Author’s response to reviews

Title: Trajectories of clinical and parenting outcomes following admission to an inpatient motherbaby unit

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Author’s response to reviews:

We thank the reviewers for their interest in this study and their most helpful feedback. Our point-by-point responses to each comment are provided in the Detailed Response to Reviewers below, and are presented as tracked changes in the revised manuscript.

DETAILED RESPONSE TO REVIEWERS

Reviewer 1

1. It is not at all obvious what the knowledge gap is, and therefore what this study adds to what is already known in this area. It is not sufficient to state that this is "the first study to use latent class modelling to examine the trajectories of clinical and parenting outcomes." I think the readers would like to know what these techniques can add to what is already known.

   - The following additional text has been added as suggested: ‘…A recent review called for more follow-up studies of women discharged from an MBU that include a focus on attachment and parenting 3. The current study responds to this call by….’

2. It's not clear why the authors are reporting trajectories for each clinical and parenting outcome separately, rather than profiling overall trajectories by including all of these outcomes in a single latent class analysis. For clinicians, overall trajectories would be more useful as many of the conditions tend to co-occur. If there is a reason for conducting separate analysis by outcome, please justify this in the manuscript.

   - Latent trajectories for each outcome were reported separately due to sample size restrictions. A larger model that profiles overall trajectories in a single latent analysis is an excellent idea for future research. This limitation has been added to the Discussion as follows: ‘…Trajectories for each clinical and parenting outcome were reported separately due to the small sample size. Larger studies that profile overall trajectories by including all outcomes in a single latent class analysis are warranted. Additional limitations include…’
3. Please provide statistical significance values and/or effect sizes for all increases / decreases, in text or tables.

- Effect size values have been added to the text in various sections of the Results section, as suggested.

4. Are the results in the abstract an accurate representation? They begin with "The majority of women (93.3%) followed trajectories that were characterised by deterioration in self-reported mother-infant attachment following discharge" which reflects badly on MBUs whereas there are also lots of positive results reported in the manuscript.

- The following additional text has been added as suggested: ‘…Overall, significant improvements in mean scores on measures of anxiety and parenting confidence were maintained 3-months following discharge. However,…’

5. Some minor corrections, suggestions and comments are provided in comments in the attached PDF.

- These corrections, suggestions and comments have been addressed throughout the manuscript, as suggested

Reviewer 2

1. Please clarify the type of patients that would be admitted to a private unit. Would these only be individuals with private insurance and what was their SES. Important as this is specific to a certain type of population and therefore results may not translate to publicly funded units.

- The following additional text has been added to the Results as suggested: ‘…All admissions to this MBU are voluntary and require private health insurance. The most severely ill mothers needing involuntary admission are cared for in the public health system…’

- Additional text has also been added to the limitations section in the Discussion, as follows: ‘….Additional limitations include ….. the assumed mid-high socioeconomic status of participants given top tier private health insurance is a requirement for admission to the MBU. These factors limit the generalisability of our results to women with more severe, low prevalence diagnoses and to women admitted to publicly funded units…’

2. All measures are self report, this is mentioned in the limitations as potentially problematic but this point should be fleshed out further. How could reporter bias influence results? How could this be resolved?

- Additional text has also been added to the limitations section in the Discussion, as follows: ‘….The use of self-report measures …..may also be considered problematic due to the
potential for reporter-bias. For example, past studies have shown that women with postnatal depression self-report a significantly more negative experience of bond with their child than women with postpartum psychosis 39…”.

Opportunities to address this issue are addressed in the original manuscript, in the sentences that follow.

3. Latent class growth models were used to look at different trajectories of outcome and predictors of outcome: maternal/infant age, parity, diagnostic group, psychosocial risk, maternal attachment, service engagement. What about other important factors that could help clinically with respect to understanding a woman’s potential outcome trajectory following inpatient admission to an MBU: support network, work status, caregiver availability out of hospital?

- We agree that there are a range of other important factors that could help to better clinically understand women’s trajectories following admission to an MBU, however data relating to support network, work status, caregiver availability out of hospital were not collected as part of this study.

4. <19% of admitted had complete data - this is important issue, sample size relatively small for growth analysis. Also suggests that the group lost to follow up may be different. What about missing data strategies - imputation? What about stability of the current result. Could boot strapping be included to look at stability of subgroups?

- Comparison of women who were and were not included in the follow-up study has been described in the Results section of the manuscript, as follows: ‘…There were no significant differences between women admitted to the MBU during the data collection period who were and were not included in the current follow-up analysis in terms of primary diagnostic group, maternal age, infant age, infant gender, length of stay, parity, partner status, country of birth, PNRQ or EPDS scores at admission, or DASS-21, KPCS, MPAS scores at admission or discharge. Women who participated in the follow-up had a lower EPDS score at discharge (M=8.83, SD=4.11) than women who did not (M=9.98, SD=5.23; t(310)=-1.98,p=.049), however the magnitude of this difference was small (eta squared=.012)…’

- The following additional text has also been added to the limitations section in the Discussion: ‘…and bootstrapping techniques to examine the stability of subgroups were not applied….’

5. Given the likely severity of psychopathology at baseline in order to warrant MBU as well as those willing to come in, it is not surprising that symptoms would increase again on a number of measures in certain women post discharge when support would decrease drastically. What were some of the clinical factors that distinguished those with worsening trajectory - more comorbidity, more medications, poorer baseline functioning. It does seem that worse psychopathology at baseline predicted poorer outcomes. This may be highlighted as important clinical factor to follow.
Factors that distinguished those women with worsening trajectories are addressed in the manuscript, under ‘Factors associated with trajectories of scores on clinical and parenting outcome measures.’

6. Although subgrouping based on clinical symptoms is interesting, it would be important to highlight the clinical relevance more. Did functioning improve in the women hospitalized? Were there subsequent emergency visits? What does this work tell us in terms of the need for transitional care for this population?

- Data relating to general functioning was not collected as part of this study.

No women reported subsequent visits to a hospital emergency department.

The need for comprehensive discharge planning and transitional care is addressed in the Discussion section of the manuscript.

7. Table 1. Clarify if length of stay is in days. What about other diagnoses. Would help to understand clinical complexity. How many women had personality disorders, multiple comorbidities, what type of medications.

- Responses to these suggestions are as follows:

Table 1 text now reads ‘Length of stay (days)’

Table 1 provides a summary of the primary diagnosis of women in the study;

No women had a primary diagnosis of personality disorder;

The following text has been added in the Results: ‘…49 women (65%) fulfilled criteria for at least one comorbid psychiatric diagnosis; of these, 66% had a comorbid depressive or anxiety disorder…’

A full medication review was outside the scope of the current study.