Reviewer's report

Title: Immediate effects of the Fukushima nuclear power plant disaster on depressive symptoms among mothers with infants: A prefectural-wide cross-sectional study from the Fukushima Health Management Survey

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Reviewer: Emily Harville

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BMC Psychiatry Fukushima

The study is largely a prevalence and descriptive study. It is not completely clear why the authors have chosen to focus on medical care as the second major set of predictors (after geography).

Major compulsory revisions:

1. Strengths: Fairly high return rate for an unsolicited mail survey, and a baseline complete survey of the population. Still, 58% means 42% are missing. Some basic sensitivity analysis about how the missing group could affect the survey results would be in order.

2. The authors say they analyze the number of days postpartum at survey response. How about the timing of the earthquake etc. relative to the pregnancy? That is, exposure in the 1st, 2nd, or 3rd trimester? If I understand correctly, there are also a certain number of women in the study who were exposed prior to pregnancy, as well as some who were exposed after their pregnancies (who were excluded). It seems like these could be an interesting control group. The authors are unique in having a fairly large sample size for each of these time frames, so I would encourage them to take advantage of it.

3. Results: What is the reason for the (univariate) differences between provinces? Which of the control variables led to the largest changes in the effect estimates? Choosing confounders by statistical significance can be a shaky strategy and it's generally recommended to raise the p-value for inclusion if you are going to do it that way (Simulation study of confounder-selection strategies. Maldonado G, Greenland S. Am J Epidemiol. 1993 Dec 1;138(11):923-36. PMID: 8256780). Here, I have some concerns that intermediates are being included, particularly psychiatric history. Also, if you believe the disaster had an effect on any of the complications, they are potentially intermediates as well.

4. I suspect the authors are correct that changes and interruptions in prenatal care after disaster are associated with worse mental health (the authors may be interested in this paper, which discusses increases in inadequate prenatal care after Hurricane Katrina: Population changes, racial/ethnic disparities, and birth outcomes in Louisiana after Hurricane Katrina. Harville EW, Tran T, Xiong X, Buekens P. Disaster Med Public Health Prep. 2010 Sep;4 Suppl 1:S39-45. doi: 10.1001/dmp.2010.15. PMID:23105034), and they are careful not to ascribe too
much causality in any direction. I do think you would find that women who change or have interrupted prenatal care, regardless of the circumstances, generally have worse mental health, and vice versa. I don’t know if there are any data for Japan, but for other countries, this is the case. Do the medical care questions refer specifically to the disaster? How were they worded?

e.g.


Prenatal care visits and associated costs for treatment-seeking women with depressive disorders. Lin HC, Lin YJ, Hsiao FH, Li CY. Psychiatr Serv. 2009 Sep;60(9):1261-4. doi: 10.1176/appi.ps.60.9.1261.PMID: 19723744

Minor essential revisions:
1. If the authors could remind the readers earlier of the exact dates of the earthquake/tsunami/nuclear plant accident, it would be helpful in locating the timing of the questionnaires and pregnancies relative to the timing of the disaster. (It comes up eventually; it’s just that when reading the abstract, the dates are a little mentally unfixed.)

2. Methods: Does “double pregnancy” mean a twin gestation or more than one pregnancy?

3. If at some point the authors could provide a sentence or two of background about how prenatal care works in Japan – I assume there is universal coverage? Is there any difficulty in accessing care or switching providers? – that would provide context for those findings.

4. Did the authors consider examining SES at the regional level as a proxy for individual status? Does the province-level SES correlate with the radiation levels in any direction?

5. Typo, table 2, Costal

Discretionary revisions:
1. 1st sentence: While it’s true that the evidence from nuclear disasters indicates that mothers of small children have higher risk for psychiatric disorders, the evidence from other types of disasters is not as clear (see Wickrama KA, Wickrama KA. 2008. Family context of mental health risk in Tsunami affected mothers: findings from a pilot study in Sri Lanka. Social Science and Medicine 66(4): 994-1007, for an example). While the number of nuclear disasters is a fairly small sample, perhaps evidence from other technical disasters (oil spills, etc.) could be brought in?
2. For what it’s worth, I don’t consider the lack of social support data a major limitation. There are many social and psychological factors that could be related to the outcome, but I would consider many of them (such as social support) more mediators or moderators than confounders. So they would be interesting to study more, but would not invalidate the major findings.

No issues on any of these:
4. Do the figures appear to be genuine, i.e. without evidence of manipulation?
5. Does the manuscript adhere to the relevant standards for reporting and data deposition?
6. Are the discussion and conclusions well balanced and adequately supported by the data?
7. Are limitations of the work clearly stated?
8. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
9. Do the title and abstract accurately convey what has been found?
10. Is the writing acceptable?

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

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

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