Reviewer’s report

Title: The role of intimate partner violence and other psychosocial events on postpartum common mental disorders: A survey-based structural equation modeling analysis

Version: 2 Date: 24 February 2014

Reviewer: Karin Rhodes

STUDY OVERVIEW:

This study attempts to address the gap in knowledge about how intimate partner violence (IPV) and “other psychosocial events” are specifically connected to postpartum common mental health disorders. It attempts to establish a framework that maps those relationships.

TITLE: Change the words “life events” to “risks”. The study refers to “psychosocial life events” but addresses socio-economic position, number of children, relationship status, and other health-related social factors - which are not an “event” per se. I think “risks” or “health-related social factors” would be more accurate than “events”?

ABSTRACT:

Stating in results that “most events showed indirect relationships with postpartum CMD” is confusing, since you go on to list multiple variables that you detected direct pathways for. It might be less confusing to start with mentioning the variables with direct pathways to post-partum CMD before listing variables with indirect relationships.

There should be consistency and/or clarity about findings of “relationships” vs. “pathways”, especially when considering that there is extensive literature about the existence of correlated risk factors. As you say in the abstract background, “literature lacks evidence about how these are effectively connected.” With addressing that gap in research knowledge being the primary aim, results reporting should be clearly connected to attempts to establish paths of connectivity. I would definitely stay away from any mention of causality.

BACKGROUND:

In terms of prior research in this area, it should be noted that some studies have addressed the concept of causal pathways in a variety of contexts that could be relevant and should be considered. [For example: Eynav Elgavish Accortt, Marlene P. Freeman, and John J.B. Allen. Women and Major Depressive Disorder: Clinical Perspectives on Causal Pathways. Journal of Women’s Health. December 2008, 17(10): 1583-1590.]
You state that in addition to other psychosocial events, you are “especially” interested in psychological and physical IPV leading to postpartum CMD. If you’re exploring an overall framework for the relationship of multiple factors acting on CMD, perhaps provide more of a rationale for why you chose to emphasize IPV and psychological and physical IPV?

“Common mental disorders” ("CMD") itself requires a much more extensive description, since it is your outcome of interest. For example, what are the exact criteria for CMD? Is it a validated classification for a group of diagnosable disorders? Where does it come from and which disorders are included? (The study mentions “depression, anxiety, and some somatic complaints”- more specificity is needed. What is the rationale for grouping certain disorders together and excluding others? What are the “somatic complaints”?)

This study relies on previous work done on an international scale (“consistent evidence across different cultures and countries”) but works with a very specific Brazilian population. The implications of the specificity of the sample should be discussed when being held up alongside international work.

The argument in favor of including both psychological and physical IPV is an asset to the study and the rationale is well argued, as is the acknowledgement of the difficulty navigating the nuances of considering the two forms separately and together. However, I think you do need to address why you did not include sexual IPV – after all these women got pregnant and there is an extensive literature around unintended pregnancies in IPV.

Paragraph 2: Clarify the relationship between gender differences and age differences in CMD prevalence.

Paragraph 6: You state there is “not much evidence on the role played by escalating acts of psychological and physical partner abuse on CMD following childbirth”… however, this study does not address the gap in knowledge about the effects of IPV escalation, so I do not think this statement adds to the paper.

Finally, since this was a study that built on the enrollment criteria used for other studies, give more background about the how and why of the broader project – and include references to those studies or reports, if they have been published.

Very important – either in the background – or early in the methods section – I think you need a good description of survey-based structural equation modeling analysis. When is it used? What is it good for? What are its strengths and weaknesses in terms of understanding the strength of association between multiple risk factors, when working with cross sectional data? You really need more introduction to help motivate the paper so that readers who are not experts in the analytic technique can understand the benefits of using this methodology.

METHODS:

Participants: Again it is critical to provide more detail surrounding the broader
study – or “other shared research purposes” for which women were enrolled. Specifically, what was the rationale for the inclusion and exclusion criteria?

How did you define an “intimate relationship”?

How did women who refused to participate differ from women who participated?

Setting: A little bit of background about the health care system in Brazil could be helpful for the audience to understand the choice of “public primary health care facilities” as a recruitment setting.

Also describe the socioeconomic status of the clinic’s population.

Specify what do the various levels of the Brazilian Criterion of Economic Classification (BCEC) mean?

Variables and measurements: Why were these particular domains (socio-economic position, stressors, substance (mis)use, IPV, and CMD) selected?

Literature has identified other variables of interest- for example, nutrition (Brenda M.Y. Leung, Bonnie J. Kaplan, Perinatal Depression: Prevalence, Risks, and the Nutrition Link—A Review of the Literature, Journal of the American Dietetic Association, Volume 109, Issue 9, September 2009, Pages 1566-1575). Of note, the literature has also identified protective factors that might mediate the impact of IPV on CMD: http://uknowledge.uky.edu/cgi/viewcontent.cgi?article=1114&context=crvaw_facpub. Why weren’t these types of measures included?

Why were the variables that make up each domain selected? In particular, why does the category of stressful life events during pregnancy include the number of children under age 5? I do not doubt that having young children is stressful but is this a validated measure? Why is relationship stability assessed specifically in a spousal context?

The measurement instrument narrative descriptions are extensive and helpful, but a table that specifies each instrument’s name, population, delivery method, length, reliability, validity, scaling/scoring, etc. would be an asset to the audience and allow for an easier read.

Substance use: The study uses the words “alcohol (mis)use”, “(mis)abuse”, “use”, and “misuse”. Choose the most appropriate term and maintain consistency.

Provide a description of the specifics of the total survey instrument used. How many total questions were included- to given an idea of respondent burden?

Did these women receive additional care based on study findings (for example, were referrals made for those reporting IPV or CMDs? ) Did they receive standard of care? What is standard of care?

RESULTS
Throughout the text and in the figures – I would avoid use of your analytic shorthand e.g. “SLEV” “AGEPR” “HHCND” “WSCHL” that require the reader to continually look back in the list of abbreviations to see what the term refers to. Instead use more understandable short hand terms. For example if “WSCHL” refers to “Women’s schooling achievements” – use the shorthand “EDUC”. Likewise, if “SLEV” stands for “Stressful Life Events during pregnancy”, note this seems like a poor term when the text describes this variable as being an amalgam of age at pregnancy, marital status, and having children under 5 years – none of which are “events” per se! So perhaps use the term “STRESSORS”? NSDUQ – use “DRUGS”. for HHCND (which is also very poorly described) use “HOUSING”.

Table 1 and each of the figures each need an extensive legend. While the authors define terms in the text, the table and figures should be understandable without having to go back to the text to clarify whether Socio-economic strata “A” or “E” is the highest strata or the lowest strata. Likewise for “household condition score”, which I could not sort out even when looking back in the text. Is a 0 worse or better than 7 for having water, sewage disposal, etc. these terms and their scores need to be fully described, in the text and legends of the table and figures. Each table and figure should alone. I really think the paper would benefit from a Measures Table that lays out each of the measures in the model (with references and number of items that comprise the measure) and the scaling and scoring of each.

Using terms like “loose conjugal relationships” is confusing if the metrics haven’t been described completely. (In this case, for example, the stratification of intimate relationships has not been precisely defined, so “loose” is a puzzling descriptive.)

Information about the substance use of both partners is helpful, but results reporting here is in the context of the couple as a unit. You never mentioned that the male partner also completed the risk assessment – or are you relying on the women’s report of their partners substance abuse? This needs clarification in the methods section.

There is very little detail surrounding findings of maternal age overall, other than references to teenage pregnancy. Was teen pregnancy the real topic of interest in the broader study? If so, that should be described more specifically in the methods section. If, in fact, the focus of the broader study was on teen mothers then perhaps some discussion of the role of the different age groups and whether some were more at risk than others in the bivariate analysis? Were there risks or mediating/moderating factors that were specific to the teenage or older population? There must be some rationale as to why age group (presumably the younger age group?) was treated as a stressful life event?

Very important: It would improve the manuscript to include a Table 2, that includes the bivariate analysis between each of the risk factors and the outcome of CMD. Currently it is unclear how many women in the study had the outcome of interest and how that outcome varied across the various risk factors. That might
address place a lot of the questions raised by the manuscript. It is very hard to evaluate the study findings without this information.

I will specifically ask the statistical reviewer whether the final model (figure 2) would benefit from including the coefficients on each of the connecting lines?

DISCUSSION

Strengths and limitations: It is mentioned that these measurement tools are “well known and comprehensive… already adapted for use in Brazil”. Was any work done to assess whether these tools are culturally generalizable? If not, that would be a potential limitation that should be mentioned.

How generalizable are these findings? It might be more appropriate for the background section – and in the limitation section to use and identify relevant research literature pertaining to Brazil.

Only 2.9% of the women invited to participate in the study declined. To what do you attribute this level of enrollment success? Was the initial survey part of their routine care?

Were there any interesting characteristics in participants that you had not anticipated that could affect results?

IMPLICATIONS AND FUTURE DIRECTIONS

You do not acknowledge that there are other risk factors that could be explored. This connects back to the need for a rationale for the risk factor selection for the broader study. While the scope of this study’s findings is necessarily limited to the variables explored, it is worthwhile to point out what other factors (unmeasured in the larger study) might also be salient and worth investigating – for example the woman’s level of social support.

I also think the issue of how findings should inform interventions is sorely neglected – but I acknowledge that this is beyond the scope of the current work and will address this issue in my on line commentary, should the paper be accepted for publication.