Reviewer’s report

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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Reviewer: Jazmin Wertz

Reviewer's report:

This study examines the association between emotion regulation and symptoms of anxiety and depression in 8-12 year old Norwegian children. The authors find clear associations when using parent reports of child emotion regulation to predict children's anxiety/depression; less so when using children's self-reports of anxiety/depression as the outcome variable. Overall this is a very interesting study. I do have a few comments:

- 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.

- 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?

- 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.

- I think it needs to be clearly mentioned in the limitations that this is a sample that is highly selected in several ways: 1) it includes children with elevated levels of anxiety and depression (the authors mention this) 2) parental response rates were relatively low 3) response appears to be skewed toward affluent parents, particularly well-educated mothers.

- Has the ERC been validated in Norwegian (or any non-American) samples?

- 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? 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)?

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

- 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.

- 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.

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

Yes

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

Yes

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

No

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