Author's response to reviews

Title: Does parental monitoring moderate the relationship between bullying and adolescent nonsuicidal self-injury and suicidal behavior? A community-based self-report study of adolescents in Germany

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

Enclosed please find our revised manuscript “Does parental monitoring moderate the relationship between bullying and adolescent non-suicidal self-injury and suicidal behavior? A community-based self-report study of adolescents in Germany” (MS: 3291151351529560). The point-by-point response to the referees’ comments and suggestions can be found below. We hope that the manuscript will now meet the criteria for publication in BMC Public Health.

Yours sincerely,

Vanessa Jantzer

Comments reviewer 1:

1) The authors find that social bullying may be a triggering factor for both NSSI and SB, whereas cyberbullying is more relevant to NSSI. However, the manuscript does not examine the correlations between the different types of bullying and whether children were exposed to more than one form of bullying. Children exposed to bullying are probably more likely to be exposed to more than one type and previous studies show that victims of multiple types of bullying are more troubled than those exposed to one type. The authors emphasize the frequency of bullying when they can address both frequency and exposure to multiple types of bullying.

We agree with the reviewer and the correlations between the different types of bullying were examined and are now reported in the manuscript (p. 9, 115-17). Additionally, the correlations of the estimates were inspected to rule out problems with collinearity. The range of the correlations is now reported along the estimates (table 3+4). The reviewer also proposed to investigate whether victims of multiple bullying are more troubled than those exposed to only one type of bullying. We found the idea very interesting and created a new variable “number of types of bullying” (ranging from 0 to 4) bridging in the
regression model as a covariate. Interestingly, this new variable correlated with the given definition of bullying by frequency with .94, which led to a huge problem of multicollinearity in the regression model. This meant, including both variables in the model was not possible and adding "number of types of bullying" as a covariate did not lead to any further explanation of variance. So we needed to decide between our definition by frequency and the proposed new one. Because the given definition is also used by Olweus himself, who created the applied questionnaire and is more common in the international literature, we preferred to keep the definition in its initial form.

2) It is not clear whether the regression analyses control for any covariates. Age and [or] grade are covariates that should be included given that bullying frequency was found to change between grades. In addition, the manuscript is not clear on the sampling strategy and how observations are correlated and if multiple subjects are drawn from the same schools and grades. It is not clear whether these correlations are taken into account. As proposed, we included grade as a covariate. The reported results of the regressions were corrected accordingly (p. 10 l. 16-25). Reviewer 1 correctly points out that observations may be correlated because multiple subjects were drawn from the same schools and grades. Unfortunately, we are not able to take these correlations into account (e.g., via multi-level regressions) because we assessed only grade level type of school which means that reliable information about individual grades/classes and schools is missing. Therefore, a possible effect of grade and school cannot be tested. This fact was added to the limitations section (p. 15 l. 15-19).

3) It is also not clear whether authors have collected data about parental history of psychopathology. Lack of parental monitoring could be due to parental psychopathology, which puts children at increased risk for exposure to bullying, NSSI, and SB. We agree with reviewer 1 that data about parental psychopathology would be very interesting and could be a reason for low parental monitoring. Unfortunately, data about parental history of psychopathology was not collected during the study. However, we included this idea as a section for future research in the discussion of our paper (p. 14 l. 13).

4) In the manuscript, the authors refer to the "incidence" of bullying when they are studying prevalence or 12-month prevalence. Prevalence and incidence should not be used interchangeably. The term "prevalence" is now correctly used in the revised manuscript.

5) Figure 1 does not directly convey the results. The authors should consider presenting the data in the form of a histogram for the relationship between bullying and NSSI and SB stratified by parental monitoring (as a categorical variable based on a cut-off). For presenting the data in the proposed form, a categorical variable for parental monitoring that is based on a valid cut-off would be needed. Because the variable was created for use as a dimensional scale, such
categories and cutoffs could not be adequately justified. Additionally, information and power would be reduced by changing the dimensional construct to a categorical variable. For these reasons, we preferred to keep the figure in its initial form and we hope that our explanation may be acceptable to the reviewer.

6) Tables should include the total Ns in each category. Table 2 now includes the total Ns in each category.

Comments reviewer 2:

1) The biggest limitation to the study is that it cannot make statements about causality given the cross-sectional nature of the data. This would appear to be a major research priority in this field of study and addressed should additional waves of the data set become available. The authors are appropriately cautious about this issue in the limitations section, but it’s such a critical issue that it merits mention upfront as well. For example, I was wondering about the research designs of studies mentioned on p. 4 that show that bullying is a factor in the development of severe emotional and behavioral problems. Is there such a way to suggest that the studies were able to rule out effectively the possibility of endogeneity—that those with severe emotional problems are more likely to be bullied.

As proposed, we emphasized in the background that statements about causality cannot be made and refer to the limitations (p. 14 l. 20 – p. 15 l. 2), where this topic was also enhanced (p. 14 l. 20 – p. 15 l. 2).

2) It would be useful to include a clear definition of NSSI early on in the paper, along with some examples. It’s assumed this would include cutting, among other behaviors?

A clear definition of NSSI early on in the paper, along with some examples, was included (p. 5 l. 3-7).

3) The authors suggest that a major contribution of the present study is to examine relationships by type of bullying. It would be useful to include a paragraph in the background developing some theoretical arguments for why we might expect the relationships to differ on this basis. Furthermore, I don’t believe the authors examined whether the possible buffering effect of parental monitoring might differ by type of bullying. Is this because of small sample sizes?

We included a paragraph in the background developing some theoretical arguments for why certain types of bullying may be particularly linked to an increased risk for certain disorders (p. 4 l. 7-11). As proposed, we also tried to examine whether the possible buffering effect of parental monitoring might differ by type of bullying. Ordered logistic regressions using parental monitoring as a covariate could not reveal significant differences between the types of bullying. But we agree with the presumption of the reviewer; we also feel that these non-significant results may not be interpretable due to the small sample sizes.
that may increase the risk of beta-error. Thus, these sub-analyses were not included in the paper.

4) Can the authors comment on the accuracy of self-reports of bullying by children? Do studies reveal that they tend to be accurate and is it possible to include other more objective measures? It is mentioned as a limitation, but I would like more detail. Can the authors add a sentence or two describing how realism may differ when both subjective and objective measures are used?

A comment on the accuracy of self-reports of bullying by children and the possibility of using other more objective measures was included (p. 15 l. 6-13).

5) Is it possible to include other controls in the models? At a minimum, it would seem important to explore whether the associations differ by gender and age.

The authors test for gender and grade differences in the univariate distributions but do not address the issue in the regression models. Possible differences by gender should be theorized and explored, particularly since prior work has found gender differences in some of these relationships.

Interactions effects of bullying with gender and bullying with grade were tested in the regression models for both NSSI and SB but revealed no significant effects. Therefore, we mentioned testing of interactions in the manuscript, but decided not to report the details (p. 10 l. 25 – p. 11 l. 11).