PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Bereavement by suicide as a risk factor for suicide attempt: a cross-sectional national UK-wide study of 3,432 young bereaved adults
AUTHORS	Pitman, Alexandra; Osborn, David; Rantell, Khadija; King, Michael

VERSION 1 - REVIEW

REVIEWER	Massimiliano Beghi A.O. "G.Salvini", Garbagnate Milanese, Italy
REVIEW RETURNED	03-Oct-2015

GENERAL COMMENTS	I read with interest this manuscript. The study develops an interesting topic. Methods are adequate. I have only few queries to improve the manuscript in my opinion. 1 In the methods section please give a clear definition of suicide attempt. 2 Still in the methods section please spell "APMS" and "CIDI". 3 In the statistical analysis (line 7) you affirm you use a different threshold for primary and secondary outcome. Please clarify this difference. 4 You did not find any difference between suicide attempt and the relationship with the decease. Is it possible to investigate the correlation between the relatedness with the decease and depression? 5 You found differences in social functioning and in post bereavement drop outs. In the discussion I did not find any speculation by the authors on these results. Please discuss more deeply this point.

REVIEWER	James Bolton University of Manitoba, Canada
REVIEW RETURNED	27-Oct-2015

GENERAL COMMENTS	This is an excellent article by Dr. Pitman and colleagues studying a very under-researched topic. The data is extremely interesting and a significant amount of work went into collecting and analyzing this data. A priori and post hoc analyses were thoughtful, well planned and add to the body of knowledge on the subject. The data is interesting in that it suggests that it is not the violence associated with the traumatic death that contributes to the mental health and occupational outcomes but rather the very nature of the suicidal death in itself. The use of large, national survey data to address these research questions is important and largely unavailable in the field. As such, this study constitutes an important contribution to the knowledge base in suicide bereavement. Some concerns that should be addressed are as follows: 1. Rationale should be further discussed regarding the choice of 10

	as a cut-off for bereavement experience. 2. Discussion: This is not the first study to demonstrate a link between suicide bereavement and suicide-related outcomes. See Rostila Soc Psych Psychiatric Epi 2014, 3. Limitations of the study need to be elaborated. While survey data offers many advantages (capturing non-treated suicide attempts and NSSH), it has limitations in the accuracy of causes of death. Respondent recall bias is a concern. The mean years since bereavement is 5, and memories of the cause of death, and even SI and SA in the interim may be inaccurate. Coroner misclassification of sudden unnatural deaths is a recognized problem in suicide research – many of these may be suicides (overdoses, MVC's, falls, etc). The authors should discuss this as a possible explanatory mechanism for the non-significant findings between these 2 groups. This also calls into question the rationale of even conducting a comparison analysis between these 2 groups. 4. This study is biased by its sampling of higher SES subgroups, and this is not sufficiently mentioned throughout the manuscript. The first paragraph of the discussion is an example – wording needs to be contextualized to reflect that this is not a nationally representative sample. Unnatural deaths (including suicides) are vastly overrepresented in low SES subgroups, and thus there needs to be caution in the wording when interpreting this results. 5. There is no mention of shared genetics as a possibly explanatory mechanism for the findings. 6. The possibility of suicide death among the bereaved is a confounder not discussed.

REVIEWER	Donald Robinaugh Massachusetts General Hospital & Harvard Medical School United States of America
REVIEW RETURNED	11-Nov-2015

GENERAL COMMENTS	The authors describe a study in which they examined suicidality and other psychosocial outcomes in those bereaved by suicide relative to those bereaved by other types of sudden death. They report that suicide bereavement was associated with increased risk for suicide attempt relative to those who were bereaved by sudden natural causes and that this increased risk remained significant after adjusting for potential confounders. This study addresses an important topic in bereavement and suicide research and makes a noteworthy contribution to the literature. I have some concerns and comments for the reviewers to consider. Introduction (1) The rationale for the primary aim of the study is communicated clearly and in great detail in the introduction. In contrast, it is not clear why Hypothesis 2 is included in the current paper nor is it clear why the authors chose to focus on these particular secondary measures. These analyses do not seem to bear on the focus of the current study (i.e., whether suicide bereavement confers greater risk for suicide outcomes than does other types of sudden bereavement) and, in this reviewer's opinion, could be removed. If the authors believe it is important to retain these analyses in the current manuscript, they should provide the rationale for doing so and for examining these factors and not others included in the survey (e.g., social support).

	(2) It would similarly be helpful for the authors to provide more detail about the rationale for examining blood-relatedness as a moderator. (3) The rationale for looking at stigma is made clear in the introduction. However, it is not clear why the authors chose to examine only stigma as a factor that can "explain" the risk conferred by suicide bereavement. In their introduction, the authors identify four factors that might explain this association (grief, shared environmental/familial risk, contagion, and stigma). If data are available on these factors, the authors should consider revising Hypothesis 4 to incorporate those factors. Method (4) The authors note that they contacted a diverse range of HEIs, however it is not clear to what extent these different types of HEIs participated in the study. Were there differences in the rate of participation? (5) It is not clear why the authors conducted their power analysis to address a comparison between the UK community prevalence and that of the bereavement groups. Did the authors also conduct a power analysis to determine the sample size needed for adequate power to detect the between-group differences that were the focus of this study? If so, it should be reported. This is especially noteworthy given later suggestions that some of the analyses may have been underpowered. (6) It appears that the authors grouped spousal/partner loss into the non-relative category. This grouping is potentially problematic given that spousal loss has consistently been shown to be associated with worse outcomes relative to other types of loss, including the loss of a friend. Accordingly, caution should be warranted in drawing conclusions about peer loss on the basis of these findings. The authors may consider running these analyses with only those reporting the loss of a friend in the 'non-relative' group to determine if there continues to be a lack of moderating effect. Results/Discussion (7) The finding that suicide bereavement was associated with increased risk for suicide attempts but not suicidal ideation is noteworthy and worthy of further discussion. As the authors note, this finding suggests the possibility that suicide bereavement increases risk not for suicidal thoughts, but for the transition from thoughts to attempt. The authors may consider doing more to discuss this finding, its implications for intervention, and next steps in research to further examine this finding. (8) The authors dismiss the null result between suicide bereavement and unnatural sudden death as having arisen due to a lack of power. The authors should clarify the magnitude of effect they were powered to detect with this sample size in order to provide context for this claim. The authors may also consider discussing the possibility that they have appropriately retained the null hypothesis and the implications of that possibility. Relatedly, in the discussion the authors characterize this as a "secondary comparison" relative to the "primary comparison" using

	natural sudden bereavement as the comparison group. It is not clear what is "secondary" about these analyses as both are identified as the primary aim of the current manuscript in the introduction. (9) The authors' suggestion that stigma can "explain" the increased risk for suicide attempts conferred by suicide bereavement seems tantamount to making the claim that stigma acts as a mediator between these variables. Such a claim goes beyond the data presented here. The fact that the AOR is attenuated when including stigma in the model is not sufficient evidence for mediation (or "explanation"). If the authors wish to draw such conclusions, they should consider reporting the association between stigma and suicidal attempts and conducting a mediation analysis (though, given the cross-sectional nature of these data, even that analysis would be very limited). If the authors are not proposing this mediation model, they may consider revising the discussion to avoid this implication. Relatedly, the authors suggestion that stigma may act as a marker of thwarted belongingness does not seem consistent with their findings regarding social support, in which suicide bereaved adults did not report lower perceived social support. (10) Consistent with past research (e.g., Bolton et al., 2013), the findings from this study suggest that suicide bereaved adults are a vulnerable group prior to bereavement. In the current study, the suicidal bereaved group reported elevated family history of psychiatric problems and family history of suicide as well as elevated pre-bereavement suicide attempt, NSSI, and depression. These findings are worthy of discussion in the manuscript, including the implications of this finding for the possibility that suicide bereaved adults may be at greater risk due to shared environmental/familial risk. Minor Comments (1) In the introduction, the authors note that suicide bereaved adults may perceive a loss of community support. It is noteworthy that no such loss of social support was observed in the suicide bereaved adults relative to those with other types of sudden loss in the current study. (2) Previous studies have found that suicide bereavement is associated with increased rates of depression relative to other types of bereavement, including sudden unnatural bereavement. Accordingly, it is noteworthy that such a finding was not observed in the current study, especially given that the suicide bereaved group reported greater pre-bereavement depression. (3) The authors are inconsistent in their description of the groups used in hypothesis 3, sometimes referring to one group as "relatives" and other times "blood-relatives" and referring to the other group as "non-relatives." To avoid confusion, the authors should clarify explicitly the types of relations in each group and use consistent terms throughout that appropriately reflect the types of relationships represented in those groups. (4) The term "international evidence" is used several times in the discussion. It is not clear what is meant by this term. (5) In the abstract, the authors report what appears to be the AOR

	for suicide bereavement relative to natural sudden death when including stigma in the model. However, in the current text of the abstract, it is not clear what this AOR represents.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Massimiliano Beghi
A.O. "G.Salvini", Garbagnate Milanese, Italy

I read with interest this manuscript. The study develops an interesting topic. Methods are adequate. I have only few queries to improve the manuscript in my opinion.

1 In the methods section please give a clear definition of suicide attempt.

The wording of the question used has now been added

2 Still in the methods section please spell "APMS" and "CIDI".
These are both added.

3 In the statistical analysis (line 7) you affirm you use a different threshold for primary and secondary outcome. Please clarify this difference.

We used a significance threshold of p=0•05 for primary outcomes and chose to use a more stringent threshold of p=0•01 for secondary outcomes to reduce the potential for chance findings, and have drawn attention to this strength in our Discussion.

4 You did not find any difference between suicide attempt and the relationship with the decease. Is it possible to investigate the correlation between the relatedness with the decease and depression?

In the section headed Kinship as a potential effect modifier we have clarified that we had tested for an interaction with kinship in all our models, and there was no evidence for effect modification, either in relation to significant or non-significant associations.

5 You found differences in social functioning and in post bereavement drop outs. In the discussion I did not find any speculation by the authors on these results. Please discuss more deeply this point.

We have added to the clinical implications that employers should be aware of the impact of suicide bereavement on occupational functioning, and make adjustments to promote workplace mental health.

Reviewer: 2
James Bolton
University of Manitoba, Canada

This is an excellent article by Dr. Pitman and colleagues studying a very under-researched topic. The data is extremely interesting and a significant amount of work went into collecting and analyzing this data. A priori and post hoc analyses were thoughtful, well planned and add to the body of knowledge on the subject. The data is interesting in that it suggests that it is not the violence associated with the traumatic death that contributes to the mental health and occupational outcomes but rather the very nature of the suicidal death in itself. The use of large, national survey data to address these research questions is important and largely unavailable in the field. As such, this study constitutes an important contribution to the knowledge base in suicide bereavement. Some concerns that should be addressed are as follows:

1. Rationale should be further discussed regarding the choice of 10 as a cut-off for bereavement experience.

We have explained that we chose this because this is the age of criminal responsibility in England and Wales, and defines the age at which an individual is deemed mature enough to be tried legally for an offence in court. It was therefore chosen as the age representing the threshold for adult cognition. Exclusion of bereavements prior to this also reduces the potential for recall bias of events or processes in childhood.

2. Discussion: This is not the first study to demonstrate a link between suicide bereavement and suicide-related outcomes. See Rostila Soc Psych Psychiatric Epi 2014,

We agree that this and other studies had previously demonstrated a link between suicide bereavement and suicide-related outcomes. Our systematic review, which we referred to in the Introduction, had highlighted many studies (using bereaved controls) investigating suicide-related outcomes following bereavement by suicide and other violent causes - (Agerbo,2003; Agerbo, 2005; Bolton et al, 2013; Segal, 2009; Qin & Mortenson, 2003), and we refer to Qin & Mortenson, 2003 and Agerbo 2005 in the introduction. However we felt our study was the first to confirm that the risk was specific to suicide bereavement, and not due to unnatural causes per se. We have tried to make this clearer in the text.

3. Limitations of the study need to be elaborated. While survey data offers many advantages (capturing non-treated suicide attempts and NSSH), it has limitations in the accuracy of causes of death. Respondent recall bias is a concern. The mean years since bereavement is 5, and memories of the cause of death, and even SI and SA in the interim may be inaccurate. Coroner misclassification of sudden unnatural deaths is a recognized problem in suicide research – many of these may be suicides (overdoses, MVC's, falls, etc). The authors should discuss this as a possible explanatory mechanism for the non-significant findings between these 2 groups. This also calls into question the rationale of even conducting a comparison analysis between these 2 groups.

We realise that we had not covered recall bias at all in the limitations and have added this ("Recall bias may have influenced judgements about the onset and severity of difficulties, particularly among those bereaved by violent causes, with the potential to over-estimate risks in these groups"). In this study mode of death was defined subjectively by the respondent, and not by coroner's verdict or death certificate, as perception of bereavement type was the exposure of interest. This means that coroner misclassification of suicides as accidental deaths was less of a problem than in other studies as we used the respondent's perception of cause of death, with minimal potential for respondent misclassification. We have added this to the methods and discussion as a clarification.

4. This study is biased by its sampling of higher SES subgroups, and this is not sufficiently mentioned throughout the manuscript. The first paragraph of the discussion is an example – wording needs to be contextualized to reflect that this is not a nationally representative sample. Unnatural deaths (including suicides) are vastly overrepresented in low SES subgroups, and thus there needs to be caution in the wording when interpreting this results.

We have added the suggested text to the first paragraph of the Discussion ("Such findings must be interpreted in the context of a highly-educated sample, in which exposure to violent losses may be lower than in a more nationally representative (but harder to recruit) sample."), picking up on where this is echoed later in the Discussion, and in the Strengths and Limitations boxes.

5. There is no mention of shared genetics as a possibly explanatory mechanism for the findings.

In the Introduction we mention shared familial and environmental risk as an explanation, but I have clarified that the aim of Hypothesis 3 was to assess whether the impact of suicide bereavement extends beyond genetic relatedness.

6. The possibility of suicide death among the bereaved is a confounder not discussed.

The discussion includes mention of survivor bias as potentially resulting in an underestimation of risks due to the higher probability of completed suicide in those who had experienced suicide bereavement.

Reviewer: 3
Donald Robinaugh
Massachusetts General Hospital & Harvard Medical School

The authors describe a study in which they examined suicidality and other psychosocial outcomes in those bereaved by suicide relative to those bereaved by other types of sudden death. They report that suicide bereavement was associated with increased risk for suicide attempt relative to those who were bereaved by sudden natural causes and that this increased risk remained significant after adjusting for potential confounders. This study addresses an important topic in bereavement and suicide research and makes a noteworthy contribution to the literature. I have some concerns and comments for the reviewers to consider.

Introduction

(1) The rationale for the primary aim of the study is communicated clearly and in great detail in the introduction. In contrast, it is not clear why Hypothesis 2 is included in the current paper nor is it clear why the authors chose to focus on these particular secondary measures. These analyses do not seem to bear on the focus of the current study (i.e., whether suicide bereavement confers greater risk for suicide outcomes than does other types of sudden bereavement) and, in this reviewer's opinion, could be removed. If the authors believe it is important to retain these analyses in the current manuscript, they should provide the rationale for doing so and for examining these factors and not others included in the survey (e.g., social support).
We have added more detail on the rationale for hypotheses 2-4, as this was lacking. In relation to hypothesis 2, we were keen to capture wider clinical and occupational impacts, both due to service user input and wider concerns (expressed in 2009 NICE guidelines on mental wellbeing at work) about the contribution of bereavement to depression, anxiety, stress, and sickness absence. Although these could be taken out and reported in another paper, we felt that presenting the results together provided a broader picture of the impact of suicide bereavement. It is particularly notable that there were no group differences in depression, effectively ruling this out as an explanatory factor.

(2) It would similarly be helpful for the authors to provide more detail about the rationale for examining blood-relatedness as a moderator.

Reviewer 2 also mentioned this. We had included shared familial and environmental risk in our Introduction as an explanatory factor, but I have clarified that the aim of Hypothesis 3 was to assess whether the impact of suicide bereavement extends beyond genetic relatedness to peer suicides. Whilst family history of suicide is a long-established risk factor for suicidal behaviour, we were interested in isolating the impact of suicide as an environmental exposure.

(3) The rationale for looking at stigma is made clear in the introduction. However, it is not clear why the authors chose to examine only stigma as a factor that can “explain” the risk conferred by suicide bereavement. In their introduction, the authors identify four factors that might explain this association (grief, shared environmental/familial risk, contagion, and stigma). If data are available on these factors, the authors should consider revising Hypothesis 4 to incorporate those factors.

We have expanded the rationale for each of our pre-specified hypotheses, to stress why stigma (being potentially more modifiable than grief or shared environmental/familial risk) was of greatest interest. The difficulties of isolating the ‘contagion effect’ are legion and we could not measure this using a cross-sectional study design, but hope to try and investigate this in future study designs.

Method

(4) The authors note that they contacted a diverse range of HEIs, however it is not clear to what extent these different types of HEIs participated in the study. Were there differences in the rate of participation?
This was an omission and we have added to our method the higher response (40%) from HEIs classified as the more prestigious Russell Group of universities, reflecting the study design’s bias towards sampling of higher SES subgroups.

(5) It is not clear why the authors conducted their power analysis to address a comparison between the UK community prevalence and that of the bereavement groups. Did the authors also conduct a power analysis to determine the sample size needed for adequate power to detect the between-group differences that were the focus of this study? If so, it should be reported. This is especially noteworthy given later suggestions that some of the analyses may have been underpowered.

At the study design phase we used the best available information regarding prevalence of suicidal ideation and attempt, which derived from UK community data. This was our best estimate of the rate in the reference group of those bereaved by natural causes. Because there were no figures for suicidality in representative UK samples of bereaved young adults we chose a relatively high effect size, aiming to detect a two fold increase over and above this baseline (rather than the 30% one might see elsewhere). We were conservative in our calculations, using 90% power and a two-sided calculation, and focussing on the lowest frequency exposure subgroups, namely the sudden unnatural death and the suicide bereavement groups. The sample size calculations indicated that we needed a minimum of 466 in these subgroups. We exceeded this number by 53% and 30% per subgroup respectively. The first comparison of people bereaved by suicide to those bereaved by sudden natural causes had far greater statistical power, since this form of bereavement was far more common (n=2106). The explanation of our sample size calculation has been amended accordingly: “We chose a relatively large effect size to reflect our comparison to a non-bereaved baseline, lacking prevalence figures for bereaved UK samples”.
(6) It appears that the authors grouped spousal/partner loss into the non-relative category. This grouping is potentially problematic given that spousal loss has consistently been shown to be associated with worse outcomes relative to other types of loss, including the loss of a friend. Accordingly, caution should be warranted in drawing conclusions about peer loss on the basis of these findings. The authors may consider running these analyses with only those reporting the loss of a friend in the ‘non-relative’ group to determine if there continues to be a lack of moderating effect.

Because of the relatively young age of the sample, the spousal/partner category was very small (3% of sample) in comparison to the friend category (21%). When split into the relatives and non-relatives, 74% described the loss of a friend, 11% described loss of a spouse, 4% described the loss of an ex-partner, and 12% described the loss of a non-blood relative (step/adoptive/in-law). This breakdown has been expanded in the Participant characteristics. We re-ran the interaction tests excluding these latter 3 groups, but again there was no effect modification, suggesting that the association applied to peer loss. This was very interesting for the reasons you describe (particularly the findings of Agerbo 2005) and we have added this to our Results ("This was the case even when excluding the 253 respondents who reported the death of a partner, ex-partner, or non-blood relative, to describe associations in a group bereaved by peer death.").

Results/Discussion

(7) The finding that suicide bereavement was associated with increased risk for suicide attempts but not suicidal ideation is noteworthy and worthy of further discussion. As the authors note, this finding suggests the possibility that suicide bereavement increases risk not for suicidal thoughts, but for the transition from thoughts to attempt. The authors may consider doing more to discuss this finding, its implications for intervention, and next steps in research to further examine this finding.

This is a really interesting point, which Rory O'Connor addresses in his IMV model, but due to word count restrictions we thought we would reference his and Matthew Nock's very useful summary, which discusses this acquired capability for suicide and key directions for future psychological research.

(8) The authors dismiss the null result between suicide bereavement and unnatural sudden death as having arisen due to a lack of power. The authors should clarify the magnitude of effect they were powered to detect with this sample size in order to provide context for this claim. The authors may also consider discussing the possibility that they have appropriately retained the null hypothesis and the implications of that possibility.

On reflection we agree with the reviewer's point, especially given the comments in (5) above about power, and the paper recently published by one of our team (Wood J, Freemantle N, King M, Nazareth I. Trap of trends to statistical significance: likelihood of near significant p value becoming more significant with extra data. BMJ 2014 Mar 31;348). We feel it was a mistake to describe this comparison as underpowered and have amended references to that. We have noted the lack of differences between the group bereaved by sudden unnatural causes and either of the other groups, and the inference that this group cannot be considered at clear higher risk of adverse mental health outcomes.

Relatedly, in the discussion the authors characterize this as a "secondary comparison" relative to the "primary comparison" using natural sudden bereavement as the comparison group. It is not clear what is "secondary" about these analyses as both are identified as the primary aim of the current manuscript in the introduction.

We agree this was a confusing term, and so we have changed this to separate instead of secondary.

(9) The authors' suggestion that stigma can "explain" the increased risk for suicide attempts conferred by suicide bereavement seems tantamount to making the claim that stigma acts as a mediator between these variables. Such a claim goes beyond the data presented here. The fact that the AOR is attenuated when including stigma in the model is not sufficient evidence for mediation (or "explanation"). If the authors wish to draw such conclusions, they should consider reporting the association between stigma and suicidal attempts and conducting a mediation analysis (though, given the cross-sectional nature of these data, even that analysis would be very limited). If the authors are not proposing this mediation model, they may consider revising the discussion to avoid this implication. Relatedly, the authors suggestion that stigma may act as a marker of thwarted belongingness does not seem consistent with their findings regarding social support, in which suicide bereaved adults did not report lower perceived social support.

Given the statistical approach we had defined a priori, we agree we should have been more tentative in our conclusions, and have edited our comments relating to this. Adding stigma to the main associations was exploratory and simply provides an indication that it might be a mediator, so we have removed references to it explaining any associations. We agree this is an interesting area and are postulating a possible pathway, but given the limitations of our cross-sectional data we did not feel formal mediation analysis would be appropriate. Instead we have revised the discussion as suggested, and been more circumspect in our conclusions. We have also deleted some of the clinical implications in relation to anti-stigma interventions.
The lack of group differences on social support is striking, but in the text we have now emphasised that the measure of social support we used was current social support, as there is no validated scale for social functioning at the time of the loss. Given wide variation in time since loss, some report a level of current social support very different to that at the time of the loss. We are also preparing a paper describing group differences in stigma, shame, responsibility and guilt scores, and discussing how the clear group differences observed are discordant with the lack of group differences in current social support.

(10) Consistent with past research (e.g., Bolton et al., 2013), the findings from this study suggest that suicide bereaved adults are a vulnerable group prior to bereavement. In the current study, the suicidal bereaved group reported elevated family history of psychiatric problems and family history of suicide as well as elevated pre-bereavement suicide attempt, NSSI, and depression. These findings are worthy of discussion in the manuscript, including the implications of this finding for the possibility that suicide bereaved adults may be at greater risk due to shared environmental/familial risk.

We had drawn attention to those findings of Bolton et al, (as well as similar findings of Seguin et al 1995, Cerel et al 1999, Cerel et al 2000, Brown et al, 2007; & Melhem et al, 2008), in our systematic review, but have now added this to our Methods (“These reflected the observed vulnerabilities of people bereaved by suicide, even before the loss, which are likely to reflect shared familial and environmental risk.”), to qualify our choice of confounders. We have also added this at a number of points in our Discussion.

Minor Comments

(1) In the introduction, the authors note that suicide bereaved adults may perceive a loss of community support. It is noteworthy that no such loss of social support was observed in the suicide bereaved adults relative to those with other types of sudden loss in the current study.

We used a measure of current social support, because our systematic review had indicated there was no valid way of measuring pre-bereavement social support. As mentioned earlier, we are preparing a paper on stigma that explores the relationship between stigma and social support in more detail.

(2) Previous studies have found that suicide bereavement is associated with increased rates of depression relative to other types of bereavement, including sudden unnatural bereavement. Accordingly, it is noteworthy that such a finding was not observed in the current study, especially given that the suicide bereaved group reported greater pre-bereavement depression.

We had not explored this in the reviewed manuscript due to reasons of word count, but have added this briefly at the point where we note the lack of association with suicidal thoughts.

(3) The authors are inconsistent in their description of the groups used in hypothesis 3, sometimes referring to one group as “relatives” and other times “blood-relatives” and referring to the other group as “non-relatives.” To avoid confusion, the authors should clarify explicitly the types of relations in

each group and use consistent terms throughout that appropriately reflect the types of relationships represented in those groups.

We agree this is confusing, and we have ensured that this is both clearer and more consistent throughout.

(4) The term “international evidence” is used several times in the discussion. It is not clear what is meant by this term.

Again, we agree this is confusing and this term has been removed

(5) In the abstract, the authors report what appears to be the AOR for suicide bereavement relative to natural sudden death when including stigma in the model. However, in the current text of the abstract, it is not clear what this AOR represents.

To clarify, and reduce the emphasis on explanatory factors, we have changed this to “The significant association between bereavement by suicide and suicide attempt became non-significant when adding perceived stigma (AOR=1•11; 95% CI=0•74-1•67; p=0•610).”

VERSION 2 – REVIEW

REVIEWER	Massimiliano Beghi A.O. G.Salvini, Garbagnate Milanese
REVIEW RETURNED	19-Dec-2015

GENERAL COMMENTS	The current version satisfied all my previous queries

REVIEWER	Donald Robinaugh Massachusetts General Hospital United States of America
REVIEW RETURNED	23-Dec-2015

GENERAL COMMENTS	The authors have thoroughly responded to each of my comments and concerns. The study makes a noteworthy contribution to the literature and, in this revised draft, the authors have improved the clarity of the study rationale, aims, and results. I have only one minor comment. In the discussion, the authors now state that "The non-significant differences in the probability of suicidality and depression when comparing adults bereaved by suicide and by sudden unnatural causes are only indicators of difference, requiring more research." Although their efforts to address my earlier concerns are appreciated, it is not clear to me what is meant by "only indicators of difference." The authors may consider elaborating further to clarify their meaning. They may also consider making suggestions for future research on bereavement by suicide as compared to bereavement by other sudden unnatural causes.