

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Criminality and Suicide – A longitudinal Swedish Cohort Study
AUTHORS	Stenbacka, Marlene; Sidorchuk, Anna; Romelsjö, Anders; Jokinen, Jussi

VERSION 1 - REVIEW

REVIEWER	Dr Roger Webb Senior Research Fellow in Mental Health Epidemiology Postgraduate Research Director - Institute of Brain, Behaviour & Mental Health
REVIEW RETURNED	26-Jul-2013

THE STUDY	<p>I felt that the standard of written English was reasonable, but that it could be improved somewhat to reach a publishable standard.</p> <p>I have addressed one or two omissions from the recent literature in my comments to the authors</p>
GENERAL COMMENTS	<p>I felt that this was a strong paper overall. It usefully adds to the growing evidence-base for elevated suicide risk among people with history of criminal offending. The data sources and design were strong, the sample size was very large (enabling the authors to generate precise effect estimates), and the analytical approach taken was appropriate.</p> <p>However, I didn't feel that the paper was particularly novel, in that much of the findings reported from this Swedish registry concur with recently published findings from a Danish register-based study. The authors cited the first paper published from that study (Webb et al. Arch Gen Psychiatry 2011), but they did not cite another subsequent paper that is highly relevant to their findings. Thus, at the top of page 12, the authors state "...to the best of our knowledge, no earlier studies have focused on the choice of suicide method in relation to criminality." This statement is now inaccurate, although the authors were probably unaware of the following paper at the time of submission:</p> <p>Webb RT, Qin P, Stevens H, Shaw J, Appleby L, Mortensen PB. National study of suicide method in violent criminal offenders. J Affect Disord 2013 [http://dx.doi.org/10.1016/j.jad.2013.04.001].</p> <p>It may also be appropriate for them to cite the following article elsewhere in their paper:</p> <p>Webb RT, Shaw J, Stevens H, Mortensen PB, Appleby L, Qin P. Suicide risk among violent and sexual criminal offenders. J Interpers Violence 2012; 27:3405-3424.</p> <p>This paper showed that suicide risk increased with rising levels of</p>

	<p>seriousness of violent crime, across the whole population of Danish violent offenders (with and without history of custodial sentencing).</p> <p>My other specific comments on the paper are as follows:</p> <p>1) I would alter the wording used by the authors in several places. For example, "Suicide poisoning was most common in the criminal groups..." (instead of "most prevalent") - Abstract, p2; "Suicide" instead of "Suicide mortality" - throughout; "... physical or psychiatric disorder/disability" (instead of "handicap") - Article Summary, p3; "Hazard ratio" instead of just "hazard" - several places.</p> <p>2) On page 9 of the Methods section, please could the authors explain why they could not censor their cohort for emigration in calculating the person-time at risk during the follow-up period? My understanding is that this can be done with a high degree of accuracy in Sweden, via complete linkage to the Total Population Register.</p> <p>3) On page 10 of the Results section, I don't believe that all the chi-squared results that are reported in the first paragraph add value to the paper. These are already presented in Table 1, and it isn't necessary to repeat them in the main text.</p> <p>4) Towards the foot of page 13, the authors cite previous research conducted by Sattar. I believe this work was done in England and Wales, as opposed to Scotland and Wales, as the authors have reported it.</p> <p>5) The first author's name for reference no.12 in the reference list has been wrong spelled as "Runneson": it should be "Runeson"</p> <p>6) In Table 2 (p24), I don't feel that "Train movement" is a particularly good description for that method of suicide. I think a better one is "Jumping or lying before a moving train". If that's too long for the narrow column width, then I suggest "Moving train" instead of "Train movement"</p> <p>7) In Table 2, I suggest that the results are presented as hazard ratios versus the general population without criminal offending history, rather than as percentages of the total number of suicides in each group. Each specific suicide method can be treated as an outcome in its own right, and with a hazard ratio calculated for each method. I feel this is a far more informative approach for determining which methods have a particularly highly elevated risk in the two offender groups.</p>
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REVIEWER	Azar Kariminia, Lecture The Kirby Institute for infection and immunity in society, University of New South Wales, Sydney, Australia
REVIEW RETURNED	30-Jul-2013

THE STUDY	<p>In this manuscript the suicide was identified through linkage with the National Registry, but the linkage process is not described. The statistical section is weak and the Cox-regression analyses and the procedure for variable selection are not adequately described. The manuscript is not consistent and lacks important results especially in tables.</p>
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RESULTS & CONCLUSIONS	The research question is simple. But the results in the text and in the table are not well presented. For this reason, I cannot make sure how well the results answered the research question. The discussion is weak and previous related research was not used properly.
REPORTING & ETHICS	Could not find a statement regarding the conflicts of interest.

VERSION 1 – AUTHOR RESPONSE

Reviewer #1:

1. "The authors cited the first paper published from that study (Webb et al. Arch Gen Psychiatry 2011), but they did not cite another subsequent paper that is highly relevant to their findings. Thus, at the top of page 12, the authors state "...to the best of our knowledge, no earlier studies have focused on the choice of suicide method in relation to criminality." This statement is now inaccurate, although the authors were probably unaware of the following paper at the time of submission: Webb RT, Qin P, Stevens H, Shaw J, Appleby L, Mortensen PB. National study of suicide method in violent criminal offenders. J Affect Disord 2013 [<http://dx.doi.org/10.1016/j.jad.2013.04.001>].

Response 1.1:

We have included the reference "Webb RT, Qin P, Stevens H, Shaw J, Appleby L, Mortensen PB. National study of suicide method in violent criminal offenders. J Affect Disord 2013 [<http://dx.doi.org/10.1016/j.jad.2013.04.001>]. and changed the text to "Very few earlier studies on the association between violent and non-violent criminality and suicide have been conducted in a cohort representing the general population and with a long follow-up period."

See page 11 last sentence and page 12. See also page 18, reference 32.

2. I would alter the wording used by the authors in several places. For example, "Suicide poisoning was most common in the criminal groups..." (instead of "most prevalent") - Abstract, p2; "Suicide" instead of "Suicide mortality" - throughout; "... physical or psychiatric disorder/disability" (instead of "handicap") - Article Summary, p3; "Hazard ratio" instead of just "hazard" - several places.

Response 1.2:

Changes made as requested.

3 On page 9 of the Methods section, please could the authors explain why they could not censor their cohort for emigration in calculating the person-time at risk during the follow-up period? My understanding is that this can be done with a high degree of accuracy in Sweden, via complete linkage to the Total Population Register.

Response 1.3:

This cohort has not been matched to any register containing information about immigration.

4. On page 10 of the Results section, I don't believe that all the chi-squared results that are reported in the first paragraph add value to the paper. These are already presented in Table 1, and it isn't necessary to repeat them in the main text.

Response 1.4:

We have shortened the text. See page Result-section, 9, last paragraph "Table Ibetween the groups."

5. "Towards the foot of page 13, the authors cite previous research conducted by Sattar. I believe this work was done in England and Wales, as opposed to Scotland and Wales, as the authors have reported it."

Response 1.5:

Changed in the text. See page 12, three lines from the top.

6. "The nature of the study (cross sectional + longitudinal) is not clear from the abstract, which could cause readers to be confused. It would be helpful if the abstract included some statements regarding the overall structure of the study."

Response 1.6:

The nature of the study is clarified in the abstract.

7. "The first author's name for reference no.12 in the reference list has been wrong spelled as "Runneson": it should be "Runeson""

Response 1.7:

The misspelling is corrected. See reference list no: 12.

8. "In Table 2 (p24), I don't feel that "Train movement" is a particularly good description for that method of suicide. I think a better one is "Jumping or lying before a moving train". If that's too long for the narrow column width, then I suggest "Moving train" instead of "Train movement"

Response 1.8:

Changed to "Moving train"

9. "In Table 2, I suggest that the results are presented as hazard ratios versus the general population without criminal offending history, rather than as percentages of the total number of suicides in each group. Each specific suicide method can be treated as an outcome in its own right, and with a hazard ratio calculated for each method. I feel this is a far more informative approach for determining which methods have a particularly highly elevated risk in the two offender groups. "

Response 1.9:

Thank you for the valuable comment on Table 2. We have changed the table according to the advice of Reviewer I.

Reviewer #2:

2.1. In this manuscript the suicide was identified through linkage with the National Registry, but the linkage process is not described.

Response 2.1:

See page 8, 11 lines from the top. "The data was linked at Statistics Sweden (Statistiska Centralbyrån).... approval of Karolinska Institute."

2.2. The statistical section is weak and the Cox-regression analyses and the procedure for variable selection are not adequately described.

Response 2.2:

The section that describes the confounders has been clarified and hopefully improved. See section "Measurement of potential confounders" Page 5-7. See also the "Statistical analyses"-section.

2.3. The manuscript is not consistent and lacks important results especially in tables.

Response 2.3:

Table 2 has been removed and replaced with a new according to the advice from Referee 1. (see point 7, and Table 2). See also above, point 1.4.

2.4. The research question is simple. But the results in the text and in the table are not well presented.

Response 2.4:

The Result-section has been changed. See point 1.4. See above point 2.3.

2.5. I cannot make sure how well the results answered the research question. The discussion is weak and previous related research was not used properly.

Response 2.5:

The Discussion has been changed and is now more structured. See pages 10-15.

2.6. Could not find a statement regarding the conflicts of interest.

Response 2.6:

Conflicts of interest statement is added. See page 15.

VERSION 2 – REVIEW

REVIEWER	Kariminia, Azar University of New South Wales
REVIEW RETURNED	16-Oct-2013

GENERAL COMMENTS	<p>The authors have not responded to the majority of comments. Those comments together with the new ones are listed below:</p> <p>Abstract and introduction</p> <p>1) The name of the country in which the study was conducted is not given in the abstract.</p> <p>2) Suicide was the main outcome for this study and there is no mention of the number (%) of suicide and the rate in abstract.</p> <p>3) The two studies referenced in para 3 (References 7 & 8) were not conducted among prison population. In these studies criminality was included as a risk factor. On the other hand, study by Webb et al was conducted among people with a criminal history and this needs to be specified. Ref 9 is among a population of young offenders. This kind of information needs to be clear in the introduction.</p> <p>4) Paragraph 8, line 3: The following sentence needs re-wording “Likewise, in a recent longitudinal study of the association between offenders and victims of violence and cause specific mortality found that suicide carried a hazard of about two after adjustment for various psychosocial covariates”.</p> <p>2) Materials and methods</p> <p>5) Page 5, lines 40 ‘From 1 July 1969 to 30 June 1970 to 2004’. Isn’t 2004 the end of your study? If yes, then you have included in this study those who conscripted for military service between 1 July 1969 to 30 June 1970. We won’t need to 2004 here.</p> <p>6) Page 5, lines 45-47: ‘conscripts born 1949 to 1950’. The your wrote: ‘75% were born in 1951’ ?? This is inconsistent.</p> <p>7) Page 6, lines 16-18: “We include 11 cofounders which have” should not be included here.</p> <p>8) Page 6, line 23: What are social class III and I-II ?</p> <p>9) Page 8, lines 41-43: “The surveillance time was calculated.....” Do the authors mean: “Follow-up started from 1 July 1969 until the date of death or the end date of the study (31 December 2004)? This will be enough and we don’t need to mention the two participants who died during 1969.</p> <p>10) Why the authors used bivariate (including two variables simultaneously). How about univariate?</p> <p>11) We read: ‘A test of the proportional hazard assumption was performed for each covariate in bivariate and multivariate analyses by using a time-dependent explanatory variable’. What do the authors mean by time-dependent variables here? Please name variables taken as time-dependent?</p>
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	<p>12) We read: 'If the p-value was significant ($p < .05$) the proportional hazard assumption was violated and the variable was not included in the analyses'. I believe the authors mean ($p < .05$) for staying in the final model. What was the level of significance in the univariate model (0.10; 0.15?) for being tested in the univariate.</p> <p>13) 'We adjusted for potential risk factors' is enough to be written once. Repeating it on line 56 is redundant.</p> <p>14) The linkage process should be described briefly even if it was conducted by the Statistics Sweden.</p> <p>3) Results</p> <p>15) Page 10, Lines 19-23: 'Violent criminality was mostly.....(HR: 4.69....) followed by non-violent criminality (HR: 2.08.....). in bivariate analysis, while after adjustment.... the hazard decreased to nearly three and two folds respectively'. But, I see the following figures in Table 3: Violent: 4.69 then 2.68 and 1.30 Non-violent: 2.08, then 1.65 and 1.35</p> <p>16) Page 10, repeated violent and non-violent offenders section: The writing in this section doesn't match exactly what has been given in table 4. According to this table both '1 offence' and '2+ offence' among violent and non-violent offenders were associated with suicide in the crude analysis. For the adjusted analyses, 1 offence in the violent group also remained significant (HR: 1.83, 1.29-2.59). Adjusted HR for 2+offences in non-violent group is 2.24 and not 2.34.</p> <p>17) The authors have not shown the univariate figures for other co-variates, they could at least include a short paragraph on them in the results.</p> <p>4) Discussion This section needs to be tidied up.</p> <p>Tables and graph</p> <ul style="list-style-type: none"> • Table 1: The title 'Risk factors...' is not appropriate for table 1. They are baseline characteristics. Including percentage is not enough and the authors should consider including the actual number. If the p values are calculated across all categories why they have been placed in the middle rows? • Table 4: The title is not correct. The table does not show the number of offences adjusted for ... It shows the association of criminality with suicide. • The graph is not self-explanatory and does not have a proper title.
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VERSION 2 – AUTHOR RESPONSE

Reviewer Name Kariminia, Azar

Institution and Country

University of New South Wales

Please state any competing interests or state 'None declared': None

General comments

The authors have not responded to the majority of comments. Those comments together with the new ones are listed below:

Abstract and introduction

1) The name of the country in which the study was conducted is not given in the abstract.

Response: The country (Sweden) has been included. See Abstract 6 lines from the top.

2) Suicide was the main outcome for this study and there is no mention of the number (%) of suicide and the rate in abstract.

Response: Changes made as requested. See abstract 16 lines from the top. "Of these 615..."

3) The two studies referenced in para 3 (References 7 & 8) were not conducted among prison population. In these studies criminality was included as a risk factor. On the other hand, study by Webb et al was conducted among people with a criminal history and this needs to be specified. Ref 9 is among a population of young offenders. This kind of information needs to be clear in the introduction.

Response: We have clarified this in the introduction. See page 4, 10 lines from the top. Studies on.....of the follow-up [9]"

4) Paragraph 8, line 3: The following sentence needs re-wording "Likewise, in a recent longitudinal study of the association between offenders and victims of violence and cause specific mortality found that suicide carried a hazard of about two after adjustment for various psychosocial covariates".

Response: We have re-worded the sentence. Page 5, 11 lines from the top. "Likewise, in..... psychosocial confounders.[13]"

2) Materials and methods

5) Page 5, lines 40 'From 1 July 1969 to 30 June 1970 to 2004'. Isn't 2004 the end of your study? If yes, then you have included in this study those who conscripted for military service between 1 July 1969 to 30 June 1970. We won't need to 2004 here.

Response: Done! See page 6, 5 lines from the top.

6) Page 5, lines 45-47: 'conscripts born 1949 to 1950'. The your wrote: '75% were born in 1951' ?? This is inconsistent.

Response: See page 5:8 lines from the top: In order to getin 1951."

7) Page 6, lines 16-18: "We include 11 cofounders which have" should not be included here.

Response: The sentence has been deleted.

8) Page 6, line 23: What are social class III and I-II ?

Response: We have explained the variable "social class" on page 6, 4 lines from the bottom "The confounders were: Father's occupation..... blue-collar workers.[22]"

9) Page 8, lines 41-43: "The surveillance time was calculated....." Do the authors mean: "Follow-up started from 1 July 1969 until the date of death or the end date of the study (31 December 2004)? This will be enough and we don't need to mention the two participants who died during 1969.

Response: Changes made as requested. See page 9, 12 lines from the top: "The surveillance time....in the cohort."

10) Why the authors used bivariate (including two variables simultaneously). How about univariate?

Response: Vi have calculated "bivariate" analyses measuring statistical association between two variables (independent and dependent)which also has been done in prior studies on this cohort.

11) We read: 'A test of the proportional hazard assumption was performed for each covariate in bivariate and multivariate analyses by using a time-dependent explanatory variable'. What do the authors mean by time-dependent variables here? Please name variables taken as time-dependent?

Response: As we can see, the text was correct, however we have made some clarifications See page 9, 5 lines from the bottom:

"A test of the proportional hazard assumption was performed for each confounder (X) in bivariate and multivariate analyses by using a time-dependent explanatory variable in the model ($X \cdot (\log \text{time} - \text{average value of the log time})$)."

12) We read: 'If the p-value was significant ($p < .05$) the proportional hazard assumption was violated and the variable was not included in the analyses'. I believe the authors mean ($p < .05$) for staying in the final model. What was the level of significance in the univariate model (0.10; 0.15?) for being tested in the univariate.

If $P < 0.05$, then it is a significant difference over time in terms of the relationship between the outcome and the independent variable (proportionality condition is not met) and one should not use proportional hazards.

Response: See page 9, 2 lines from the bottom. "If the p-value was significant ($p < .05$) the proportional hazard assumption was not fulfilled and the variable was not included in the analyses."

13) 'We adjusted for potential risk factors' is enough to be written once. Repeating it on line 56 is redundant.

Response: Done! We have deleted the sentence.

14) The linkage process should be described briefly even if it was conducted by the Statistics Sweden.

Response: See page 9, 3 lines from the top "Information about alcohol and drug..... Stockholm."

3) Results

15) Page 10, Lines 19-23: 'Violent criminality was mostly.....(HR: 4.69....) followed by non-violent criminality (HR: 2.08.....). in bivariate analysis, while after adjustment.... the hazard decreased to nearly three and two folds respectively'. But, I see the following figures in Table 3:

Violent: 4.69 then 2.68 and 1.30

Non-violent: 2.08, then 1.65 and 1.35

Response: Corrected the text. See page 11, 1 lines from the top under "Crude and multivariate analyses"

16) Page 10, repeated violent and non-violent offenders section: The writing in this section doesn't match exactly what has been given in table 4.

Response: Corrected the text. Page 11, 8 lines from the bottom. "HRs=2.42 and 2.24, respectively (Table 4)."

According to this table 4 both '1 offence' and '2+ offence' among violent and non-violent offenders were associated with suicide in the crude analysis. For the adjusted analyses, 1 offence in the violent group also remained significant (HR: 1.83, 1.29-2.59).

Response: See point 16.

Adjusted HR for 2+offences in non-violent group is 2.24 and not 2.34.

17) The authors have not shown the univariate figures for other co-variates, they could at least include a short paragraph on them in the results.

Response: The HR:s for the other confounders are presenting in the paragraph "Crude and multivariate analyses" See page 11, 5 lines from the top.

4) Discussion

This section needs to be tidied up.

Response: The Discussion has been partly rewritten and hopefully improved.

Tables and graph

- Table 1: The title 'Risk factors...' is not appropriate for table 1. They are baseline characteristics. Including percentage is not enough and the authors should consider including the actual number. If the p values are calculated across all categories why they have been placed in the middle rows?

Response: We have change the title to" Baseline characteristics...

- Table 4: The title is not correct. The table does not show the number of offences adjusted for ... It shows the association of criminality with suicide.

Response:Changes made as requested.

- The graph is not self-explanatory and does not have a proper title.

Response: We have included a Figure title and made it more self-explanatory.