Author’s response to reviews

Title: Entrapment as a Mediator of Suicide Crises

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Author’s response to reviews:

Response to reviewers

Thank you both for your very thoughtful & helpful reviews. Our responses follow each specific point, in italics and corresponding changes in the text are highlighted. See the supplementary changes highlighted document.

Thomas Forkmann (Reviewer 1) writes: Thank you for giving me the opportunity to review the manuscript entitled "Entrapment as a mediator of suicide crises". The manuscript is well written, easy to read and with a clear structure. The authors report on an empirical investigation examining whether feelings of entrapment mediate the relation between ruminative-flooding, panic-dissociation, fear of dying, and emotional pain on the one hand with suicide ideation on the other hand. Results suggest that these relations are fully mediated through entrapment except for the relation between emotional pain and SI, which is less clear. The research question is well-derived from current theories in the field and broadly and thoroughly discussed. Nevertheless, I have some concerns with the manuscript that I will describe in chronological order:

Abstract

1) The conclusion of the abstract only deals with the relation between entrapment, emotional pain and SI, but says nothing about fear of dying, ruminative-flooding, and
panic-dissociation. I would appreciate if the authors could revise the conclusion so that it contains statements referring to all constructs investigated.

Thank you for pointing this out; the conclusion section of the abstract has now been revised to include the other subscales of the SCI. See page 2 line 21-22.

Introduction

2) Although (as said above) the introduction refers to much actual research and theories on suicide ideation and behavior, I miss the integration of the recent meta-analysis by Franklin et al. (2017). This meta-analysis sheds some doubt on the credo that the predictive value of certain risk factors is fully established. I would like to see this research integrated to the introduction so that it becomes a bit more balanced.

Thank you for pointing this out. Indeed, while the risk factors in question are fairly well established, the small effect sizes highlight their limited utility as predictors. We now integrate this work accordingly in the introduction. See page 3 line 5-9.

3) Second paragraph: The authors state that the incremental predictive value of the SCI has been shown in a prior study. So, compared to which variables added the SCI incremental variance? It would be nice for the reader if this information could be added.

Thank you for pointing this out. The variables to which the SCI was compared are now described in the introduction. See page 3 line 20-21.

Methods

4) Most importantly, my major concern with this study is that I wonder whether analyses are based on the same data as that reported in this study from the same research group: Galynker I, Yaseen ZS, Cohen AA, Benhamou O, Hawes M, Briggs J. Prediction of suicidal behavior in high risk psychiatric patients using an assessment of acute suicidal state: The suicide crisis inventory. Depress Anxiety. 2017;34(2):147-158. I compared the reported sample descriptive information and it appeared to be identical. Of course, reporting different analyses that base on the same large data set in different publications is common practice. However, I did not find a clear and explicit reference in the present manuscript saying that data comes from the same sample. In my view, the authors should be clear and transparent about this issue.

We thank Dr. Forkmann for highlighting this important point of confusion. The current manuscript indeed represents a novel analysis of the same data reported on in Depression and Anxiety, as we now state unambiguously in the introduction. See page 5 line 19-21.

5) If this is indeed the same sample as in Galynker et al. (2017) some further issues arise: The authors could refer to the prior publication so that not all sample descriptive information would have to be reported again.
We have retained table one as a convenient reference for the reader and trimmed the text description of the sample to its essentials. See page 8 line 12-20.

6) In Galynker et al. (2017) the development and validation of the SCI is reported. In the current manuscript, this instrument is applied to measure the central study variables. However, usually, the development of an instrument and the use of this instrument to investigate research questions about the constructs this instrument measures, should be done in different, independent samples. I would appreciate if the authors could comment on this.

Your point is well-taken and such studies are indeed underway; accordingly, we now highlight this issue among the limitations of the study, and we note that the present report represents an extension of the original validation manuscript. However, there is no intrinsic circularity in examining how the empirical subscales of a measure interrelate in the same data-set which was used to examine how they relate to other measures. See page 14 line 16-18.

7) Since Galynker et al. (2017) report longitudinal data, I wondered whether BSS-data was available for follow-up assessments. This would open the opportunity to calculate mediation analyses based on longitudinal data, which would make a much stronger argument for the role of entrapment in the development of SI and for potential causal pathways.

Unfortunately, BSS was not assessed at follow-up; the lack of longitudinal analysis is noted among the study limitations. See page 14 line 23-24.

8) Measurements: the authors report Cronbach's alpha values from Galynker et al. (2017). Again, either report alpha for the current sample or - if this is identical to that from Galynker et al. (2017) - make this explicit.

As this is the same data set, the alphas are indeed identical; we now highlight this fact with the phrase “as previously reported”. See page 6 line 21.

9) Statistical analysis: I would appreciate if the authors could add the completely standardized effect size in addition to kappa-square when reporting on the indirect effects.

Thank you for the valuable suggestion. We now also report completely standardized effect sizes.

Results

10) A table reporting the intercorrelations between all study variables should be added since this helps in appreciating the results of the mediation analyses.

Thank you. We have added these to the results as suggested. See page 8 line 22-24.
11) Mediation analyses: Different information is reported in the text, tables and figures. This makes it a bit hard to read and I feel that that some information is missing. In my view, all information reported in the text should also be part of the tables. For example, p-values are only reported in text and could be added to the tables. The last column of tables 2 and 3 report on the "indirect effect of X on SI by Normal theory test: [coefficient effect]; (Z score). First of all, why are all these brackets needed? Then, in the methods section, the Sobel test, kappa square and some boot-strapping is described. I think the authors should organize tables (and text) so that it fits to the methods section and name the columns accordingly. This includes adding the CI and kappa square to the tables and reporting on the number of boot straps (at least in a table foot note). Lastly, what does the value in the brackets [] in the last columns refer to? Is this the raw effect?

By contrast to the tables, the figures report the simple regression coefficients only. I would suggest adding this information to a table, too, and further adding the corresponding SE. Furthermore, a mediation model basically is a regression model including a constant. Information on this is missing. To appreciate the overall quality of the regression model in terms of amount of explained variance in the dependent variable, information on corrected R square should be added, too.

Thank you for the excellent advice. Now changes have been made according to all of the above suggestions. We clarify in the methods that 95% CIs were calculated using 5000 bootstraps;

We have eliminated the figures, and present all the values suggested above in tabular form.

12) An idea could also be to control for some "traditional" risk factors of SI, e.g., age, gender, depression. If the prediction of SI by the components of the SCS and the mediation through entrapment emerged even while controlling for these variables, this would - in my view - even corroborate the importance of entrapment as "final common pathway" to SI.

This is an interesting suggestion. Controlling for e.g. depression in the mediation model would give the variance in SI attributable to e.g. the variance common to entrapment and panic that is not shared with depression. While this addresses a somewhat different question from the one originally posed, the independence of our mediators and depression as predictors of SI is a pertinent question. Indeed, entrapment and depression (as assessed by the Beck Depression Inventory (excluding item 9 – suicidal ideation)) are independent predictors of SI and mutually partially mediate their relationships with SI.

Gender was not significantly associated with SI, depression, or any SCI subscale, and thus was not included in the mediation models.

Models with entrapment as a mediator retained significance when controlling for age and depression, with the exception of mediation of emotional pain where the original mediation effect was weaker. We now report these analyses in the methods section and summarize the findings in the results section and present the mediation results in a supplementary data file.
Discussion

13) In the detailed discussion, the authors state that their results suggest that entrapment IS an emotionally painful experience. However, I am not convinced that this is the only possible interpretation. Doesn't the data only show that these constructs are closely related and share more bidirectional (causal) relations than the other constructs?

Thank you for pointing this out. We clarify the bi-directional relationship between entrapment and emotional pain in the discussion. See page 12 line 12-17 & page 13 line 6-8.

14) Further in the discussion (p. 14), the authors state that both active and passive SI have been shown to predict death by suicide (which is true), making an argument for investigating suicide ideation as a "proxy" for suicide behavior. Would it be possible to rerun the analyses separately for active and passive SI? This would be a very interesting analysis, especially as some theories (e.g. Joiner's interpersonal theory) suggest different risk factor profiles for the development of passive and active SI.

Unfortunately, although this is an interesting suggestion, the BSS part 1 does not support an active/passive distinction.

Figures

15) The figures could be improved. For example, the boxes around the numbers could be deleted.

The figures are now replaced by tables.

References

16) Some references appear to be not up to date (e.g. (45) Teismann & Forkmann (2017), (54) Galynker (2017))

Thank you for catching this error. The references have been corrected.

Christopher R. Hagan (Reviewer 2): Thank you for this opportunity to review this manuscript that seeks to clarify the role of entrapment in suicide, specifically through the lens of suicide crisis syndrome (SCS). The authors tested the hypothesis that entrapment mediates the relationship of components of SCS (i.e. ruminative flooding, panic-dissociation, fear of dying, and emotional pain) and suicidal ideation (SI). They tested this in a relevant population of recently hospitalized psychiatric inpatients. This manuscript was well written, interesting, and used a highly relevant and informative sample of higher risk individuals. Despite these strengths, I have several concerns and questions that I would like to see addressed prior to publication.
Primary Concerns

1) The introduction would benefit from an additional paragraph early in the manuscript clearly explaining SCS. The first paragraph makes it appear that SCS just states that having suicidal thoughts and being very upset leads to attempts.

Thank you for drawing attention to the need for clarification here. We now describe the proposed SCS in fuller detail in the introduction. See page 3, line 11-17.

2) Please remove all instances of "committing suicide" and "completed suicide" from the manuscript and use terms such as "died by suicide," "seeks to escape through suicide," etc., and just "suicide," rather than "completed suicide."

Thank you for pointing this out. Now "committing suicide" and "completed suicide” have been replaced by “suicide”.

3) The statements at the end of the introduction that SI is one of the strongest predictors of death by suicide are incorrect. The citations used to support it are 17 and 12 years old. David Klonsky and Jessica Ribiero (and their coauthors) have published several papers in the past several years showing that suicidal thoughts are not great predictors of death by suicide and that risk factors identifying suicidal ideators are largely different from those that identify those who will attempt and die by suicide. While SI is correlated with suicide attempts and is an important topic to study, I believe the second to last paragraph of the intro overstates the ability of studying suicidal ideation to measure "suicide risk." Ideation is not a proxy for, but rather one aspect of risk for death by suicide or serious suicide attempt. This same comment also applies to the last paragraph of the discussion.

We appreciate your point here, which is very well taken, and have revised the introduction and discussion accordingly. See page 5 line 11-16 & page 14 line 20-23.

4) I would like to see the statistical analyses computed with the bootstrapping method rather than the more conservative Sobel test. Given that Sobel is more conservative, I doubt that this would make any of the findings non-significant. The same PROCESS macro can be used for these analyses. David A Kenny's website (http://davidakenny.net/cm/mediate.htm) includes some good basic information on this, and Hayes' book (Citation 43) also goes into detail about this method.

Thank you for this suggestion; we agree and have revised the methods accordingly. See page 7 line 14-20.

5a) The discussion does a good job of relating these findings to multiple theories of suicide. I am most interested though in how these findings relate to Klonsky & May's newer Three Step Theory (3ST) of suicide. The 3ST focuses on the interaction of pain and hopelessness in predicting suicidal ideation that in conjunction with capability for suicide leads to suicide and suicide attempts. These results seem like they may provide
support for that theory if entrapment (which is somewhat connected to feeling hopeless, although certainly not the same) and emotional pain are coming out as the two factors most directly tied to suicidal ideation.

Thank you for this excellent observation which we have accordingly integrated in the discussion. See page 13, line 5-8.

5b) I am also interested in a more explicit discussion of what these findings mean for the validity or utility of SCS if most of the factors are mediated by entrapment.

This is an interesting and difficult question. As the SCS is proposed to be a syndrome the strong inter-correlations among the SCI subscales are certainly supportive of the SCS concept. Whether such a syndrome is more predictive of STB than simple severity of entrapment is beyond the scope of this study, however.

6) From my understanding of the methods section, the SCI and BSS were administered up to 72 hours apart from each other. If they were administered at the same time, this point is moot and that should just be clarified in the methods section. If the BSS was administered up to 72 hours prior to the SCI, this is a major limitation of the study that should be explicitly noted. Suicidal crises often quickly abate, resulting in substantially lower suicidal ideation and BSS scores within hours to a day of the peak of the suicidal crisis, which often occurs prior to arrival at the hospital. Of course, the problems leading to the crisis, in part captured by the SCI do not abate as quickly in most cases. Are there any data available to indicate if SI decreased from the time of BSS administration until SCI administration? If not, the concern that SI had decreased between administration of the two measures should be addressed.

Indeed, the measures were administered concurrently. See page 6 line 9-11.

7) Are there any data to assess if these results differ between those admitted for suicidal ideation and those who actually attempted suicide?

We first did t-test to compare SI scores from patients admitted for suicidal ideation (n=142) and those from people who actually admitted for attempted suicide (n=58), there was no difference. We then did mediation analysis for each group and indirect effects were similar for those two groups. For example, rumination towards SI mediated by entrapment: $\kappa^2=0.20$ (significant, CI did not include 0) for attempted suicide group vs $\kappa^2=0.20$ (significant, CI did not include 0) for suicidal ideation only group. We now report this in brief. See methods, page 8 line 3-5 & Results, page 10 line 22-23.

Secondary Concerns

1) I am unclear about what "material factors" means in the 1st paragraph of the introduction.

We have clarified as follows: “….material factors such as access to means.”
2) Please clarify the number of items on the SCI. Initially, it is listed as having 49 items, but the breakdown by subscale only sums to 36. Also, the in text citation in the SCI paragraph needs to be converted to a number, consistent with other citations.

We have clarified. See page 6 line 19-20.

3) Please include the percentages of Latino and Asian participants.

Please see response to comment 5 from Dr. Forkmann. Values are reported in table 1.

4) Please include a correlation table of the primary variables. I would like to see the univariate relationships between SCS variables, entrapment, and SI.

Please see response to comment 10 from Dr. Forkmann. The correlation table has now been added.

5) On the top of page 11, "Our data also supports" should be "Our data also support."

Thank you for catching this error which we have now corrected.

6) Citation 52 has some odd underlining at the end of the citation (this may just be the formatting of the document I received also)

7) Unless otherwise specified by the editor or journal, the tables should be formatted according to APA standards.

8) Tables 2 & 3 are missing a definition of "SCS"

We have corrected each of the above.

9) The Figures are missing a definition of "SI" (SI is also a common abbreviation for self-injury which is sometimes is used including suicide attempts, so precision here is important to prevent any misinterpretations of the data).

10) Figures 2-5 would benefit from being made to look more like publication ready figures (e.g. remove boxes around a, b, & c’, make all lines solid black, center words within boxes, uniformly connect ends of arrows to lines of boxes, etc.)

We have replaced the figures with tables in response to these and the first reviewer Dr. Forkmann’s comments.