systematic bias was introduced with this cohort and thereby wave of data collection and analysis?

- "This two-stage procedure is relevant in epi studies focusing on uncommon conditions", please provide a citation for this.

- The sample was reduced to those completing the long-interview (which included questions on suicidal behaviour). Is this sub-group representative of the broader population? Are those completing the long face-to-face and shorter versions the same? How might this impact external validity?

- Attrition analysis - "no difference in sociodemographic variables". Please state which variables were examined and perhaps consider including this in a supplemental table.

- "Analyses were carried out in SPSS 24 using the Complex Samples Plan"… for stratified samples? Authors could consider adding a brief statement about what this package includes for those readers who are not working in SPSS.

Results/Discussion:

- The authors should review the language used when reporting odds ratios. E.g., "Probability" and "more likely" are not accurate for the effect measure and test performed.

- I wonder how the findings might have been influenced by the fact that suicidal ideation was not the main health outcome of interest in the SWAG design. How might the oversampling of women with alcohol problems have influenced the prevalence and trend of suicidal ideation and also the relationship between education-suicide, employment-suicide? This could be commented on in the discussion.
Tables:

- The sample included in analysis for Table 3, 4, 5: The authors have included the total number who responded to stage 1 of the sampling, when I had understood that data on suicide was only available among those who answered stage 2 for the SWAG questionnaire and completed the long form. Assigning those who did not respond to stage 2, a "no" to lifetime suicidal behaviours requires conceptual and statistical justification.

- Related to this, I would expect that the totals for each wave of Table 3 match the number completing the long baseline and follow-up interviews in table 1 by wave. For example, in table 1, wave 1 = 95, but the total for wave 1 is cited as N=352 which does not match even the response rate for that year (table 1 = 457). Further still, within table 3, the sub-total by suicide behavior doesn't match the wave total (e.g., suicidal ideation = 116+226 = 342).

This raises two concerns that require clarity for the tables to be self-explanatory. First, who entered the analyses for Tables 2-5? Second, the totals should be double checked within tables and any differences explained in the table footnotes. I understand the authors have used prevalence weighted descriptive statistics, but the denominator should still match across tables.

- Small cell sizes in Table 3, 4, 5. There are some small cell sizes (<10 observations) and wide confidence intervals, including for the surprising negative association between being a student and suicidal ideation. How stable are those findings? Was any adjustment made for small cell sizes?

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

Yes

**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?**
If not, please explain in your comments to the authors.

Yes

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