Reviewer’s report

Title: Entrapment as a Mediator of Suicide Crises

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Reviewer: Thomas Forkmann

Reviewer’s report:

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

- 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.

Introduction

- 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.

- 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.

Methods

- 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. If this is indeed the same sample as in Galynker et al. (2017) some further issues arise: (1) The authors could refer to the prior publication so that not all sample descriptive information would have to be reported again. (2) 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. (3) 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.

- 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.

- 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.

Results

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

- 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 bootstrapping 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 bootstraps (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. 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.

Discussion

- 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?

- 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.

Figures

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

References

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

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

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Unable to assess

Are the conclusions drawn adequately supported by the data shown?
If not, please explain in your comments to the authors.

Yes
Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
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I am able to assess the statistics

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