Author's response to reviews

Title: Risk factors for antenatal depression, postnatal depression and parenting stress

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Author's response to reviews: see over

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Dear Editorial Team,

Re: Risk factors for antenatal depression, postnatal depression and parenting stress.

We are most pleased to have the opportunity to revise our manuscript and resubmit it for consideration for publication in BMC Psychiatry. We are sorry for the delay, due to unforeseen work commitments. We have addressed the comments from your reviewers in a revised manuscript and thank them for their input. The following pages give a point-by-point response to the concerns, beginning with referee 1 (Catherine McMahon) followed by referee 2 (Debra Creedy).

We have ensured this revised manuscript conforms to the journal style. We believe the manuscript has been strengthened by the review process and are grateful for the helpful comments provided by the reviewers. We hope this revised manuscript is now considered publishable in its current form. We look forward to hearing from you at your earliest convenience.

Yours sincerely

Dr Bronwyn Leigh Professor Jeannette Milgrom

Amendments from Reviewer 1: Catherine McMahon

Introduction

Discretionary Revisions

1(a). The reviewer suggests a discussion of the similarities and differences between antenatal and postnatal depression with depression at other times in the lifespan.

Action: The comparison of risk factor profile between maternal-related depression and depression at other times is not a focus of this paper. Rather, we have sought to illuminate the similarities and differences in risk factor profile within maternal-related depression - antenatal and postnatal depression - as well as parenting stress. Therefore we have only made brief reference to the overlap in risk factors across the lifespan, which appears in the Background section, as follows:

'A review of the empirical literature revealed a range of risk factors similar to risk factors for depression at other stages of the lifespan. These and additional risk factors that influence the onset of antenatal and postnatal depression differentially are summarised below.'

1(b). The reviewer also suggests a consideration of attachment styles and caretaking history as risk factors for antenatal or postnatal depression.

Action: We agree that this is a fair point regarding the exclusion of previously identified risk factors in this study but we have limited our focus to core risk factors described in current meta-analyses and reviews. However, we have made note of the importance of attachment in the Limitations paragraph in the Discussion, as follows:

'Finally, although this study assessed many risk factors, it is a challenge to account for all previously identified variables in any one multivariate study. Caregiving history including a harsh, rejecting parenting style in one's family of origin and attachment styles have been linked with antenatal and/or postnatal depression (Bernazzani & Bifulco, 2003; Bifulco et al., 2004) and were not accounted for in this study.'

2. The reviewer points out that the literature review requires qualifying statements regarding the impact of postnatal depression on child outcomes.

Action: As per the reviewer's suggestion, the following has been added to the 1st paragraph under 'Background':

'The impact on child development is quite modest in high socioeconomic samples and greater when the postnatal depression is chronic and severe.'

Minor Essential Revisions

1. The reviewer suggests a broader consideration of harsh, rejecting parenting or parenting lacking in warmth as risk factors may be useful additions to the concept of abuse. Research conducted by Bilfulco and colleagues assessing attachment style should be cited.

Action: As noted in our response to point 1(b), in this preliminary study we were not able to include all factors. However, this factor was partly assessed through the use of the Parenting Stress Index, which has a Parent Domain subscale on attachment. As per point 1(b), this limitation is noted in the paragraph in the Discussion, described previously.

2. The reviewer noted that the attributional style questionnaire is included but there was no reference to attributional style as a risk factor for antenatal or postnatal depression.

Action: The reviewer is correct that there is an absence of reference to attributional style as a risk factor for antenatal depression. Thus, the following has been added (underlined) under 'Risk Factors for Antenatal Depression':

'There appears to be a paucity of research examining major life events and negative cognitive attributional style and their role in antenatal depression.'

There is an existing reference to negative cognitive attributional style and its role in postnatal depression on page 4 under 'Risk Factors for Postnatal Depression', as follows. Thus, no additional revision has been made.

'Three major meta-analytic studies have been conducted revealing a number of risk factors strongly associated with PND: a history of depression, antenatal depression, antenatal anxiety, stressful life events, negative cognitive attributional style…'

Major Compulsory Revision

1 (a). The reviewer requests a more detailed definition of the construct of parenting stress.

Action: We have added the following paragraph in the Background under the subheading 'Parenting Stress':

'The term 'parenting stress' encompasses the difficulties in adjusting to the parenting role. Previous research has used a broad construct of parenting stress and employed various methods of measurement. These have been inconsistent in their conceptual definition and measurement but do have broad overlap. In this study, we refer to parenting stress as it pertains to the Parenting Stress Index (PSI), outlined in the Measures section. The PSI examines the level of stress within the parent-child system and consists of

factors reflecting a parental domain of coping and perceptions of the child. Thus, the subscales assess a range of factors including depression, maternal health, difficult child and difficult parent-child interaction. '

1 (b). It needs to be made clear in the literature cited related to parenting stress whether the literature has used validated measures for the construct of parenting stress.

Action: The concept of parenting stress can be used broadly, as acknowledged in the previous point. Accordingly, we have made amendments to the following two paragraphs, which appear under 'Parenting Stress'. The qualifying additions are underlined:

'In our earlier studies we found that women suffering from postnatal depression were less attached to their infant, found their infants more demanding from 3 months postpartum and that up to 42 months they continued to show significant parenting stress <u>according to the PSI</u> [9, 27] and that their interactional difficulties may persist until 3 years postpartum [28]. Thus, understanding the precursors of parenting stress is important because of the potential implications for child development and adjustment, as well as parental adjustment [28-30].

It is unclear however, whether the constellation of risk factors for parenting stress are similar to those identified for perinatal depression. ... <u>However some of these studies have not used validated measures for the construct of parenting stress.</u>'

1 (c). The sentence "Women with severe parenting difficulties including sexual abuse have reported feeling less confident and less in control as parents…" needs clarification.

Action: We thank the reviewer for highlighting an unclear sentence. We have amended it, as follows:

'Women with a history of childhood sexual abuse have reported parenting difficulties including feeling less confident and less in control as parents [32], have experienced more stressful life events in the previous month [33], have low social support [34] and a negative cognitive style [35].'

Methods

Minor Essential Revision

1. A broader and more comprehensive description of the PSI measure was requested, including the factor structure.

Action: The following addition has been made to the description of the PSI on pages 9-10, with the underline denoting additions to the original description:

'The Parenting Stress Index (PSI) [54] measures the level of stress within the parent-child system. It was developed as a screening and diagnostic instrument for those at risk for the development of dysfunctional parenting and behavioural or emotional problems in children. The PSI consists of 101 items and has two domains: child (47 items) and parent (54 items). The child domain items relate to temperament and assess the degree to which each child characteristic causes stress to the parent. The child domains are: (1) adaptability, (2) acceptability, (3) demandingness, (4) mood, (5) distractability/hyperactivity, (6) reinforces parent. The parent domain items assess personal characteristics and level of social support. The parent domains are: (1) depression, (2) attachment, (3) restriction of role, (4) sense of competence, (5) social isolation, (6) relationship with spouse, (7) parent health. Total scores (used in this study) range from 101 to 505. Scores below 175 are considered low, 180-250 is within the normal range and scores above 260 are considered high. The PSI demonstrates good overall psychometric properties [54].'

Results

Discretionary Revisions

1. The reviewer requests additional information about the sample in relation to ethnicity, for example, how many women were raised in non-english speaking households.

Action: Whilst the sample description has important implications for interpretation and generalisability, unfortunately we do not have extensive descriptive data on ethnicity. However, we note in the results section the main groups and in the limitations section of the discussion that the sample is largely Australian-born. The sentence added to the Results section reads:

'The major ethnic groups were from Europe and America (North and South) and Asia.'

2. The reviewer suggests that given the point prevalence data on depression and anxiety are not central features of the research, they should be presented at the beginning of the results section along with the descriptive data.

Action: We agree with the reviewer that the prevalence data could usefully be presented alongside the general descriptive data. Accordingly, we have moved the sub-section about point prevalence data from the end of the results section to earlier in the section when presenting descriptive data.

Major Compulsory Revisions

1. The reviewer asked for clarification regarding whether or not antenatal depression, postnatal depression and antenatal anxiety were entered into the regression models as dichotomous or continuous dependent and predictor variables.

Action: We have reviewed the document and have identified the source of the confusion. The error is on our part, that we preceded the three regression analyses with the following statement:

'In order to include all women who had a mild mood disorder, a lower BDI cut-off score was employed for regression analyses. A cut-off of 12.5 was therefore chosen as a means of being inclusive to better understand risk factors contributing to perinatal depression and parenting stress.'

This statement has been removed, as in fact the variables were used in a continuous way in the regression models, not as dichotomous variables as this statement suggests. We thank the reviewer for identifying this confusing oversight.

2. The reviewer asks for additional analyses on the assumption that the regression models used depression as a dichotomous variable.

Action: As explained in the previous point, we did not employ dichotomous, but continuous variables in our regression analyses. Thus, the reviewer's request is unnecessary. Again, we appreciate the reviewer's careful and considered reading of our results and hope we have allayed her concerns.

3 (a). The reviewer requested an additional analysis exploring whether PND predicts parenting stress if the depression scale items are excluded from the PSI.

Action: The reviewer's suggestion that it is important to demonstrate that the impact of the depression subscale in the PSI is not significantly affecting the outcome, we have included the following on page 11, under 'Results, Sample':

'Postnatally depressed and nondepressed groups significantly differed on the Parenting Stress Index (PSI). The PSI contains a 9-item depression subscale. To ensure this subscale did not significantly influence the observed differences between the depressed and nondepressed groups, analyses were

conducted with the depression subscale removed. Analyses revealed that even without the depression subscale there was a significant difference between the depressed and non-depressed groups on PSI scores (t (159) = -6.13, p < .001). The depression subscale was therefore retained for all subsequent analyses, keeping the questionnaire intact to maximise psychometric status.'

3 (b). In addition, she suggested additional descriptive data regarding subscales of the PSI in relation to postnatal depression.

Action: Given the large number of variables already included in the regressions and the sample size, we have only used total PSI scores. This is a possible area for future research.

Discussion

Minor Essential Revisions

1. The reviewer requests the overlap between constructs of PND and parenting stress be considered as a limitation in the discussion.

Action: In line with the inclusion of a broader acknowledgement of the overlap in construct between PND and the PSI, we appreciate the need to also make mention of this as a limitation. The following sentence was added to the limitations:

'The overlap between the constructs of postnatal depression and the PSI, as previously acknowledged, further limits the ability to interpret results. However, an attempt was made to minimise the confounding of results by confirming that observed differences on the PSI between depressed and non-depressed groups were not solely due to the depression subscale.'

2. The reviewer noted in the Introduction that non-inclusion of attachment styles as a risk factor needs to be acknowledged as a limitation.

Action: We have addressed this in point 1(b) of the Introduction comments from this reviewer. We have made note of this in the Limitations paragraph in the Discussion, as previously outlined.

Amendments from Reviewer 2: Debra Creedy

Minor essential revisions

Abstract: The reviewer notes that the abstract is unclear in relation to the sample size. Additionally, the calculation of the sub-sample (n=161) in the Method section was unclear.

Action: We acknowledge the need for greater clarity in describing the sampling frame in the method section. We have attempted to clarify this with rewording in the Method section, as follows:

'In addition to the 367 participants, twenty-one women declined participation citing a lack of time; five women had already given birth (prematurely); four women were uncontactable by phone, post or email; and fifty-one failed to return antenatal questionnaires. From the 367 women who participated antenatally, a sub-sample of 161 women completed postnatal questionnaires, with a further forty-seven who failed to return postnatal questionnaires; two which were undelivered; and one participant was excluded postnatally due to a stillbirth. For practical reasons, women were selected for postnatal follow-up on the basis of their due date occurring three-months prior to the end of the recruitment period, given that postnatal follow-up was at 10-12 weeks postnatally.'

This description should clarify that of the antenatal sample who completed questionnaires was n = 367. The 81 attrition in the antenatal phase referred to by the reviewer was in addition to this number. Similarly, the postnatal sample who completed questionnaires was n = 161, and the 51 attrition in the postnatal phase was in addition to this number. Thus, the abstract numbers are correct.

Methods/Participants: The reviewer notes that given the point prevalence is similar to previously reported estimates, perhaps the authors should provide evidence of the necessity to recruit more depressed women in the sample.

Action: The biased sampling strategy was not an attempt to achieve parity with previously reported prevalence estimates. Rather, it was employed as a deliberate strategy to ensure a stronger representation of women with elevated antenatal depression scores. However, the authors concede that this point could be made clearer in the text. Thus, the following sentence under 'Participants' was added:

'This was a deliberate strategy to increase numbers to further ensure a strong representation of women in the antenatally depressed group in order to facilitate group comparison and to maximise the ability to detect the multiple risk factors under investigation.'

Methods/Participants: The reviewer requests that recruitment rates in the further two month period compared to recruitment rates in the previous 12 months should be reported.

Action: The following two sentences were added under the 'Participants' section:

'Recruitment rates during this biased period were similar (approximately ten per week) to rates during the initial 12-month recruiting phase (approximately seven per week). A slightly higher recruitment during the final two months may have been due to a reduced commitment for participants, with their involvement only comprising the antenatal battery of questionnaires and not postnatal questionnaires.'

Limitations: The reviewer suggests that limitations could be strengthened by suggestions for future research.

Action: We are happy to make brief suggestions about future directions. As such, the following paragraph, the final paragraph under 'Discussion', was added:

'These limitations provide future research directions. Most notably, onset of depression, prior to or during pregnancy, may relate to duration of the depressive episode. Additionally, research into effective interventions for antenatal depression in an effort to diminish or ameliorate postnatal depression and early parenting stress seem warranted.'

Limitations: The reviewer suggests that limitations could be strengthened by suggestions regarding the extent to which the authors believe that these limitations bias results.

Action: We have attempted to further comment on and illuminate how the limitations of the study may have impacted the results. The following paragraph has underlined the additions to the original paragraph, under the 'Discussion' section:

'Limitations of this study include the under-representation of women who are not partnered or from diverse cultures in the sample and subsample, which largely comprised married, Australian-born women. Compared with Victorian averages from 2004 [55] this sample comprised a higher percentage of partnered (95.9% this sample; 86.5% Victorian average) and Australian-born women (87.5% this sample; 76.1% Victorian average). Thus, the results of this study may have limited generalisability to women from other cultures or unpartnered women. Those categorised in the antenatal depressed group were assumed to have their depressive onset in pregnancy but this was not established. As such, some women identified as depressed antenatally may have been depressed prior to conception. Previous research found that time

of depressive onset, prior to or during pregnancy, was related to the duration of the depressive episode [7]. '

Measures: The reviewer identified that information on cut-off scores for the BDI refers to "… two major Australian based studies employing a 12.5 cut-off score for clinical diagnosis [44, 45]." Only Affonso et al (2000) included a sample of Australian women.

Action: The reviewer is quite right. There was an error in this reference, which has now been amended. The correct second reference is:

Milgrom, J., et al., *Screening for postnatal depression in routine primary care: properties of the Edinburgh Postnatal Depression Scale in an Australian sample.* Australian & New Zealand Journal of Psychiatry, 2005. **39**: p. 833-839.

Discretionary revisions

A closer edit of the beginning of the Method section was suggested by the reviewer.

Action: We have attempted to take on board this comment and have made a specific effort to edit the beginning of the Method section, which now reads:

'Participants were primipara and multiparae mothers recruited from antenatal clinics in two major public hospitals in suburban Melbourne, Australia. The antenatal phase comprised 367 participants consecutively recruited over 12 months. For a further two months, the recruitment was biased to include all women screened as depressed (according to EPDS scores above 12.5), but not all women screened with EPDS scores below 12.5. '