

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form ([see an example](#)) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below. Some articles will have been accepted based in part or entirely on reviews undertaken for other BMJ Group journals. These will be reproduced where possible.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Psychological morbidity among suicide-bereaved and non-bereaved parents - a nationwide population survey
AUTHORS	Omerov, Pernilla; Steineck, Gunnar; Nyberg, Tommy; Runeson, Bo; Nyberg, Ullakarín

VERSION 1 - REVIEW

REVIEWER	Annette Erlangsen Senior Researcher Mental Health Centre Copenhagen Denmark No competing interests.
REVIEW RETURNED	22-May-2013

THE STUDY	I believe that there is a substantial body of American studies on bereaved by suicide, which - despite potential methodological concerns - might address related objectives and might be worth referencing.
GENERAL COMMENTS	<p>It is an interesting study examining outcomes related to mental and physical health of parents before and after bereavement by child's suicide. The study has an acceptable response rate, which lowers the risk of sampling bias. Also, it is laudable that the authors examine for pre-existing conditions prior to bereavement (see below). The questions of the scales are very specific and likely to be underreported for the period preceding the loss.</p> <p>Minor comments:</p> <ul style="list-style-type: none">- You might want to begin your Result sentence in the Abstract with "In all,".- Consider skipping the last sentence of the Introduction in the main manuscript – it is covered in the following section. You could add a sentence on the relevance of the study if preferred.- Is there any clear definition of the timing of the questions for the pre- and post-bereavement conditions? Were the questions specifically formulated as 'before' and 'after' for the bereaved while the terms 'more than 10 years ago' and 'less than 10 years ago' were used for the matched controls? It would be good with a deeper explanation of this in the Method section and an equivalent consideration in the strength/limitations of the Discussion.- Also, if I understand the footnote of Table 2 correctly 'during last 10 years' was interpreted as 'after child's death' – how does this fit with the actual timing of the bereavement?- Please express the similarities presented in Table 1 in more statistical terms, i.e. by demonstrating that there are no significant differences.

	- Please consider how the table texts of Table 2 and 3 can be made more reader-friendly. An example: "Persistent anxiety" instead of "Persistent anxiety 1 day/week or more". The unit could be added to the footnote.
--	--

REVIEWER	Huibert Burger, MD, PhD Department of General Practice University Medical Center Groningen University of Groningen The Netherlands
REVIEW RETURNED	14-Jun-2013

THE STUDY	<p>The two ORs (2.6 and 2.3) presented in table 3 and in the abstract suggest that the authors wanted to show effect modification of the association between bereavement and depression by long term psychological premorbidity. However, this specific interest is not mentioned as an hypothesis in the introduction. Nor is it in the data analysis (statistical interaction).</p> <p>It is unclear how valid the questions about psychological premorbidity 10 years ago actually are . Has this not been validated (by others)?</p> <p>It is concluded that in parents who lost a child through suicide in Sweden no higher prevalence of long-term psychological premorbidity was found than in parents who had not lost a child. This is, however, not supported by the data. In table 2 it is shown that there is a statistically significant higher prevalence of medication against low mood or depression more than 10 years ago (OR: 1.5 (95%CI 1.0 – 2.4) in suicide bereaved than non-bereaved parents. Although this was not significant this also regarded a first psychiatric diagnosis and medication against anxiety. This means that the conclusion from the authors is not supported by the data.</p> <p>It is unclear why the investigators use two different regression, i.e. log-binomial and logistic. Why not use logistic regression throughout?</p> <p>The question of interaction between premorbidity and bereavement effect is not in the analysis.</p> <p>Although the limitation that the increased risks of depression may not be specific to bereavement by suicide but may also be implicated in other causes of bereavement is adequately discussed . However, the authors have failed to refer to a recent study in which it was shown that parents who lost their child through a motor vehicle accident had similarly increased depression risks as those by bereavement (Bollon et al. JAMA Psychiatry. 2013;70(2):158-167).</p>
RESULTS & CONCLUSIONS	<p>The results are not credible in my opinion. The OR of 1.0 in table 2 on which the main conclusion is based is hard to interpret given the other OR in that table of which 3 were higher than 1.0.</p> <p>This also questions the validity of the stratified results in table 4.</p> <p>More specifically, in table 2 it is shown that there is a statistically significant higher prevalence of medication against low mood or</p>

	<p>depression more than 10 years ago (OR: 1.5 (95%CI 1.0 – 2.4) in suicide bereaved than non-bereaved parents. Although this was not significant this also regarded a first psychiatric diagnosis and medication against anxiety.</p> <p>This means that the conclusion from the authors is not supported by the data.</p>
GENERAL COMMENTS	<p>This is a valuable data set. In my view it is possible to redo the analysis and draw more credible conclusions. I sincerely hope that the authors will engage in this as the topic is very important.</p>

VERSION 1 – AUTHOR RESPONSE

1. Reviewer: Annette Erlangsen

1.1 I believe that there is a substantial body of American studies on bereaved by suicide, which - despite potential methodological concerns - might address related objectives and might be worth referencing.

Response: We have added the references below and the following text to the introduction (page 6):

Suicide-bereaved family members may have an increased vulnerability for psychological morbidity due to genetic and environmental factors such as psychiatric illness, personality traits and suicidal behaviour¹²⁻¹⁴

8. Feigelman W, Jordan JR, Gorman BS. How they died, time since loss, and bereavement outcomes. *Omega* 2008;58:251-273.
9. Jordan JR. Is suicide bereavement different? A reassessment of the literature. *Suicide Life Threat Behav* 2001;31:91-102.
10. Clark S. Bereavement after suicide--how far have we come and where do we go from here? *Crisis* 2001;22:102-108.
11. Cvinar JG. Do suicide survivors suffer social stigma: a review of the literature. *Perspect Psychiatr Care* 2005;41:14-21.
12. Baldessarini RJ, Hennen J. Genetics of suicide: an overview. *Harv Rev Psychiatry* 2004;12:1-13.
13. Brent DA, Mann JJ. Familial pathways to suicidal behavior--understanding and preventing suicide among adolescents. *N Engl J Med* 2006;355:2719-2721.
14. Tidemalm D, Runeson B, Waern M, et al. Familial clustering of suicide risk: a total population study of 11.4 million individuals. *Psychol Med* 2011;1:1-8.

1.2 It is an interesting study examining outcomes related to mental and physical health of parents before and after bereavement by child's suicide. The study has an acceptable response rate, which lowers the risk of sampling bias. Also, it is laudable that the authors examine for pre-existing conditions prior to bereavement (see below). The questions of the scales are very specific and likely to be underreported for the period preceding the loss.

Response: We thank the reviewer for the positive remarks.

Minor comments:

1.3 You might want to begin you Result sentence in the Abstract with "In all,".

Response: We have corrected the sentence according to suggestion.

In all, 94 (14%) suicide-bereaved and 51 (14%) non-bereaved parents (relative risk 1.0; 95% CI 0.8 to 1.4) had received their first treatment for psychological problems or had been given a psychiatric diagnosis more than ten years earlier.

1.4 Consider skipping the last sentence of the Introduction in the main manuscript – it is covered in the following section. You could add a sentence on the relevance of the study if preferred.

Response: We have removed the last sentence of the Introduction and added the sentences:

In a recent Canadian register study previous psychological morbidity was found to be more prevalent among suicide-bereaved parents than non-bereaved control parents;⁹ we do not know if these results are relevant for European communities.

1.5 Is there any clear definition of the timing of the questions for the pre- and post-bereavement conditions? Were the questions specifically formulated as 'before' and 'after' for the bereaved while the terms 'more than 10 years ago' and 'less than 10 years ago' were used for the matched controls? It would be good with a deeper explanation of this in the Method section and an equivalent consideration in the strength/limitations of the Discussion.

Response: The definition of premorbidity was identical for the bereaved and non-bereaved, but our previous specification of the answer alternatives for the premorbidity questions were unclear and the footnote of Table 2 has been clarified. The bereaved and non-bereaved were both given the opportunity to specify "more than 10 years earlier" or "during the last 10 years", while the bereaved who responded "during the last 10 years" were asked to further specify if this was before or after the child's death. Premorbidity was defined as a response of "more than 10 years earlier" to any of the four questions, for both participant groups. We have added the pre-bereavement questions to the method section:

We used four questions with a follow-up question to measure psychological premorbidity: 1) "Have you ever been given treatment for psychological problems such as depression, anxiety, psychosis, or personality disorder?" Treatment was defined as treatment prescribed by a physician, e.g. medication, electroconvulsive therapy [ECT], or conversational therapy. "If yes, when did you receive your first treatment?" 2) "Have you ever been given a psychiatric diagnosis e.g. depression, panic disorder, psychosis, or personality disorder?" "If yes, when were you given your first diagnosis?" 3) "Have you during a period of your life medicated against anxiety?" "If yes, when did you take your first medication?" 4) "Have you during a period of your life medicated against low mood or depression?" "If yes, when did you take your first medication?" The answer categories were "more than 10 years earlier" and "during the last 10 years" for the non-bereaved and "more than 10 years earlier", "during the last 10 years, before my child's death" and "during the last 10 years, after my child's death" for the non-bereaved.

1.6 Also, if I understand the footnote of Table 2 correctly 'during last 10 years' was interpreted as 'after child's death' – how does this fit with the actual timing of the bereavement?

Response: Please see the response to the previous question.

1.7 Please express the similarities presented in Table 1 in more statistical terms, i.e. by demonstrating that there are no significant differences.

Response: We have added p-values for the comparison between suicide-bereaved and non-bereaved to Table 1 and updated the Statistical analysis section with the following:

We tested for differences in characteristics using Pearson's chi-square test and Wilcoxon-Mann-Whitney's test".

1.8 Please consider how the table texts of Table 2 and 3 can be made more reader-friendly. An example: "Persistent anxiety" instead of "Persistent anxiety 1 day/week or more". The unit could be added to the footnote.

Response: We have changed the table text according to the suggestions.

2.Reviewer: Huibert Burger

2.1 The two ORs (2.6 and 2.3) presented in table 3 and in the abstract suggest that the authors wanted to show effect modification of the association between bereavement and depression by long term psychological premorbidity. However, this specific interest is not mentioned as an hypothesis in the introduction. Nor is it in the data analysis (statistical interaction).

Response: Our intent was not to show effect modification, but rather to study whether there were differences in premorbidity between the suicide-bereaved and non-bereaved, and whether there was an association between bereavement and depression (as well as other current outcomes). For this association, psychological premorbidity has previously been pointed out as an important confounder although as of yet not carefully studied, and it was for this reason we chose to present our results stratified by premorbidity.

Also, in line with the reviewer comments, we have chosen to focus primarily on the results for the participants without premorbidity (see our response to comment 2.3 below). We have added the absolute risks to the manuscript, but prefer not to include any formal tests for interaction. We hope this is acceptable for the reviewer and the editor.

In the Results:

When stratified according to psychological premorbidity, the prevalence of moderate to severe depression among those with premorbidity was 33 of 93 (35%) among the bereaved versus 7 of 51 (14%) among the non-bereaved (RR; 2.6 95% CI 1.2 to 5.4), while among those without premorbidity the corresponding prevalences were 82 of 560 (15%) among the bereaved versus 21 of 323 (7%) among the non-bereaved (RR 2.3; 95% CI 1.4 to 3.6).

2.2 It is unclear how valid the questions about psychological premorbidity 10 years ago actually are. Has this not been validated (by others)?

Response: To the best of our knowledge, there are no validated (by others) questions created to capture psychological premorbidity. We validated all the questions in the study-specific questionnaire in a thorough four-year preparatory study described in our methodological paper published in Crisis (Omerov P, Steineck G, Runeson B, et al. Preparatory studies to a population-based survey of suicide-bereaved parents in Sweden. Crisis 2013;34:200-210.). We have

no indication that the groups of suicide-bereaved and non-bereaved parents, with and without psychological premorbidity would systematically interpret the questions differently.

2.3 It is concluded that in parents who lost a child through suicide in Sweden no higher prevalence of long-term psychological premorbidity was found than in parents who had not lost a child. This is, however, not supported by the data. In table 2 it is shown that there is a statistically significant higher prevalence of medication against low mood or depression more than 10 years ago (OR: 1.5 (95%CI 1.0 – 2.4) in suicide bereaved than non-bereaved parents. Although this was not significant this also regarded a first psychiatric diagnosis and medication against anxiety. This means that the conclusion from the authors is not supported by the data.

Response: The prevalence of psychological premorbidity, defined as occurrence more than 10 years earlier of at least one of the four factors treatment for psychological problems, a psychiatric diagnosis, medication against anxiety, or medication against low mood, was very similar between bereaved and non-bereaved (14% and 14% respectively; Table 2). This definition allows us to sort out the participants without any of the four forms of premorbidity from those with premorbidity. We did a modest interpretation in that we did not find a higher prevalence of psychological premorbidity among the suicide-bereaved which we do believe is supported by the data, but we made no comment about the severity of the premorbidity.

However, as the reviewer points out a number of these premorbidity factors were more prevalent in the group of suicide-bereaved than among the non-bereaved (although none of them were statistically significant; the RR for medication against low mood only appeared to be due to rounding of 0.95 to 1.0). This reflects that the 14% of the bereaved that reported some form of premorbidity more often indicated several of the premorbidity factors than the 14% among the non-bereaved with some form of premorbidity. This may suggest that among those with premorbidity, the bereaved may have more severe morbidities than the non-bereaved parents. On the other hand, the bereaved and non-bereaved without premorbidity should be homogenous in terms of these four premorbidity factors, and we believe that these groups are comparable.

We have therefore revised the manuscript to focus primarily on the bereaved and non-bereaved without premorbidity, and have made the following changes.

In the Abstract, Results:

In all, 94 (14%) suicide-bereaved and 51 (14%) non-bereaved parents (relative risk 1.0; 95% CI 0.8 to 1.4) had received their first treatment for psychological problems or had been given a psychiatric diagnosis more than ten years earlier. The prevalence of moderate to severe depression was 115 (18%) in suicide-bereaved versus 28 (7%) in non-bereaved (RR 2.3; 95% CI 1.6 to 3.5). For those without psychological premorbidity, the relative risk was 2.3 (95% CI 1.4 to 3.6).

In the Abstract, Conclusions:

In parents who lost a child through suicide in Sweden we did not find a higher prevalence of long-term psychological premorbidity than among parents who had not lost a child; the more than twofold risk of depression among the bereaved, can probably be explained by the suicide and the stressful time preceding the suicide.

In Results, Primary outcomes:

Altogether 94 of 663 (14%) suicide-bereaved and 51 of 377 (14%) non-bereaved parents (RR 1.0;

95% CI 0.8 to 1.4) reported that they had received their first treatment for psychological problems or had been given a psychiatric diagnosis more than ten years earlier, although the bereaved had somewhat higher prevalences for all the individual single-item questions (table 2).

In Discussion:

However, the 14% of the bereaved with premorbidity more often reported several forms of premorbidity as compared to the 14% of the non-bereaved with premorbidity, possibly reflecting more severe afflictions. Among those without premorbidity, the bereaved parents had a more than twofold higher risk of moderate to severe depression two to five years after the loss, as measured by PHQ-9.

In Discussion, Conclusions:

Our finding that the suicide-bereaved parents' prevalence of psychological premorbidity was not higher than the non-bereaved parents' prevalence adds important information for further intervention studies. The knowledge is also valuable for contradicting the prejudiced assumption that suicide primarily occurs in especially vulnerable families.

We have also added a footnote to Table 2 to clarify that the RR of medication for low mood or depression was not statistically significant.

2.4 It is unclear why the investigators use two different regression, i.e. log-binomial and logistic. Why not use logistic regression throughout?

Response: We believe that relative risks are easier than odds ratios to comprehend, which is a concern to us as we believe our results may be important to clinicians who are not always familiar with statistical methodology. For example prevalences of 30% vs 10% will give a relative risk of 3.0, but an odds ratio of 3.9, and if this OR were to be presented by itself it might be misinterpreted as reflecting a near fourfold higher occurrence while the occurrence is actually threefold. The problem of misinterpreted odds ratios has been discussed in the literature [Davies HT et al. When can odds ratios mislead? *BMJ* 1998;316(7136):989-91; Schmidt CO & Kohlmann T. When to use the odds ratio or the relative risk? *Int J Public Health* 2008;53(3):165-7] As we wrote in the Methods section, we tried to use log-binomial regression and relative risks throughout our calculations but had to use odds ratios for the multivariable results when log-binomial regression did not converge and failed to produce estimates, which is a well-known issue with this model [McNutt LA et al. Estimating the relative risk in cohort studies and clinical trials of common outcomes. *Am J Epidemiol* 2003;157(10):940-3].

We prefer to keep the results as they are, using relative risks when possible.

2.5 The question of interaction between premorbidity and bereavement effect is not in the analysis.

Response: Our intent was not to show effect modification, but rather to study whether there were differences in premorbidity between the suicide-bereaved and non-bereaved, and whether there was an association between bereavement and depression (as well as other current outcomes). For this association, psychological premorbidity has previously been pointed out as an important confounder although as of yet not carefully studied, and it was for this reason we chose to present our results stratified by premorbidity.

2.6 Although the limitation that the increased risks of depression may not be specific to bereavement by suicide but may also be implicated in other causes of bereavement is adequately discussed . However, the authors have failed to refer to a recent study in which it was shown that parents who lost their child through a motor vehicle accident had similarly increased depression risks as those by bereavement (Bollon et al. JAMA Psychiatry. 2013;70(2):158-167)

Response: We did actually refer to this article by Bolton et al. (see reference Bolton JM, Au W, Leslie WD, et al. Parents Bereaved by Offspring Suicide: A Population-Based Longitudinal Case-Control Study. Arch Gen Psychiatry 2012;1-10.). However, at the time of our submission only the e-published article was available, at which time JAMA Psychiatry was still named Archives of General Psychiatry (the journal changed name in 2013). We have now updated the reference accordingly.

Bolton JM, Au W, Leslie WD, et al. Parents bereaved by offspring suicide: a population-based longitudinal case-control study. JAMA Psychiatry 2013;70:158-167.

2.7 The results are not credible in my opinion. The OR of 1.0 in table 2 on which the main conclusion is based is hard to interpret given the other OR in that table of which 3 were higher than 1.0.

This also questions the validity of the stratified results in table 4.

More specifically, in table 2 it is shown that there is a statistically significant higher prevalence of medication against low mood or depression more than 10 years ago (OR: 1.5 (95%CI 1.0 – 2.4) in suicide bereaved than non-bereaved parents. Although this was not significant this also regarded a first psychiatric diagnosis and medication against anxiety.

This means that the conclusion from the authors is not supported by the data.

Response: As mentioned above, the definition allows us to distinguish the vast majority among both bereaved and non-bereaved that reported none of the premorbidities, two groups which should be comparable. Please see our response to comment 2.3 for a full list of our revisions.

2.8 This is a valuable data set. In my view it is possible to redo the analysis and draw more credible conclusions. I sincerely hope that the authors will engage in this as the topic is very important.

Response: We thank the reviewer for the encouraging remarks concerning our data set, and hope that the reviewer and the editor will be satisfied with our revised manuscript.

VERSION 2 – REVIEW

REVIEWER	Annette Erlangsen, PhD Seniorforsker/ Senior researcher Psykiatrisk Center København/Mental Health Center Copenhagen Denmark
REVIEW RETURNED	12-Jul-2013

GENERAL COMMENTS	Please check sentence 16-18 on page 8 ("The answer categories..."): the last part on "after my child's death" for the non-bereaved" seems to be a copy/paste error.
-------------------------	---

REVIEWER	Huibert Burger
-----------------	----------------

	Associate Professor University Medical Center Groningen The Netherlands
REVIEW RETURNED	17-Jul-2013

GENERAL COMMENTS	I feel that the authors have very adequately dealt with my comments. I have no more remarks.
-------------------------	--