Reviewer's report

Title: Mother-Infant Bonding is not Associated with Feeding Type: A Community Study Sample

Version: 3 Date: 20 Aug 2018

Reviewer: Jill Demirci

Reviewer's report:

I appreciate the authors' very thorough responses. I am still concerned, however, that the title and some of the narrative is misleading regarding the distinction between maternal infant bonding versus relationship dysfunction. I understand that few instruments exist to measure "bonding," but to say that the PBQ sum score and individual scale measure quality of bonding (or just use the positive items to assess quality of bond) is a misrepresentation of the scale and what it has been validated to do. I do think that this paper could be strengthened by clearly and consistently noting that authors are measuring "impaired mother-infant bonding" rather than "bonding."

Along the same lines, I think some statements in discussion seem to creep a bit toward an overall subjectively negative view of breastfeeding/over interpret/misinterpret your results. For example, lines 45-52 on page 17 (i.e., argument that women breastfeed to be "a good mother", which authors equate to a strong bond; in fact, the narrative of "good mother is complex and also tied to the desire to provide infants the "healthiest start"—the idea of nutritional and immunological superiority of breast milk). Also line 24, page 18, "bonding is likely achieved via multiple modes, in which breastfeeding plays a minor part, if at all." The entire last paragraph of discussion also rests on the premise that you measured quality of bond. A more balanced discussion might read something like you found no difference in impaired bonding regardless of feeding method and that mothers may be reassured that we don't currently have evidence that the bond with their child will be negatively impacted if they choose to formula feed.

Minor points:
What is the actual power for detecting differences in disordered bonding/bonding, given that the sample distribution of non-breastfeeders vs breastfeeders is very lop-sided with almost 90% breastfeeders? It is noted in the middle of discussion paragraph on page 19 that the "sample was large enough for the analytical methods employed," but that seems to circumvent the issue of whether there is actual power to detect difference if one exists. Power should be discussed as a primary limitation of the study and/or a discussion of actual power to detect difference should be included in results. I am not familiar enough with Bayesian stats to know whether this lopsided distribution is a major concern, and I'm sure the majority of readers are not either—so important to include a bit on this.

The PBQ description of scoring on page 7 and 8 is still unclear. What does higher score/cut-off score indicate?—disordered bonding I assume, but this should be made explicit.

The description of the new analysis using only PBQ positive items belongs in analysis section, not results.
Table 1—what is criteria for "significantly below/above" median? Typically actual income ranges are reported with a footnote noting median income for context (which you do include). For clarity, I would suggest using actual income range groups or proportions above/below median instead of arbitrary classifications.

Table 2—Since entire table is means and SDs, recommend just using that in table columns for easy reading, instead of including it in footnote.

Page 12, second paragraph, your hypothesis #s seem different than introduced in the introduction. This should follow parallel structure (e.g., results of 1st, 2nd, and 3rd hypothesis) for sake of clarity.

Figure 1—In footnote, it is stated "as can be seen, neither infants age, nor breastfeeding change score of any of the subscales." It actually appears that mean subscale score is consistently lowest among those currently breastfeeding at 3-6 mos of age and lower for past and current breastfeeding on the rejection and anger scale. If this difference is not significant, I would just change that last footnote statement to indicate just that.

The Feldman study cited does not support the authors’ assertion that breastfeeding may have a similar effect on oxytocin levels as other forms of infant interaction. That study measured oxytocin in a controlled environment at three timepoints, when parents were not specifically interacting with their children or breastfeeding. I do agree that there is not strong evidence that oxytocin levels are categorically different during breastfeeding vs. when mother is engaged in other activities with infant, like skin-to-skin.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Unable to assess

Does the work include the necessary controls?
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Yes

Are the conclusions drawn adequately supported by the data shown?
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No

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