Reviewer's report

**Title:** Prevalence and Associated Factors of Post Partum Depression and Anxiety in two Peri-urban Communities of Karachi, Pakistan: A Quasi-experimental Study

**Version:** 1  **Date:** 24 May 2009

**Reviewer:** Jane Fisher

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There are gross disparities between the world’s high income countries, most of which have local data about perinatal mental health problems in women on which to base policy and practice, and resource-constrained countries where most women live, but few of which have local data. It is of public interest to have data about the prevalence and determinants of perinatal mental disorders from different settings published. The data from peri-urban areas of Karachi Pakistan, reported in this paper make a valuable contribution to the field, and confirm the higher prevalence of perinatal mental disorders in low income than in high income settings. In my opinion however significant revision and rewriting is warranted in order to maximise the clarity of description of the study and the nature and implications of these findings.

**Major Compulsory Revisions**

1. It appears that this paper is reporting data generated as part of a larger ‘quasi-experimental’ study investigating the impact of maternal mental health on child growth and development. This is not however described distinctly. It appears that the element of the project reported here actually comprises a prospective cohort study, with some data collected in pregnancy and data about maternal mental health collected at four time points after childbirth. These need to be made clear in the title and the methods section.

2. There is a lack of clarity in the theoretical background. Although the study is described as being about depression and anxiety, the introduction only focuses on depression. It might be that the authors are acknowledging that postpartum depression is an indistinct construct and an umbrella term covering a range of common mental disorders, but if so, this is not made clear. Rather their use of the term ‘postpartum depression’ is undefined. There is also some blurring of the blues and depression, which in my opinion introduces unnecessary confusion. A distinct introduction to conceptualisations and determinants of anxiety and depression in women after childbirth would reduce this.

3. Although there is acknowledgement that postpartum depression is multifactorially determined including by social factors like low socio-economic status, the authors use the much cited prevalence estimate of 15% without recognising that it was drawn only from research in high-income Anglophone settings. Over the past decade evidence has accrued that prevalence is much
higher in resource-constrained countries, in particular where women face gendered role restrictions. This should be described and then this project can be located appropriately in the cultural and socio-political context in which it was conducted.

4. A clear study aim needs to be stated, rather than a description at the end of the introduction of study outcomes.

5. The description of the methods would not permit another investigator to duplicate the study. Under the heading ‘Implementation and sample size’ there is a description of what appear to be inclusion criteria which include ‘women residing at the study sites … and willing to be trained’. It then emerges that this applies to the local recruiters. The use of sub-headings and distinct descriptions in each sub-section would assist the reader. These could include: sample (needs to describe inclusion criteria for the participants), recruitment, data collection instruments, and procedures, (in which the selection and training of the recruiters and data collectors could be described).

6. There needs to be a clear description of how the data about some of the risks were conceptualised and measured, for example, how were exposure to violence, and whether or not the pregnancy was ‘accepted’, assessed?

7. As many women in this setting reside in multi-generation households, the procedures used to maximise privacy and ensure that participation was voluntary need to be described.

8. The recruitment rates are not reported and need to be added or, if unavailable, an explanation about this added.

9. The retention rates were much lower than have been reported by Rahman et al (2007) in a comparable Pakistani context and so an explanation needs to be provided of what women were actually asked to consent to do. Rahman’s name is misspelt throughout the paper and should be corrected.

10. It is idiosyncratic to refer to participants as ‘non-dropouts’, I suggest that a different descriptor is used instead.

11. It would assist the international readership of this journal if the sociodemographic characteristics were described in a way that is more widely recognisable, for example to use grade level equivalents to describe amount of education.

12. The authors report a prevalence rate as being the rate of any score in the clinical range during the postpartum year. This means that the data are not directly comparable to most other international data which are estimates of point prevalence assessed in cross-sectional studies. This difference needs to be addressed, perhaps with a more detailed presentation of data reporting the prevalence at the individual time points in addition to the overall prevalence. Some discussion of how many women met criteria at more than one time point and the risk factors for persistent affective disturbance are warranted.

13. It is easier to understand the results of the regression analysis if all the factors are entered in the same direction rather than varied as they are here.
14. It appears in Table 1 that incorrect data have been entered for Manzoor Colony – as these two columns add to only 75.1%. These data should be checked.

15. The use of clinician confirmation of screening assessments is a strength of this study, but there is no description of how anxiety was classified as there is only a reference to ‘PPD’. This needs to be added.

16. Overall the paper would benefit from English language editing. Some terms require definition or replacement e.g. ‘working women’ or ‘working mother’, ‘history of child death’, ‘accepted current pregnancy’. It is no longer acceptable to describe people who contribute their experiences to research as ‘subjects’ and an alternative term e.g. ‘participants’ should be substituted. Similarly, women prefer the term ‘birth’ to the word ‘delivery’. The title is quite long and could be shortened without loss of meaning.

17. Although these data are comprehensive and able potentially to cast light on the experiences of impoverished women living in Pakistan, the Discussion does not engage with the social determinants of mental health problems in women after childbirth. It would be useful to readers to know more about the social circumstances of women in this context and how similar or different they are from other parts of Pakistan and other countries and the implications of these for their mental health as mothers of infants.

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

**Quality of written English:** Not suitable for publication unless extensively edited

**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