Reviewer's report

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

Version: 0 Date: 01 Oct 2017

Reviewer: Anna Mueller

Reviewer's report:

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.


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.

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.

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.

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:


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.

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.
Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

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