Author's response to reviews

Title: Does parenting style matter? - Predictors of suicide attempts in a representative sample of adolescents

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Author's response to reviews: see over
Dear Editors,

Please find as follows our answers to the remarks of the reviewers which we found truly helpful.

The abstract is formatted into Background, Methods, Results and Conclusions and has 349 words.

The manuscript has been read and was edited by a professional native US-American language editor.

Answer to the reviewers:
Reviewer 1:

1. Since it has been shown, that the way an assessment is carried out can hugely influence the outcome, I would urge the author to include more details about the study procedure.
   How was the study announced to the students? Was the study announced as study on suicidality? Who informed the students? Were they only informed per mail, or was there an announcement in the classroom. Was there an incentive for participating in the study?

   The study was announced by a letter sent to the parents and to the students from the KfN. The study was not announced as a study on suicidality, since it was in reality a study with broad interest. The official announcement was “concerning different problems in the youth”. The teachers in the classroom who delivered the questionnaires referred to the information letter. There was no incentive to take part other than that two school lessons were cancelled for the time when the questionnaire was filled out.

   This information is included in the methods section.

2. I find the classification of countries somewhat confusing (like for example: where would Bosnia end up, which is both a former Yugoslavian state but the culture is essentially influenced by Islamic theology with 44% muslims representing the largest religion group?) What was the rationale for choosing this classification?

   The classification that “Islamic imprinted countries” are analyzed as a separate group came into existence because of our already undertaken analyses concerning other risky behaviors for example in the substance consumption field. We observed that adolescents with roots in those countries behaved obviously different; while there was no big difference between adolescents with migration background from different countries with a rather “western” culture. Concerning Bosnia there was no further differentiation possible since the item in the questionnaire was “former Yugoslavia/Albania”. Therefore we do not know, if they were from Bosnia, Serbia or another cultural group living in this country. But true, this is an issue where in consequence the categories are not absolutely strict separated.
A sentence explaining the rationale of the classification of migration background was included in the methods section.

3. Number of non-profit volunteer activities is described as predictor of suicide attempts, but not discussed in the paper, although it’s mentioned as a possible option for intervention earlier in the paper. These finding seems counterintuitive. Can the authors discuss this finding?

The result concerning non-profit volunteer activities is discussed now.

4. The recently publishes work by Brent et al. (2013) in the Journal of the American Academy of Child and Adolescent Psychiatry 52(12):1260-71. doi: 10.1016/j.jaac.2013.09.009, presents an overview about interventions on adolescent suicidality, showing that the involvement of family is an important point. The findings underline the authors’ conclusion and might be a good contribution.

Thanks for the suggestion. Brent et al. is cited now.

5. Furthermore the findings of the authors are in line with research from a longitudinal study by Wichstrom et al. (2009) (Arch Suicide Res 13: 105-122), who showed, that attachment to parents was a protective factor for adolescent

We also appreciate this remark and included Wichstrom in our discussion.

6. We included in the limitation section, that we used a relatively new measurement for suicidality.

Reviewer 2:

Some suggestions in order to make the manuscript shorter:
- Methods: It should be taken into account that methods section comprises more than 12 word pages!!!! I really think that can be summarized.

The methods section was shortened. Details from the “Design” and “Instrument” parts were excluded. Some details were also omitted from the “Statistical Analysis” part. The Methods section is 2 manuscript pages shorter now.

- Results: some results (odds ratios) are repeated in both the text and tables. Please choose one.

The ORs were excluded from the text. That improves readability.

Is it table three really necessary? The full model is the most important result.

Table 3 was omitted. One important additional information from Table 3 was included in the results section (p-value change).
-Discussion: Your main aim was to identify the role that parenting styles plays in adolescent suicidal behaviour. However, in discussion section this is not deeply discussed. I suggest you to focus on this issue and the most interesting associations. Some of the predictors you find are well-known risk factors, so that I suggest not going into detail about them.

To the discussion of parenting styles two further points were added including literature reference. Discussion of the other variables was shortened especially concerning the variables that did not meet the criteria for clinical relevance. However, as the first reviewer suggested, discussion of volunteer activities has been included now. For the reasons of logic, no significant predictor that was at least on the threshold to clinical relevance was left undiscussed. “Significant only” predictors were not discussed.

-Limitations: you have addressed suicide attempt by a single question. No information about number or severity of suicide attempts is provided.

Unfortunately we do not have this information. The point was added to the limitations section.

Finally, I have a question about questionnaires.
-Information about diagnosis of ADHD is given in your study. I wonder whether some other questions regarding other psychiatric disorders are included in the questionnaire. It has been extensively reported the association of depression with suicide attempts.

Have youngsters been questioned about having ever been diagnosed by a depressive disorder or other relevant psychiatric disorder (psychosis, bipolar disorder)?

I wonder whether the inclusion of depression as an independent variable would change the associations reported.

Yes we have this item (only for depression – “Have you ever been diagnosed with depression”). It was, as you suggested also, my first impulse to include it in the analysis. The literature base for that is very good. But unfortunately the data quality of this item was insufficient. There were so many missings, that I could not impute the missing values because they were for sure not missing at random. The validity of the answers of this item are therefore more than questionable. Thus I didn’t include it.