Author’s response to reviews

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

Authors:

Solveig Lövestad (solveig.lovestad@gu.se)
Jesper Löve (jesper.love@gu.se)
Marjan Vaez (marjan.vaez@ki.se)
Margda Waern (margda.waern@gu.se)
Gunnel Hensing (gunnel.hensing@gu.se)
Gunilla Krantz (gunilla.krantz@gu.se)

Version: 1 Date: 09 Jan 2019

Author’s response to reviews:

2019-01-09

Revised Manuscript PUBH-D-18-03615

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

Dear Editor

Thank you for the opportunity to submit a revised version of our manuscript. We are grateful for the constructive and helpful comments from the reviewers that we believe have improved the content of our manuscript. We have carefully revised the manuscript according to reviewers' concerns and suggestions and we hope that we have clarified all questions and doubts. Changes in the manuscript are highlighted and colored in yellow.

Our responses to the reviewers’ comments and suggestions, point by point, follow below. All co-authors have read and approved the revised version of the manuscript.

Best regards, Solveig Lövestad
Response to reviewer # 1

*Reviewer’s comments:

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.

Our comments:
Thank you very much for your valuable comments and suggestions for improvement of this paper. We are extremely grateful for these inputs and we hope that we have managed to clarify any doubts or unanswered questions with our responses and changes in this paper. Our answers are found below, after each reviewer comment.

*Reviewer’s comments:
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.

Our comments:
Our main interest was to measure the period effect within two different age groups (20-30 and 31-49 years). We wanted to see whether there were differences according to age groups, since recent reports indicated increasing trends of mental illness particularly among young women. Previous research in Sweden was mainly based on registered suicide attempts and we wanted to know whether self-reported suicidal ideation and attempts followed similar patterns as registered suicide attempts and/ or trends in data on depression and anxiety. We will contribute with more detailed comments and additions further down in our response to you.

*Reviewer’s comments:
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.

Our comments:
Please see our response below under the heading “Section specific comments”.
Reviewer’s comments:
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?

Our comments:
The background section has been expanded in the following part (Background/ lines 58-86 page 5-6) to further clarify what is already known and why this was an important issue to measure at population level.
As the main purpose of this study was descriptive in its nature we did not specify any hypotheses in advance in relation to the objective and specific research questions.

Reviewer’s comments:
"Temporal trends are likely to be dependent of time and place". The word "of" should be "on".

Our comments:
Done. Thank you for catching this.

Reviewer’s comments:
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.

Our comments:
Please see our response above regarding the expanded Background (see: Background/ lines 58-8g page 5-6).

Reviewer’s comments:
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?

Our comments:
The different sampling procedures employed in stage 1 are now clarified and we have added a revised part under the methods section (Methods/ lines 119-141, pages 7-8) which we hope will clarify the sampling procedure. We have also clarified the difference between the two stages through additional sub headings (“Stage 1” and “Stage 2” respectively) in table 1. Stage 1 includes ONLY the screening
phase, which employed the SWAG screening questionnaire. (number of women that SWAG was sent to, number of women who responded to the SWAG-questionnaire and number of women who were randomly selected according to the answers in SWAG). Stage 2 includes those who completed the long interviews.

*Reviewer’s comments:
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?

Our comments:
We believe that one important systematic bias with regard to the 1993 cohort is that they are more highly educated compared to previous waves. Therefore we mention the following under the Discussion section/ Methodological considerations (Line 426-429, page 20): “As women with low socioeconomic status and educational attainment are more likely to report suicidal ideation than their counterparts [18], the prevalence figures for suicidal ideation and attempts are likely to be underestimated in our study”. We have added the following sentence in order to be more specific “This is particularly true for the last wave in which a greater proportion had higher education” (Line 229-230, page 20).

*Reviewer’s comments:
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?

Our comments:
Before including the new district in the fourth wave, we looked at official data for each of the districts. The additional districts were chosen carefully and due to their similarity (such as educational attainment and unemployment rates) with the western and central districts of Gothenburg included in earlier waves. Therefore, and since we cannot trace women who did not answer to SWAG, we have not done any sensitivity analyses.

*Reviewer’s comments:
"This two-stage procedure is relevant in epi studies focusing on uncommon conditions", please provide a citation for this.

Our comments:
We have added a new reference (reference number 23) and the following two sentences in order to clarify the topic (Methods, lines 119-123, page 7): “This two-stage procedure is relevant in epidemiological studies focusing on uncommon conditions such as alcohol-related disorders in women, for which this study was initially designed [23]. The main purpose of this two-stage procedure was to increase the number of individuals with alcohol problems, while keeping the numbers of interviews at a reasonable level [23]”.

Reviewer’s comments:
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?

Our comments:
After analysing the differences between women who completed the long and short interviews, we found that women who completed the long interviews were older, had a higher educational attainment and had a somewhat higher alcohol consumption compared to women who completed the short interviews. We have added new information about this under the methods section (line 177-181, page 10). This might influence on the external validity since younger women and women with low educational attainment are underrepresented in our study. These groups are recognized to have higher prevalence of suicidal ideation, thus suicidal ideation in our study may be underestimated.

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

Our comments:
We have added the following paragraph (Methods/ Attrition analysis/ lines 178-181, page 10) in order to specify the variables that were used: “An analysis performed in a previous study showed no difference in sociodemographic variables (age, marital status, number of children, education and employment status for women and her partner) between those who did and did not respond to SWAG. This type of attrition analysis was possible to perform in the first two waves, when women from the Stage 1 attrition group were invited for interview in Stage 2, thus answering to a range of background factors during the interview.”

Reviewer’s comments:
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.

Our comments:
We have added more information according to your suggestion (Methods/ statistical analysis/ lines 240-243, page 12).

Reviewer’s comments:
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.

Our comments:
We have made changes according to your suggestion in the results and discussion section, as well as in the abstract (marked in yellow) and now, instead of saying “more likely” we say for example “higher OR”. However, we have maintained “more likely” in the discussion section when we are discussing and/ or referring to other articles.
Reviewer’s comments:
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.

Our comments:
It could be assumed that women with higher scores on SWAG (a higher number of affirmative responses to items mirroring alcohol related problems) would have a higher prevalence of suicidal ideation. However, we do not think that this is a problem in our study as we have used the weighting process in order to account for any bias related to the stratification process including the oversampling of women with alcohol-related problems. This means that any association between affirmative responses to SWAG and suicidal ideation is distributed as it is in the source population.

Reviewer’s comments:
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.

Our comments:
We apologize for the lack of clarity regarding the numbers shown in Tables 3-4. Only women who participated in the long interviews in Stage 2 are included in these tables. The presentation of weighted numbers seems to have caused some confusion. We have now changed all the tables and added weighted numbers to table 1 in order to clarify where the subsequent total numbers in the other tables come from. In addition, rows showing missing values have been added to Tables 2 to 5 in order to clarify that not all women who completed the long interviews answered to the questions of suicidal ideation and attempts.

We have also added a new sentence under the methods section/ statistical analyses (lines 245-247, page 12-13) in order to clarify why no weights were used with regard to the cohort born in 1993: “Since no randomized selection based on the SWAG scores was performed in the cohort born in 1993, no weights for oversampling of alcohol related problems were applied”.

Reviewer’s comments:
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.

Our comments:
As mentioned above we believe that there is some confusion regarding the weighted and unweighted numbers, as well as the lack of information about missing values. The following changes have been made in the tables:

Table 1:

a) We have added headings naming the stages clearly in order to clarify the difference between stage 1 (screening procedure) and stage 2 (long face-to-face interviews).

b) In the headings, we now clarify when unweighted and weighted numbers are used.

c) Apart from unweighted numbers of the total sample who completed the long interviews and therefore are included in this particular study, we also have added weighted totals for each birth cohort so the reader more easily can see in the subsequent tables who is included at each wave. For example in wave 2, there are 85 women born in 1965 who completed a long interview. This makes 349 women after the weighting procedure. Further 543 women born in 1970 and 1975 completed a long interview, which makes 1891 after the weighting procedure. This yields a total N= 2240 (349+1891) within the age-range of 20-30 years.

Table 2:

a) We have added unweighted numbers (n) in addition to the weighted numbers in order to clarify the real numbers based on the real totals that are distributed within the different sociodemographic characteristics at each wave.

b) We have further added rows with missing values so the numbers in each column at each wave now sum up the unweighted and weighted totals.

c) Important to note is that during the weighing procedure, sometimes the analysis yields for example 23,5 or 50,5 individuals instead of 24 or 51 individuals. This is why the percentages sometimes will not correspond with the numbers presented since we have rounded the numbers in order to get whole numbers (individuals). Thus, the percentages presented in Tables 2 to 5 are correct while the numbers at some instances may be rounded.

d) We have added a “we” in the table and footnotes to highlight when weighted numbers and % are used.

Table 3:

a) We have added unweighted numbers (n) to make it more easy to trace how many women from the original, ‘real’ number of the sample have answered “yes” and “no” to experiences of suicidal ideation and attempts at each wave.

b) We have added a row with missing numbers in order to demonstrate that those who answered “no” to the questions about suicidal attempts really answered no and did not refuse or otherwise not responded to the question. Those who are missing values are presented in the table. For example: at Wave 1, 116 women (weighted numbers) answered that they had experienced suicidal ideation during their lifetime while 226 women (weighted numbers) responded that they had not experienced this. Further 10 women (weighted numbers) had missing values.
Tables 4 and 5:

a) Instead of total numbers from the table number 2, we have added those who responded “no” to suicidal ideation according to each sociodemographic factor. We have also put the corresponding total numbers of those who responded “yes” at each wave in the columns. In this way, we think it will be less confusing and easier to calculate the OR at each wave. So for instance, at wave 1 among those aged 20-30 years, 166 women responded that they had experienced lifetime suicidal ideation. Of these, 53 women (weighted numbers) had more than 12 years of education, 55 women had 10-12 years of education, 8 women had 9 years of education or less, summing up to 116 women with lifetime suicidal ideation.

b) As pointed out above, the weighting procedure may yield numbers with decimals which are rounded to yield whole numbers of individuals. This means that the numbers presented in tables 4 and 5 do not always yield the exact ORs presented in the tables.

*Reviewer’s comments:
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?

Our comments:
Since we wanted to make a comparison over time with repeated measures during the four data collection waves, we wanted to have as comparable variables as possible. Some variables (depending on age-group and data collection wave) have satisfactory cell sizes while other variables have small cell sizes. Thus, we kept the categories similar over the data collection waves and independent of the cell size in order to be able to analyse the changes over time and at each data collection wave. No adjustment for small cell sizes was performed. The small cell sizes merit caution when interpreting the findings, therefore we have added a section about small cell sizes. Please see the discussion section on page 19-20 line 418-421.

Response to reviewer #2

Reviewer’s comments
The study is part of a four-wave longitudinal population-based project initiated in 1986 that assesses the prevalence of self-reported suicidal ideation and attempts among women from the general population aged 20-49 years. Moreover, authors investigate associations between sociodemographic factors and suicidal ideation.

The goal of the study is very relevant. Suicidal behaviors research needs to integrate sociodemographic factors in theoretical models in order to adapt suicide prevention policies to social groups at risk because the effect of these factors may be modified through policies and public health interventions (Conejero et al., 2016).

Overall, the article is well written and the logic of the study is in accordance with the goal. The paper provides useful data, the main conclusions are consistent and the authors are aware of the limitations of
the study. Hence I congratulate the authors for the work they are doing. Some considerations and suggestions are provided below about background, discussion and conclusions sections.

Our response
Thank you very much for your valuable comments and suggestions for improvement of this paper. We are grateful for these inputs and we hope that we have managed to clarify any doubts or unanswered questions with our responses and changes in this paper. Our answers are found below, after each reviewer comment.

*Reviewer’s comments

Background
It would be necessary to include a reflection on the meaning of the sociodemographic factors. The information obtained in this study is aimed at informing the design of new public health interventions. Therefore, the authors should highlight some ideas about the design of these interventions.

Our comments
According to the reviewers suggestion’s we have included two new parts in the background section explaining the meaning of sociodemographic factors within public health research (Background/ lines 90-94, page 6), and the meaning of studying these factors in relation to suicidal ideation over time (Background/ lines 103-106, page 7). We have further deleted the phrase “Thus, more knowledge on temporal changes in the relationship between sociodemographic factors and suicidal ideation may inform the design of new public health interventions” as our aim is not to include the development of new public health interventions but rather identify risk groups for suicidal ideation, and potential changes in risk groups over time.

*Reviewer’s comments

Discussion
The first two paragraphs of the discussion section do not discuss the implications of the findings in context of existing research. These paragraphs are an explanation of results, and the authors should replace them.

Our comments
We have eliminated the first two paragraphs of the discussion section. However, we have maintained a short summary of the main findings in accordance with the suggestions stated in the checklist by STROBE (J.P Vandenbroucke et al. Strengthening the Reporting of Observational Studies in Epidemiology (STROBE): Explanations and findings, 2007). Please, see the part under the discussion section (lines 288-292, page 14). We have also written a sentence in the first paragraph under the discussion section in order to highlight the implications of our study within the context of existing research: “This is one of the first studies in Sweden assessing period changes of self-reported suicidal ideation and attempts among women from a population-based sample.” (Discussion/ lines 287-288 page 14)
*Reviewer’s comments*
In the third paragraph, the authors do not discuss about the result that they have obtained (an increase of lifetime suicidal ideation).

Our comments
In accordance with the reviewers suggestions, we have added a part where we discuss our findings of increasing suicidal ideation and we discuss these findings in relation to recent data on depression and anxiety. Please see discussion section, lines 303-308, page 15.

*Reviewer’s comments*
Comparisons are difficult (as authors state) but:
Would it be necessary to propose some interventions?
What is the meaning for public health?
Moreover, the authors should consider these questions in the fourth (educational), fifth (current occupation) and sixth (relationship status) paragraphs.

Our comments
We think that the meaning for public health in general is that our study provides additional information, and confirms previous data regarding concerns about an increasing trend in mental illness, particularly among young women. Another important issue related to the meaning for public health is that we think that our study identifies risk groups over time and indicates that there may be changes in sociodemographic factors due to possible changes at an historical, societal and environmental level.

According to the reviewers suggestions, we have added a part which proposes an intervention + additional research in the discussion section under ”Prevalence of lifetime suicidal ideation and attempts” (lines 308-3012, page 15). We have also proposed an intervention regarding “educational attainment” under the discussion section, line 331-333, page 16.

Regarding ”current occupation”, we previously had written the following: “Strong social safety nets that prevent economic exclusion due to unemployment, together with maintenance (instead of cost-cutting measures) of mental health care services are shown to mitigate the negative mental health effects of unemployment in times of economic recession”. We think this is a type of public health intervention at structural level.

We have not proposed any specific intervention regarding relationship status, as we believe that some of the proposed interventions may mitigate the relationship between being single and suicidal ideation.

*Reviewer’s comments*
Conclusions
It is true that no conclusions can be drawn about the mechanisms behind the changes in the association between sociodemographic factors and suicidal ideation over the time. But the authors should consider the meaning of the findings for future preventive interventions in public health.
I hope these proposed modifications will serve to improve the manuscript.

Our comments
The conclusion has been expanded in accordance with the reviewer’s suggestion (Conclusions/ lines
“However, the results support clinical and public health focus on younger, socioeconomically disadvantaged women.”