Reviewer's report

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

Version: 2 Date: 24 July 2009

Reviewer: Jane Fisher

Reviewer's report:

The author’s response and the revised paper have attended to many of the matters raised in my original review. In my opinion it is overall a much improved paper, but there are some aspects of the revised paper that require reconsideration or more detailed elaboration.

Major compulsory revisions

1. The authors need to acknowledge from the outset that this is a study about women who are having particular experiences not of a condition per se. The infants of mothers with postpartum depression and anxiety are more vulnerable rather than the conditions themselves existing independently and exerting adverse effects on infants.

2. In the abstract and in the body of the paper the nature of the prevalence estimate needs to be described very clearly. It appears that an accrued prevalence – i.e. of any woman meeting diagnostic criteria at any of the assessment points is being reported. This is not directly comparable to the cross-sectional estimates that are reported in most other studies and this is not acknowledged. The exact data about how many women met criteria at each point and at more than one point are required, not a general estimate (‘about 15 percent’). Some discussion of the risk factors for persistent affective disturbance are still required.

3. It would be useful to identify in the description of the study to identify specifically which data are being reported in this paper.

4. The description of the data collection instruments is much more comprehensive and distinct, but there is still no description of how exposure to violence was assessed. As this is a major determinant of mental health problems in the participants in this study this needs to be provided.

5. It needs to be made clear whether the diagnostic interviews were conducted on all women who scored above the agreed cut off on the AKUADS at any of the assessment points or whether this was only undertaken sometimes. It needs to be made clear how this process was managed and whether there was any blinding.

6. In the revised paper it is reported that participants meeting diagnostic criteria were given counselling or referred for specialist treatment during the study. This will have had an effect on the outcomes and needs to be acknowledged. It is
likely to have contributed to the relatively low point prevalence estimates and must be discussed.

7. There is still no description of the procedures used to maximise privacy during the interviews and ensure that participation was voluntary and these need to be added.

8. Ultimately these data are about only 41% (267/651) of women meeting eligibility criteria. While there are no data about women who were not recruited, in the comparisons between those who were retained and lost to follow up, it appears that the participants to whom these data apply are more socioeconomically advantaged than those who were not followed. The implications of this for the generalizability of these data and the likely prevalence in less advantaged women warrant comment.

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

**Quality of written English:** Needs some language corrections before being published

**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’ below