Author’s response to reviews

Title: Attitudes to suicide following the suicide of a friend or relative: a qualitative study of the views of 429 young bereaved adults in the UK

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Author’s response to reviews:

Thank you very much for your comments and suggestions. These have been very helpful in revising the language used to describe social modelling, clarifying our sampling strategy, identifying differences between relatives and non-relatives, and revisiting the existing research evidence.

Roland Mergl (Reviewer 1):

In this article, the authors present the results of a study aiming to explore the views of young adults in the UK bereaved by suicide towards their own probability of dying by a completed suicide. For this purpose, thematic analyses of qualitative responses to a question regarding attitudes to own suicide had been conducted.
Overall, this study is clearly innovative since attitudes towards suicide after suicide bereavement had not been analysed in the context of British studies so far. The research question is clinically important because of its implications for suicide prevention. The presentation of the study is clear and the qualitative analyses had been carefully conducted, but there are some points which could be addressed within the context of a revision.

Major compulsory revisions

1. Whereas the authors have well justified whether analyses were focussed on young people it is not clear why the recruitment had been restricted to British higher education institutions. A corresponding sentence could be presented in the introduction.

Our choice of sampling method reflects the outcome of our scoping, in which we had considered recruiting a primary care sample by letter from practices in the (former) Medical Research Council General Practice Research Framework but this risked a very low response rate from a computer-literate generation unaccustomed to postal surveys. Another option was to recruit via UK coroners but this was refused. The third option was to recruit via bereavement support services, but we rejected this as it reflected only those seeking help.

Under Study Design we had mentioned that we had used the email systems of large institutions because we judged that this would be the best means of accessing hard-to-reach groups, whilst avoiding the biases associated with recruiting a help-seeking sample. We had also explained that sampling from a diverse range of colleges, universities, agricultural colleges, and art and drama schools offered unique access to a large, defined sample of young adults. However, we have added to the Introduction a clarification: (page 5; lines 109 to 112).

To access a large community sample of young adults, otherwise under-represented in health research, we chose to use the email systems of large higher education institutions, as we anticipated that this would elicit a better response than from a primary care mailshot.
2. Were there any differences between persons with a family member as suicide victim and persons with a non-relative having committed suicide regarding the frequency distribution of extracted themes (suicide as a more tangible option, awareness of shared vulnerabilities to suicide,…)? The discussion of these differences would be very interesting.

Under Theme 2 ‘Identification with the deceased and awareness of shared vulnerabilities to suicide’ we had mentioned that this theme primarily applied to relatives of the person who had died by suicide. However, we did not mention that a distinction was also apparent between relatives who focussed on the role that mental illness had played in the suicide, and non-relatives, who focussed on similar character traits as markers of vulnerability. We have now edited this section of the Results to separate out the quotes, and emphasise this distinction. Thus: (page 15; lines 347-348)

“The minority who were non-relatives recognised shared personality traits and social difficulties, understanding these as contributing to suicide risk.”

This theme also overlapped with Theme 4 of ‘Beliefs regarding safeguards against suicide’, in which respondents described a need to make active efforts to reduce the chances that they, or others, might become suicidal. Again, this primarily applied to relatives and we have added this clarification to the text of the Results. (page 17; lines 391-394)

An awareness of shared vulnerabilities to suicide (Theme 2) was implied as the basis for these behaviour strategies, representing an overlap in these themes, and again applying primarily to relatives of the person who had died by suicide.

The only other kinship differences we have identified in our analysis are those within Theme 1 - ‘Suicide as a more tangible option’. We had not emphasised this in our original submission, and the differences related to the very small group of individuals described under the subtheme of ‘Normalisation of suicide’. Within this theme we identified responses from a few participants who described suicide having been normalised for them, and to some degree romanticised. These individuals were bereaved by a friend’s suicide, and we have edited the text of our Results as follows to clarify this: (page 12; lines 285 to 286)
For a few participants bereaved by a friend’s suicide, the exposure had stimulated their curiosity in the experience.

Minor essential revisions

1. It is not obvious whether the authors intended an explorative analysis or wanted to test a hypothesis. This could be clarified at the end of the introduction.

We have clarified at the end of the Introduction: (page 5; lines 116 to 120)

To develop our theoretical understanding of these associations we also collected free text responses to conduct an exploratory analysis of qualitative data in relation to a specific research question: How does a friend or relative’s suicide influence an individual’s own attitude towards dying by suicide?

We have also added to the section within the Methods on Theoretical approach, by clarifying at the end of our discussion of possible theories that: (page 9; lines 198 – 199)

Our exploratory approach acknowledged the possibility of these and other positions being evidenced in this dataset.

2. Page 9: Suicide of a family member: It would be of interest to know the frequency distribution for different family members having committed suicide (parents, siblings, distantly related people).

We have added Supplementary Table 1 in which we provide the sociodemographic characteristics of our sample. This shows that 53% were relatives, and that the frequency
distribution of relatives is as follows: father (31%), cousin (18%), uncle/aunt (15%), mother (12%), brother (13%), grandparent (5%), sister (5%), and niece/nephew (1%).

We have added this to the Results as follows (page 10; lines 236 to 238)

Among relatives, bereavement was by the suicide of a father (31%), cousin (18%), uncle/aunt (15%), mother (12%), brother (13%), grandparent (5%), sister (5%), or niece/nephew (1%).

3. Reference 5: Volume and pages are missing.

These have now been added – thank you.

Discretionary revisions

None.

Recommendation: Accept after minor essential revisions (which the authors can be trusted to make)

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests: I declare that I have no competing interests.
Anna Mueller (Reviewer 2): This is one of the most exciting, innovative, and fascinating studies of suicide that I have read this year (and I read and review extensively in this area). I am really in awe of the authors (1) novel data, (2) robust and thorough analytic procedures (following the gold standard in qualitative research methods), (3) and extremely interesting findings that are certain to be a major contribution to the literature. Additionally, the article is a pleasure to read; it is very clear and very well written.

This study provides a substantial step towards filling a major gap in existing knowledge. Despite over 40 years of data indicating that exposure to suicide increases the risk of suicidal thoughts, attempts, and deaths among the bereaved (and exposed), we know shockingly little about what about the experience confers risk and how we might ameliorate that risk. This study details the role that attitudes play in risk, and presents compelling evidence for the complexity of the role of attitudes. If I were to simplify their study a bit, their main contribution is outlining how suicide can come to be seen as a more accessible and applicable option for some, while it becomes less so for others, after exposure to suicide. The authors detail which attitudes lead to which outcomes (at least in their sample). On top of the study’s implications for our theoretical understanding of the diffusion of suicide (something I push the authors on a bit below), the study has clear clinical implications and implications for public health that the authors detail (admittedly with some cautions about the limits of their data). It will also be an agenda setting paper as it offers great hypotheses for further testing in additional populations (e.g., non-white populations, older populations, etc.)

Though there are some limitations with the data - for example, the lack of in-depth interviews or the lack of insights regarding how social contexts further shape the bereavement experience, the authors do an admirable job with the data analysis. Indeed, they are using the gold-standard protocols for qualitative research. Additionally, though my own research on this specific topic is making its way through peer-review, my own findings from (1) an ethnography of a place with repeated adolescent suicide clusters and from (2) in-depth interviews with a general sample of suicide bereaved individuals, echoes what these authors find suggesting that the authors are really onto something robust and not idiosyncratic though they recognize there are limits to the generalizability of their findings. The authors may find Niemeyer's clinical psychological research interesting. His work is consistent with these findings. What this study has over Niemeyer and my own research is the breadth of their data, which allows some nuances to emerge that my data or Niemeyer's perhaps can't.

I hope the authors forward me the final publication when this comes out. I look forward to citing it asap. I also expect that many others will be extremely interested in this study.
Suggestions:

1. I am really not a fan of the language "imitative" suicide. This reduces what the authors show to be an extremely complex process to the mere aping of behavior. Many psychologists and psychiatrists still argue that the increased risk of suicide after exposure or bereavement is not a "real" social phenomenon (at least in the U.S. this argument is still prevalent); instead they argue it is due to shared preexisting risk factors or social selection into relationships (also called homophily or assortative relating) (Joiner, who the authors cite, has made this argument repeatedly). I suspect that this belief has multiple roots one of which is the ridiculousness of the idea that suicide could be aped. Another root is the failure of early studies to use causal modeling and longitudinal data which is essential to assessing how real this risk is.

I am also not a fan of suicide "contagion" as the word "contagion" itself brings to mind passive processes of "catching" a disease simply by exposure. I prefer the language of suicide suggestion or when appropriate suicide diffusion.

That said I realize that the authors need to be legible within the literature and I am never in favor of the use of opaque jargon; but I hope they consider adding commentary that defends/explains/problematizes whatever language they use.


We found these detailed comments very helpful in challenging our previously-held concepts of imitative suicidal behaviour, and have clarified the range of terms used to describe this phenomenon, and added key references for those terms to this part of the Introduction. These also include the references given below (Abrutyn & Mueller, 2014; Fletcher 2017; Mueller &
Abrutyn, 2015). This means that we have reedited a large section of the Introduction. Thus: (pages 3-5; lines 67 to 99)

Our earlier systematic review described the increased risk of suicide, psychiatric admission, and depression in people bereaved by suicide[2]. We also suggested various explanations for the association between suicide bereavement and suicide attempt, based on the literature and clinical observation[2]. These included: the psychological trauma of a suicide loss; shared familial or environmental vulnerabilities to mental illness and suicidality; the influence of stigma[12] on help-seeking; and suicide suggestion.[13] The last of these describes the impact of a role model’s suicide or suicide attempt on a person’s internal constraints against self-harm,[14] whether due to social learning, imitation or emotional contagion. [13] The phenomenon is described by a number of terms, none of which satisfactorily describe the phenomenon, but include; imitative suicide,[14] suicide contagion[15], or suicide diffusion.[16] Gaining an understanding of this phenomenon is the focus of the current study.

Personal attitudes to suicide are theorised to play a key role in forming suicidal ideas[14]. Despite the possibility of residual confounding due to shared social adversity and/or assortative homophily,[14] there is evidence to support the effect of suicide suggestion after exposure to a peer’s fatal or non-fatal suicide attempt[17, 18]. Young people are thought to be particularly susceptible to emulating suicidal behaviour of their peers[19], and this is thought to explain a number of well-publicised suicide clusters[20, 21]. There is also evidence that young people are more likely than their elders to hold accepting views towards suicide[22], regarding it as a means of expressing despair[23]. Adolescents and young adults who most strongly believe that it is acceptable to end one’s life are more likely to make a suicide plan than those who do not have such beliefs[24]. Longitudinal analyses of US data find that adolescents’ exposure to the suicide attempt of a friend or relative can trigger new suicidal thoughts and attempts, [13] [25] particularly in girls, [13] and after a friend’s suicide attempt, [13] but that these effects fade with time.[13] They also identify a triggering effect of exposure to suicidal behaviour in a peer’s family member.[26]

We have also amended the end of the main findings in our Discussion. Thus: (page 19; lines 434 to 441)

A small minority of our sample described a diminution of the fear of suicide, having re-evaluated it as a viable future means of escaping (and therefore controlling) threat. Such views
corresponded to theoretical constructs of social modelling of suicidal behaviour, although again this was not mentioned explicitly. However, these views also demonstrated that terms such as 'imitative suicidal behaviour' convey an over-simplified passive process, rather than the more complex process of discovering meaning in a suicide and how such meaning influences one’s attitudes and behaviour, as detected in our data.

We have used the Neimeyer references later on in the Discussion to revisit the findings of previous studies in the light of this new evidence, and to develop the idea that the specific affinity an individual has to a relative or close friend who dies by suicide (eg perceived shared vulnerabilities to mental health problems) influences the meaning they assign to the suicide, and in turn influences their subsequent mental health and their attitude to their own chances of dying by suicide. Thus, in the Discussion: (page 19; lines 446 to 451)

In keeping with sociological perspectives on bereavement,[38] our findings suggest that the specific affinity an individual has to a relative or close friend who dies by suicide (whether they were aware of shared vulnerabilities to mental health problems, or found the suicide completely unexpected) influences the meaning they assign to the suicide, and that this in turn influences their subsequent mental health and their attitudes to their own suicide.

2. My critique of the language of "imitation" also points to a missed opportunity for this study. It would add to the impact of the paper if in the discussion they leveraged their findings to provide scholars with a better understanding of what "imitation" or "contagion" is as a social phenomenon. Simplistically, the authors show that it is not just about grief, and it is not just (as Joiner also argues) about habituation to death and changes in fear. The authors' findings about control are extremely interesting and could be leveraged to help us better understand suicide suggestion.

If the authors want to push it further, sociological theory (re the role of culture or meaning in action) or phenomenology could become substantially more helpful than it currently is in the paper. Essentially, the authors' study confirms Niemeyer's insight that the meaning bereaved individuals assign to suicide after a loss is extremely important to their experience and their subsequent mental health. Niemeyer draws on phenomenology to elaborate this experience, but there is also a large literature within sociology that points to the power meaning has in shaping human behavior. Niemeyer's work however is very focused on the individual meaning making process, and isn't used to understand generally how people work or trends in this process that
could point us towards a deeper understanding of how suicide suggestion works. Thus this is something the authors can and ideally would contribute.

Re sociological theory, cultural sociology, and specifically the idea of cultural frames, could provide useful insights. Generally, we learn meanings socially through interactions with salient social groups or through cultural ideas available in society. These frames then shape our cultural repertoire that we use to guide our own actions. This understanding of how the individual meaning is socially situated is lacking from this study (this impressive study can't do everything!) but it still could be alluded to in the discussion. I notice that the research team includes people from medical sociology so there is no reason that this part of the study’s contributions couldn't be highlighted at least a little bit more. Unfortunately I have a piece forthcoming, but not yet out, at Sociological Theory, that outlines this theoretical perspective and how it may help us understand suicide and suicide bereavement better. If the more sociological bit feels uncomfortable for a psychiatric journal, at least problematize the notion of referring to the increased risk of suicidality after bereavement as "imitation" and note that this is a really important area for future research. This is one of the main things I find extremely exciting about this study.

Regarding the interesting issue of control, we have added to the Main findings that: (page 20; lines 447 to 468)

‘Exploring whether someone bereaved by suicide feels in control of their own mortality is likely to be a useful means of uncovering their attitudes to their own suicide.’

Regarding adding insights from sociological theory or phenomenology, we found the Neimeyer readings very useful in adding this perspective (as mentioned above) and have used this at the end of the Discussion. (page 19; lines 446 to 451)

Regarding adding insights from cultural sociology, we have added a line to our Strengths and Limitations to state that: (page 22; lines 496 to 498)
“In lacking a sense of respondents’ cultural frames, we were unable to explore how the meaning they ascribed to the suicide loss was influenced by their interactions with their social groups or societal culture.”

Regarding problematic use of the term imitative suicide, we have added to our Discussion (as mentioned above): (page 19; lines 434 to 441)

A small minority of our sample described a diminution of the fear of suicide, having re-evaluated it as a viable future means of escaping (and therefore controlling) threat. Such views corresponded to theoretical constructs of social modelling of suicidal behaviour, although again this was not mentioned explicitly. However, these views also demonstrated that terms such as ‘imitative suicidal behaviour’ convey an over-simplified passive process, rather than the more complex process of discovering meaning in a suicide and how such meaning influences one’s attitudes and behaviour, as detected in our data.

3. Regarding the evidence for social diffusion of suicide (or suicide "contagion") I would encourage the authors to cite research that used longitudinal data and explicitly uses causal modeling strategies such as:


This issue of causality in this area is really central and its time that we move beyond saying that suicide suggestion is solely due to social selection; but in order to do that we need to emphasize studies that explicitly use causal modeling strategies.

Thank you for these references. Reading through them helped revisit the concept of suicide suggestion and separate out the possible mechanisms (social learning, imitation, emotional contagion). This has resulted in a substantial re-edit of the Introduction – as mentioned above.

4. Saying that early adulthood lasts until 40 seems out of sync with the age range typically used in the literature which is generally 18 to (at the latest) 30. It does seem like many of the respondents are younger than 30 and not in the 30-40 range (at least the ones whose quotes are featured). I would suggest clarifying the language used though I don't think it is necessary to exclude individuals from 30-40.

We chose the 18-40 age range to reflect the group of greatest policy interest at the time of designing the study (as outlined in the 2002 Suicide Prevention Strategy for England) and subsequent epidemiological studies showing high rates in middle-aged men (White A, Holmes M: Patterns of mortality across 44 countries among men and women aged 15-44 years. Journal of Men's Health and Gender 3:139-151, 2006). The age range is wider than usual WHO definitions for young adults as up to 34 (Pitman A, Krysinska K, Osborn D, King M. Suicide in young men. The Lancet 2012;379:2383-2392), but we wanted to have a clear margin in order to avoid collecting only the recent experiences of adults at the higher age limit. We have edited the text under Study design to explain that this age range included (rather than defined) young adults (line 124). We also added: (page 5-6; lines 117 to 120)

The 18-40 age range was chosen to reflect the group of greatest policy interest at the time of designing the study.[30] We used a higher upper age limit than that used by the World Health Organisation (WHO) for young adults[27] in order to avoid collecting only the recent experiences of young bereaved adults.
5. I personally would not say that the authors are using an inductive analytic approach to data analysis. I would call this approach an abductive analytic approach following Timmermans and Tavory. It's not a crucial distinction, as some would say this approach fits within Charmaz's grounded theoretical (inductive) approach, but perhaps the authors would find Timmermans and Tavory's methodological approach intriguing:


We discussed this as a team, having read more about the abductive analytic approach as described by Timmermans and Tavory, and together felt that we could describe our approach as inductively-orientated, and have edited the Theoretical approach section of the methods. Thus: (page 8; lines 176-178)

In this study we took a primarily inductive approach, acknowledging an awareness of potentially conflicting theories in relation to the influence of suicide bereavement on attitudes to suicide.

We have also addressed the comment of Reviewer 1 regarding clarifying whether this was an exploratory analysis or one testing a specific hypothesis (see above).

6. I do not think that using a Cohen's Kappa Score should be required for qualitative research, thus I would prefer the authors omit this and instead state that they reviewed the consistency between coders for over 100 responses (which I do think is absolutely necessary and in itself a sufficient robustness check). Quantifying the consistency between coders may work for a study like this with short qualitative data entries, but these scores can be artificially deflated for interview studies or ethnographic data. My concern is that this should not be a generalized expectation for qualitative research.

We agree and have deleted this. We had been unsure whether to have calculated a Kappa score for a study like this, but this comment addresses that uncertainty.
7. On page 19, lines 447-449, the authors undersell their findings as "confirming" the findings of prior research. They could rephrase this to highlight how novel their data and approach is. They go way beyond prior research and clearly articulate this in other areas.

Thank you for this comment. We have edited this sentence as follows: (page 21; lines 479-483)

‘Our findings, within a large and representative dataset, of a deterrent effect, a normalising effect, and a fear of own suicide, are novel and demonstrate the complexity and diversity of attitudinal change after suicide. These findings build on the findings of the above studies, and provide much more detail as to how such attitudes had arisen and had subsequently been enacted in behaviour change.’