Reviewer's report

Title: The Buffering Effect of Relationship Satisfaction on Emotional Distress in Couples

Version: 1 Date: 2 August 2011

Reviewer: Joseph M Trombello

Reviewer's report:

The authors of "The Buffering Effect of Relationship Satisfaction on Emotional Distress in Couples" have written an appealing manuscript that asks important questions about the relationship between marital satisfaction, emotional distress and stressful life events among pregnant women and their husbands. The introduction is carefully and thoroughly reviewed and builds a helpful case for the importance of their research questions as well as speaks to several interpersonal models of depression. Method and results are clearly presented, while the discussion makes note of a variety of limitations. That being said, there are a few suggested changes below to enhance the quality of this manuscript:

Compulsory:

1) I'd like to see a much stronger case made for why they authors have chosen to focus on first-time motherhood. This transition in the first paragraph of "other risk factors associated with symptoms of depression" seems a bit jarring to what has come before. Furthermore, their comment in the next paragraph that "the risk factors for depression in pregnant women seem to be very much the same as for women in general" seems to argue against the need to focus on pregnant women as a special group. Why have the authors chosen to focus on this sub-sample? How has this been an understudied area? Why should the reader care? How would focusing on this sample of women help to advance the field or theories about the relationship between marital satisfaction and distress/depression?

This concern is echoed in their concluding paragraph that says "based on earlier research on the association between relationship satisfaction and emotional distress, we suppose that these findings are valid for most couples, regardless of pregnancy?" Why, then, have they chosen to focus on pregnant women?

2) I would also like to see a stronger case for why the authors selected their 12 risk factors? It feels more like that they were chosen because they are in the available dataset than for any substantive or theoretical reason.

3) Are there differences between fathers who completed the full 5 relationship items and the ones who completed less than this number? I'm just worried about data imputation a bit given that fathers could have completed a variety of the already very limited number of items in this scale. On the other hand, the alpha values and correlation numbers are helpful.
4) This cannot be changed, but the single item social support measure is less than desirable.

5) Why were women given a checklist of 53 somatic disease items while men only had 19 such items? Could this have affected their results?

6) Similar to number 4, this cannot be changed, but the fact that only a very small minority of participants were classified as low relationship quality is a problem for external validity and for clinical significance.

7) I'd like to see effect sizes computed for their results, especially in tables 1 and 2. Given the huge number of participants, I'm not sure if significant results are due to high power or due to a strong effect. The authors helpfully note this limitation, though, in their opening paragraph of the discussion.

8) Similarly, given the large number of analyses and comparisons run, I'd like to see some control or correction for multiple comparisons or capitalization on chance. Bonferroni correction may be one such (admittedly conservative!) approach.

9) In "the effect of relationship satisfaction on emotional distress" portion of the discussion, the authors note that "this finding has already been demonstrated for women in the present sample and is consistent with previous research on couples." If so, what is novel or exciting about their finding? Is this merely a replication?

Essential:

1) The transition to "buffer effects of relationship factors" in the introduction needs improving.

2) In the "level of relationship satisfaction in couples" section of the discussion, I'd like to see a stronger connection made to the decline in marital satisfaction after pregnancy literature.

Discretionary:

1) The final two sentences of the results are a bit confusing as written.

**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.