Reviewer's report

Title: Pictorial Representation of Attachment: Measuring the parent-fetus relationship in expectant mothers and fathers

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Reviewer: Bronia Arnott

Reviewer's report:

This manuscript addresses whether a non-verbal measure is a useful assessment of bonding in mothers and fathers in the prenatal period. The authors investigate the Pictorial Representation of Attachment Measure (PRAM). In an adequate sample of primi- and multipara women and their partners the study finds a significant positive association between the PRAM and a verbal measure of prenatal attachment (M/PAAS). The authors also investigate relations to individual characteristics of mothers and fathers, including parity and age. The authors conclude that this measure is quick, effective and easy to administer.

There are two main question posed by the authors: the first whether there are relations between the non-verbal PRAM and an established assessment (M/PAAS) and the second whether there are relations between parental age, gender and parity and the PRAM. The questions are clearly defined and the authors provide a good overview of the existing literature in the introduction.

The methods in the manuscript are appropriate and well described by the authors in the method section. However, I would like the authors to comment on the problems potentially associated with the 2 antenatal attachment measures being administered at the same time point.

The data presented in the results section seem to be sound. However, the authors do not present the range of scores for participants for the M/PAAS or for the PRAM. I would like these to be included, particularly for the PRAM since it is a new measure. The authors seem to have conformed to the relevant standards, including ethical clearance.

The discussion and conclusions seem reasonably well balanced. The authors discuss the limitations of the study, but perhaps do not place enough emphasis on them. While this is a useful research tool the fact that it has not yet been investigated with a clinical sample is a major limitation. Although the authors do mention this, it could be stressed further. Another major limitation is the lack of validity and reliability data. While the authors acknowledge this is a problem, I do wonder if this is something that they could have addressed. From the published protocol of this study it seems that they have collected more data that would be of relevance to these questions. It would be useful for the authors to comment on the other data that they have and whether it would be appropriate to include it in the paper. For example, a comparison with the Working Model of Child Interview
seems very useful and if they had data on the PRAM at any other time points in pregnancy this would also be very relevant.

The authors acknowledge the work of their own and that of others that they are building on throughout the manuscript. However, referring to my earlier concern, I do think that the authors could discuss further the other data that they seem to have in relation to this sample and this measure and whether it is relevant to include it in this manuscript.

Both the title and abstract do accurately convey what is found in the study, with one very large caveat: the use of the term ‘attachment’ in relation to this measure. In the strictest attachment terms a parent cannot form an attachment to a child (unborn or otherwise) – unless there is a significant degree of role reversal with clinical significance. Instead in the strictest terms bonding or caregiving would be more preferable. The authors should provide thorough justification as to why they have chosen the term attachment to describe their instrument.

The written English in the manuscript is perfectly acceptable. There are a couple of discretionary changes that I have suggested (see below).

In summary:

Discretionary:
1. Would suggest moving this sentence in Introduction section:
   “Moreover, only few studies have evaluated both the feelings of antenatal attachment in women and their partner concurrently (Lorensen et al., 2004; Ustunsoz et al., 2010)”
   to the end of the previous paragraph as it fits better with the content of the previous paragraph.
2. In the in Introduction section, there is a one sentence paragraph:
   “Finally, there is some evidence that demographic variables such as age of the parent show a negative relationship with feelings of attachment, although these findings are not consistent (Cranley, 1981; Lerum & LoBiondo Wood, 1989; Lindgren, 2001; Zachariah, 1994; Ustunsoz et al., 2010)”.
I suggest that the authors could unpack this idea a little more to make a more substantial paragraph.

Minor compulsory:
1. Third sentence of abstract refers to ‘young’ parents – I suspect this is a typo since this is not a particular characteristic of this sample.
2.

Major compulsory:

1. The authors need to thoroughly justify their reasoning for describing this measure as an ‘attachment’ measure, when in the strictest sense of the term parents cannot show ‘attachment’ to a child (unborn or otherwise) without significant clinical concerns. Suggesting that a parent is ‘attached’ to the fetus might suggest that they used the unborn child as a safe base from which to explore and a safe have to return to – yet this is not the case. Instead it would be preferable to use the terms bonding and/or caregiving or even to discuss the measure as a representation of the relationship between the parent and the fetus. The authors do use these terms throughout but unfortunately the very name of the instrument implies more than this.

2. The authors should discuss the decision to use the two assessments (PRAM and M/PAAS) at the same time point in pregnancy and whether this has any implications for the results.

3. The authors don’t give range of scores in this sample for M/PAAS or for the PRAM. I would like to see this information added to the results, especially in the light of the PRAM being a relatively new measure. It would be useful to know as much about the psychometrics of it as possible.

4. While the authors do propose that the PRAM measure may suffer less from participant social desirability concerns they need to make a more convincing argument for this. Indeed, it could be argued that the significant positive correlation between the PRAM and the M/PAAS might actually be indicating the desire of some participants to respond in a socially desirable way. It would be good if the authors could address these concerns further.

**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