Reviewer's report

Title: Paternal postnatal depression and relationship with a partner in Japan: a cross sectional study

Version: 2 Date: 3 November 2014

Reviewer: Liana Leach

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Review: BMC Pregnancy and Childbirth

“Paternal post-natal depression and relationship with a partner in Japan: a cross sectional study”

General summary

Thank you for the opportunity to review this paper. The paper provides some sound findings regarding the association between several demographic, social and health related correlates and paternal post-natal depression in Japan. It also finds an association between paternal post-natal depression and higher levels of negative feelings towards babies. However, sections of the paper are poorly written, and there are some major issues that need substantial improvement. These issues are outlined below:

Major Compulsory Revisions

1. The structure of the paper needs revision. Please ensure that each paragraph has an opening sentence explaining what the paragraph is about, and that the information in that paragraph then relates back to this opening sentence. For example, in the discussion, the second paragraph seems very long and contains several ideas, this paragraph could be separated to more clearly discuss the current study’s findings in relation to past research.

2. The paper intermittently provides results for mothers as well as fathers. Given that the focus is on ‘paternal’ postnatal depression, this is confusing at times. I suggest the paper focus only on findings for fathers, unless there is a pertinent reason (clearly justified in the paper) as to why findings for mothers are also included.

3. There is a problem with the lack of informed consent, as outlined in the ‘Participants/Methods’ section. The lack of written informed consent, leads the reader to question other aspects of the study (i.e. it suggests a lack of rigour), and wonder whether participants were adequately informed about the study before they participated. Could you please provide other details as to how participants were adequately informed – did you have a participation information sheet that outlined the possible risks to participants, how their data would be used/stored, and who to contact in case of mental distress?
4. More information about the measures assessing the covariates/explanatory variables is needed in the Measurement section. Please provide specifics – E.g. how were details of current illness and psychotropic drug use assessed? What were the questions/items asked?

5. I wonder about the analyses for the second part of the study – the t-tests assessing the relationship between depression diagnosis and feelings towards babies. Why didn’t you conduct a multiple regression analysis adjusting for possible covariates (similar to how the Logistic regressions have been conducted)? This would be more rigorous than just providing the univariate association.

6. How was ‘work stress’ coded for those who were unemployed? Please check that including the ‘work stress’ item in the logistic regression didn’t cause everyone who was unemployed to drop-out of the analyses.

7. Occasionally there are parts/data in the paper that pop-up unexpectedly. For example, the results mention the number of participants with specific psychiatric diagnoses (e.g. depression in 1, anxiety neurosis in 1, PTSD in 1, obsessive-compulsive disorder in 1), but the methods do not mention any psychiatric assessments? Therefore I am left wondering where this data came from, and what am I missing? Please check the paper carefully to ensure that any results mentioned are described clearly in the methodology section.

8. The paper needs to have a clear statement about what is novel about the current study, or how it adds to the existing literature in the introduction. You mention a previous study examining the mental health of Japanese fathers at one month post-partum, is the current paper the first to examine mental health at 4 months? If so, that should be stated clearly. Or, perhaps this is the first paper to use the Feelings Towards a Baby Scale at 4 months in Japanese men? The conclusion should again remind readers how this paper has added to the existing literature.

9. Please be careful about making statements that are not linked to enough evidence. I was concerned that in the opening paragraph of the introduction the authors mention death of infants due to abuse, and that this might be caused by negative feelings towards the child and postnatal depression. This statement is not supported by a reference. As statements such as these are sensitive, please either ensure they are supported by evidence or removed from the paper.

10. The limitations section needs expansion. Other than a lack of causality and low response rate, other major limitations include the lack of a diagnostic interview to assess depression, the lack of generalizability/representativeness of the sample due to a low response rate. I do not understand the final limitation listed by the authors in relation to economic anxiety and work stress – are you saying a validated measure of economic anxiety is needed? Please clarify what measure you did use in the Measures section.

Minor Essential Revisions
1. The title of the paper is inappropriate. It disproportionately mentions only one of the correlates under investigation (partner relationship), and does not mention ‘adverse feelings towards children’, which is a major part of the paper. Perhaps a better title would be: “Paternal postnatal depression in Japan: an investigation of correlated factors and links with feelings towards babies”.

2. The ‘Background’ section of the abstract is missing some information. For example, it could say “The aim of the study was to examine relevant factors associated with paternal postnatal depression at four months postpartum, including relationship quality with partners, partner depression, etc, etc….. The paper also examined how paternal postnatal depression was associated with feelings towards babies.”

3. The paper should mention more frequently that these are the findings for paternal postnatal depression in a sample of Japanese men – to remind the reader of the population group. Linking the current findings to other studies of Japanese fathers in the post-partum is needed.

4. The number of participants needs clarity. It is not 2032 as suggested in the abstract and other sections, this is only the number invited to participate. I suspect the number who actually participated (took part in the study) is 807 fathers and mothers?

5. I wondered about the decision to categorise fathers who are taking medication for a mental health problem (with cut-off scores that do not categorise them as depressed) in the depressed category. Why was this done? It seems to me that if they are not symptomatic enough to be above the relevant cut-point, then they are not depressed – does it really matter if they are taking medication? The ‘ever visited medical institution’ question should be enough to assess past mental health problem.

6. Please clarify details of the item ‘ever experienced visiting a medical institution for due to a mental health problem’. I wondered if this meant being hospitalised, or if just visiting a local doctor (GP) was also included.

7. How do you know mothers and fathers didn’t discuss their responses together (as mention in the first paragraph of the participants section)? As it was a mail-out questionnaire with self-report data, this could be possible. Please explain further.

Discretionary Revisions

1. I don’t think the N’s are needed in the rows for the tables, the percentages are enough. You could consider taking them out to make the tables less crowded.

Reviewer: Dr Liana Leach, Centre for Research on Ageing, Health and Wellbeing, The Australian National University.
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, and I have assessed the statistics in my report.

Declaration of competing interests:
'I declare that I have no competing interests'