Author’s response to reviews

Title: Emotion regulation and its relation to symptoms of anxiety and depression in children aged 8-12 years. Does parental gender play a differentiating role?

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Author’s response to reviews:

Dear Editor

BMC Psychology

We hereby resubmit our manuscript “Emotion regulation and its relation to symptoms of anxiety and depression in children aged 8-12 years: Does parental gender play a differentiating role?” We appreciate the comments from the editor and reviewer, they were most helpful in our effort to revise the manuscript. We believe that the manuscript is greatly improved as a result.

We hope that we have managed to address all of the important concerns raised. Each comment is answered point by point in the following response letter, including page number and line indicating where to find the changes in the revised manuscript. All changes in the manuscript are highlighted by track-changes.

The manuscript has been approved by all the authors.

The manuscript has not been submitted or published elsewhere.
On behalf of all the authors,

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Responses to the reviewers

REVIEWER 1

Response to comment 1 A) Further coverage would be helpful, however on the validity of parent and child reports of psychopathology, to provide a stronger context for the interpretation of the results that differ between parent and child reports.

Thank you for your comment. We have added a paragraph at page 5, line 103 to 108 in the reviewed manuscript, describing reporter differences between parent and child. “Informant difference between child and parent is common, and in studies on anxious and depressive symptoms moderate discrepancies are typically reported [23]. Parental reports of children’s internalizing symptoms are considered valid [24]. Informant differences have traditionally been viewed as measurement error, but resent research have pointed to this instead being a reflection of different perspectives and relationships, and providing clinically meaningful information [25].“

1. B) This discussion should also induce the issue of common method variance in the context of identifying correlates of psychopathology.

We agree with the reviewer that deriving data from a single source is a significant limitation, as this could introduce common method variance, which could potentially affect associations. We have emphasized this concern under “Strengths and limitations” and recognize that similar concerns are raised by the reviewer in question 11.

Hopefully, we have addressed this concern adequately on page 17, line 396 in the revised manuscript where the following paragraph is included: “Not having multiple informants allows the possibility that shared method variance could affect our results [54]. The relationship between emotion regulation and anxious symptoms was not statistical significant when children self-reported on anxious symptoms. As a result, we cannot rule out that the association found for
parental reports of anxious symptoms and emotion regulation was inflated by shared method variance. However, the relationship between emotion regulation and depressive symptoms was evident using only parental report for both measurements, and when children’s self-report of depressive symptoms was used as the dependent variable. Although the effect diminished when different reporters were used, this may indicate that the relationship are not merely a result of measurement bias.”

Response to comment 2. The first two paragraphs should be included under the sub-heading of Emotion regulation, anxiety and depression, ideally amalgamated into one paragraph.
The proposed change are included in the manuscript at page 3, line 48 and line 52.

Response to comment 3. p<.001 not p<.000
We appreciate that the reviewer noticed this, and we have made the change in the manuscript, see p. 9, line 195.

Response to comment 4. EASQ please comment on the reliability of this measure or explain why you believe reliability is not relevant to this scale.
EASQ is a questionnaire describing negative life events and chronic stressors that the children potentially have experienced during the last 12 months, answered by yes or no. As the experiences of changing schools are unassociated with experiencing someone close to you dying, including reliability calculations for this scale are uninformative. The purpose of EASQ is to access the cumulative load from different stressors. To clarify this we added the following sentence in the revised manuscript, see p. 11, line 240: “The EASQ measures the cumulative load of unrelated stressors that the child have experienced, therefore reliability scores are uninformative.”

Response to comment 5. P10 Line 35. I am not clear how the univariate regressions provide different information from the correlation of the outcome and predictor as included in the correlation results.
The univariate regression provide no additional information to the manuscript when the correlation table are included. We appreciate this important notice from the reviewer, and have removed the phrase regarding this from the manuscript, see p. 11, line 248. Deleted sentence: “Initially, we performed six independent univariate regressions, with anxious or depressive
symptoms reported by the child, mother or father as the dependent variable, and emotional regulation reported by the mother or father as the only independent variable, all of which showed significant results.”

Response to comment 6. Please explain the Paternoster test in more detail.

We included the following text to the manuscript in order to give a more detailed description of the Paternoster test, see p. 12, line 265; “The Paternoster test is used to test if an empirical relationship estimated in two independent samples are similar, by comparing the unstandardized regressions coefficients from the two independent regressions”.

Response to comment 7. P11 line 38, 'surely it should be p>.05 for the correlation coefficient estimated at .00?

We thank the reviewer for noticing this, and have made the change in the revised manuscript, see p. 12, line 276.

Response to comment 8. P13 Line 36 the suggestion that emotion regulation strategies are a target for intervention needs to be more explicitly tempered with the consideration of the cross-sectional data limitations that are detailed in the paragraph below.

We see that the reviewer promotes a valid critique here, and as a consequence we have revised the sentence as follows, see p. 14, line 318: “Our findings indicated that a lack of positive strategies to regulate emotions, as well as the presence of negative emotion regulation strategies, were associated with anxious and depressive symptoms. Such regulation strategies should therefore be explored in longitudinal studies as potential targets for intervention”.

Response to comment 9. P14 para 2. The limitation of the current sample relative to a clinical sample is discussed. I would also be interested in hearing the authors' thoughts on how well they think the results from the current sample would generalize to a fully random general population sample.

Thank you for your important comment. As our sample is limited to children reporting increased anxious and/or depressive symptoms, our results are not generalizable to the general population. This is now included in the manuscript under “strengths and limitations”, see p. 17, line 388. “However as children in our study were recruited on the basis of their self-reported elevated anxious and/or depressive symptoms, the results are not generalizable to the general population”.

Response to comment 10. P14 Line 50. The issue of whether child or parent provides the most accurate report of psychopathology does not seem to me to be the main concern. For me the most important question is whether each provides clinically useful information. My reading of the literature including the work cited here, is that they do. So this might be a useful point to make here.

We agree with the reviewer that the main concern is whether each informant provides clinically useful information, and we have tried to clarify our position in the revised manuscript, see p. 15, line 351: “Both child and parental reporters provide clinically meaningful information, enlightening a phenomenon from different angles [25].”

Response to comment 11. A) P14 Line 55/57 I don't quite understand how the pattern of results support validity. I would like the authors to re-write this for clarity.

We thank the reviewer for drawing this ambiguity to our attention, to reduce imprecision we removed the sentence in question from the revised manuscript, see p. 16, line 354. Deleted sentence: “However, as the association is found for both anxious and depressive symptoms when parental reports are used, we assume that the associations found in the present study are valid”.

Response to comment 11 B) The issues of common method variance needs to be discussed, as relationships between constructs from the same reporter seem to be inflated relative to associations between constructs across different raters.

We agree with the reviewer that deriving data from a single source is a significant limitation, as this could introduce common method variance, which could potentially affect associations. We also recognize that the same concern is raised by the reviewer under comment 1 B. We have emphasized this concern under “Strengths and limitations”, see p. 17, line 396 in the revised manuscript, where the following paragraph is included: “Not having multiple informants allows for the possibility that shared method variance could affect our results [54]. The relationship between emotion regulation and anxious symptoms was not statistically significant when children self-reported on anxious symptoms. As a result, we cannot rule out the possibility that the association found for parental reports of anxious symptoms and emotion regulation was inflated by shared method variance. However, the relationship between emotion regulation and depressive symptoms was evident using only parental report for both measurements, and when children’s self-report of depressive symptoms was used as the dependent variable. Although the effect diminished when different reporters were used, this may indicate that the relationship is not merely a result of measurement bias.”
Response to comment 12. P15 Line 5. I don't agree that the finding that there being no difference in the strength of links between mother and father reports in prediction strength indicates that there is no value in taking ratings from both. This point would only be tested in a model where both mother and father reports were included as simultaneous predictors.

We thank the reviewer for this important consideration. As this is not statistically tested, this interpretation is removed from the manuscript and replaced by the following formulation, see p. 16, line 357 “This might indicate that there is no difference between parental reports regarding this association. Another potential explanation is that our sample size of fathers was too small to detect differences.”

REVIEWER 2.

Response to comment 1. It's a little confusing that the title includes the phrase "as reported by caregivers" even though these reports are available from children as well and included in the study.

We agree with the reviewer that this phrase in the title are confusing, and the title is now changed to: “Emotion regulation and its relation to symptoms of anxiety and depression in children aged 8-12 years. Does parental gender play a differentiating role?” See page 1, line 3.

Response to comment 2. It wasn't entirely clear to me why the association between emotion regulation and anxiety/depression should differ by parental gender. The authors review differences in ratings of the level of emotion regulation and anxiety/depression by mothers/fathers, but I don't think it automatically follows from this that associations between these constructs would differ, too. Could the authors elaborate their argument a little more here?

In the background section of our article we emphasizes that there are differences between maternal and paternal reports, and that such knowledge may have the potential to broaden our understanding of the association between children’s internalizing symptoms and emotion regulation. However, whether the association between internalizing symptoms and emotion regulation may differ by parental gender have not been assessed in previous research, and will therefore be investigated in the present study. To reduce ambiguity we have made the following changes in our hypothesis on page 7, line 156; “We further examine whether the association between internalizing symptoms and emotion regulation may differ depending on the reporter being mother or father.”
Response to comment 3. Please clarify how the children were recruited. Schools are mentioned, but it wasn't entirely clear to me whether recruitment happened there and what the procedure was. I think this needs to be in the paper, rather than referred to in another publication.

Thank you for your important comment. Description of the recruitment process should have been included in more detail in the manuscript. We have now added a description of the recruitment procedure under the heading “procedure”, see p. 8, line 167.

Response to comment 4. I think it needs to be clearly mentioned in the limitations that this is a sample that is highly selected in several ways:

a) it includes children with elevated levels of anxiety and depression (the authors mention this)

The following phrase are included under “Strengths and limitations” in the revised manuscript, see p. 17, line 388. “However as children in our study was recruited on the basis of their self-reported elevated anxious and/or depressive symptoms, the results are not generalizable to the general population.”

b) parental response rates were relatively low.

We recognize that response rates are an important area of consideration, and that there is always an issue of whether we manage to recruit the lowest scoring individuals.

The parental response rate are 78.5 %. Generally, a response rate above 70 % are considered good, as noticed by Babbie 1990 [1] and more recently by Nestor and Schutt 2012 [2]. Based on this, parental response rate are not included in the limitations. Nevertheless, we do agree with the reviewer in that striving for higher response rates are important in future research, especially so for including fathers.

c) response appears to be skewed toward affluent parents, particularly well-educated mothers.

The percentage of well-educated mothers in our sample are mentioned on page 9 line 204, and compared to the education level in general population in Norway. In the revised manuscript, this is also included in the following phrase under “Strengths and limitations, see p. 17, line 390: “Furthermore, the sample is skewed toward well-educated parents, especially for mothers, indicating that our sample are skewed towards higher SES. As low SES has been associated with
increased risk for psychopathology in children [31], the skewness in our sample possibly reduce generalization of our results further.”

Response to comment 5. Has the ERC been validated in Norwegian (or any non-American) samples?

The ERC is not validated in Norwegian sample, but the questionnaire is validated in other non-American samples. The following phrase are added in the revised manuscript, see p. 10, line 220: “The questionnaire was previously validated in European samples [41] in addition to the original American validation, but the ERC has not been validated in a Norwegian sample.”

Response to comment 6 A). The fact that associations are much reduced or disappear once dependent and independent variables are reported on by different informants (i.e. the model where parental reports predict child reports) makes me think that shared method variance could perhaps explain some portion of the positive findings. Could the authors discuss this possibility in a bit more detail?

We agree with the reviewer that deriving data from a single source is a significant limitation, as this could introduce common method variance, which could potentially affect associations. We also recognize that both reviewers raise this issue. We have emphasized this concern under “Strengths and limitations”, in the revised manuscript, where the following paragraph is included, see p. 17, line 396: “Not having multiple informants allows for the possibility that shared method variance could affect our results [54]. The relationship between emotion regulation and anxious symptoms was not statistically significant when children self-reported on anxious symptoms. As a result, we cannot rule out the possibility that the association found for parental reports of anxious symptoms and emotion regulation was inflated by shared method variance. However, the relationship between emotion regulation and depressive symptoms was evident using only parental report for both measurements, and when children’s self-report of depressive symptoms was used as the dependent variable. Although the effect diminished when different reporters were used, this may indicate that the relationship is not merely a result of measurement bias.”

Response to comment 6 B). As part of testing for this possibility, I wondered what the results were when mothers' ratings of emotion regulation predict fathers' ratings of childhood depression/anxiety (or vice versa)?
We thank the reviewer for raising this interesting question, although this is beyond the scope of this article and we have not tested this. We hope our extended discussion of shared method variance described under comment 6A are satisfying for the question.

Response to comment 7. The authors refer to "unstandardized betas", but betas refer to standardized estimates in a regression. Do the authors mean b's? Please clarify.

We thank the reviewer for noticing this ambiguity; the sentence are revised as follows: “The Paternoster test was used to compare the unstandardized regression coefficients (b1) between regressions containing parental reports”. See page 13, line 288 and 301.

Response to comment 8. Although it is a strength, I think it should be mentioned that is also a limitation that the study selected participants with elevated levels of anxiety/depression; we don't know what the associations look like in children who (so far) seem symptom-free, which makes it difficult to conclude whether emotion regulation would be a worthwhile target for prevention efforts.

We agree with the important comment that sampling children with elevated symptoms does reduce generalization to the general population, and have included following comment at page 17, line 388; “However as children in our study were recruited on the basis of their self-reported elevated anxious and/or depressive symptoms, the results are not generalizable to the general population”.

Response to comment 9. The authors write "One potential interpretation of this is that no extra information is gained by including both parents.". I don't think this conclusion is entirely justified. The test would require examining whether fathers' report of emotion regulation statistically adds to the prediction of mother's reported anxiety and depression and vice versa. I don't think the authors have tested this? It would be interesting to see the results of this test.

We agree on this point, and thank the reviewer for noticing this. As this is not statistically tested, this interpretation is removed from the revised manuscript and replaced by the following formulation, see p. 16, line 357: “This might indicate there is no difference between parental reports regarding this association. Another potential explanation is that our sample size of fathers was too small to detect differences“.
Additional changes:

For the sake of clarity, we also made the following minor changes in the manuscript:

Page 2, line 29. The sentence is altered into: “The present study is part of the Coping Kids study in Norway, a randomized controlled study of a new indicated preventive intervention for children, EMOTION”.

Page 2, line 36 and 38. We have changed the word depression to depressive symptoms.

Page 2, line 41. We have altered the sentence into: “Problems with emotion regulation probably coexists with elevated levels of internalizing symptoms in children”.

Page 4, line 84. We altered the sentence into: “This result is in line with a cross-section study by Zeman and colleges [16] indicating associations between internalizing symptoms and poor emotion regulation”.

Page 6, line 118. We altered the sentence into: “Is it necessary to include both parents in research regarding emotion regulation? In order to answer this question, we must compare maternal and paternal reports of child emotion regulation”.

Page 6, line 136. We altered the sentence into: “Sociodemographic factors (SES), such as parental education and the family economy, also influence children’s mental health [31] and possibly the association between internalizing symptoms and emotion regulation [32]”.

Page 7, line 148. We altered the sentence into: “Both mothers and fathers reported on their child’s emotion regulation capacities, and we further investigated whether parental gender has a differentiating role”.

Page 7, line 151. We altered the sentence into: “To our knowledge, these questions have not previously been investigated in a Norwegian child population with emotional problems, and very few relevant studies have been conducted worldwide”.

Page 8, line 163. We altered the sentences into: “The present study uses baseline data from the Coping Kids study in Norwegian schools. Coping Kids is a national cluster randomized controlled study of an indicated group-based cognitive behavioral therapy (CBT) intervention, EMOTION, for children between the ages of 8 and 12 with elevated anxiety and depressive symptoms”.

Page 8, line 176. We corrected the total number of children how underwent screening for symptoms of anxiety and depression to 1686.

Page 8, line 177. We altered the sentence into: “…, and 873 children were invited to participate in an intervention study…. ”.
Page 14, line 312. We altered the sentence as follows: “These results were retained even after controlling for known risk factors such as parental mental health, SES, stress the preceding year, and the child’s age and gender”.

Page 19, line 435. Regarding competing interests, we have included the following: ”KM receives royalties from sales of the Emotion intervention in Norway. The remaining seven authors declare that they have no competing interest with publishing this article”.

References:
