Reviewer’s report

Title: Coping, Adaptation, and Perceived Risk to Children: A Survey of Parents Affected with Bipolar Disorder

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Reviewer: Sarah Doucette

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The manuscript titled: Coping, Adaptation, and Perceived Risk to Children: A Survey of Parents Affected with Bipolar Disorder examined associations between coping, adaptation and perceived risk of illness to children in a sample of parents self-identified as having bipolar disorder. Although the qualitative data in the study are interesting, as well as the findings on different predictors of adaptation, as this is part of an understudied area, the rational, research question and flow of this manuscript are in need of major revision.

Major Compulsory Revisions

(1) The question posed by the authors is not clear. The only hypothesis stated is at the end of the introduction, “greater adaptation would be correlated with lower perceived risk to children”. Is this the studies primary aim? This implies that adaptation is the predictor; however, this is listed under outcomes in the methods. The authors should clearly articulate in numerical form what their primary aims are and then address those aims in a systematic way throughout the manuscript. If the latter is the primary aim, the rationale for this is not sufficiently explained in the introduction. One qualitative study is described indicating that parent’s degree of concern for their children was related to their own subjective perception of their own well being. How was parent’s degree of concern measured in this study? Parent degree of concern is not the same as parent’s perception of genetic transmission to their children. The rationale for including this type of measure needs to be explained, or I would consider omitting this from the manuscript. I am unaware of any literature to suggest that parent’s perception of genetic transmission of mood disorder to their children is associated with their own well being, nor is there any described here, and I have concerns regarding the soundness of this hypothesis. I am not surprised these variables were not correlated. The high genetic loading of bipolar disorder is well established in both the scientific community and general public. I would presume most parents with bipolar disorder would adequately understand that their children are at an increased risk of developing mood disorders compared to the general population. If this was not the case, this may reflect the education of the parent, which is not measured in this study or reported at least. Major revisions need to be made to the introduction, rational and objective of this manuscript. Once a clear objective is described, the title of the manuscript should be revised to reflect this.

(2) The discussion of this manuscript needs to be revised in the following areas:
a. The primary aim of this paper is not articulated as to identify interventional targets for bipolar disorder, nor is it placed to do so. Please omit this from the first line of the conclusions. This research has the potential to inform more research on potential targets for intervention.

b. The data from this study does not suggest that interventions targeted at enhancing coping may improve adaption. Please revise, as this is over interpreting the results. This study did not examine the impact of any interventions. There are other areas where this is described. Please revise accordingly.

c. It is not clear why the authors are suggesting the following: “One may speculate that dispositional pessimism is not only a risk factor for mental illness, but is perhaps a manifestation of the disorder”. The finding that dispositional optimism is negatively correlated with depressive symptoms does not suggest the latter statement. This is over interpreting the results.

(3) The limitations of this study are not adequately described.

a. The authors comment on the self-referral nature of this study and how this limits the generalizability of their sample, however, the reasons for this are not adequately described. There is a major concern of selection bias here, not only because subjects may be interested in the research question (a special population), but because there is a high possibility that the sample may not even have bipolar disorder. In addition, the authors present no information on education or socio-economic status (SES) of the parents or any other comorbid diagnoses which has large implications for family functioning and parents psychosocial functioning. If education or variables of SES are not available, the authors need to address the potential impact this has on their findings and how it relates to external validity.

(4) Is there any information on the location of the subjects (i.e., Country, State, Province)? Please include if it is available. If this information is not available, the authors need to comment on this in the limitations section.

(5) The scores on the scales are difficult to interpret as there are no cut off scores reported. If there are cut off scores available, report these in the measures section. Are there any other comparisons of adaptation scores rather than Klinefelter syndrome? This is not a helpful comparison as the symptom presentation and etiology of these two disorders are very different despite a shared high genetic loading.

(6) Were there any associations between the demographic variables and perceived risk to children or adaptation? If the authors have these data, they should report this as the demographic information also has important treatment implications (e.g., racial differences).

Minor Essential Revisions

1) The tables are poorly formatted and difficult to interpret. Please add in the unit of analysis in Table 1 and justify all tables so the alignment is straight. It is unnecessary to describe the measures in Table 3. Please remove this information and replace with the measure name. Measure descriptive information
should be described in the methods section.

2) It is unclear what “demographics” means in the multivariate models, did the authors include all demographic variables in to the model? Please describe this in the results section. Please clearly state the variables that were included in the final model as this is not clear.

3) The proportions of race categories do not add up? Did some respondents report more than one category? Please revise accordingly as this is not clear.

Discretionary Revisions

1) Identifying predictors of adaptation is helpful for informing future research and is in an understudied area, the authors should consider re-working their primary aim to: To identify predictors of adaptation in parents who self identify as BPD. I would suggest leaving the data regarding parents perception of risk to their children as descriptive (i.e., removing the logistic regression findings and leaving in the figure and correlations).

2) The authors should consider revising the title of the manuscript. The subjects are not necessarily affected with bipolar disorder. To appropriately inform the reader, I would consider revising to: A survey of parents who self identified as having bipolar disorder.

3) Means on an ordinal 1-5 scale are not very informative. The authors should consider revising to proportion ratings within each category (1-5)

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests