Author’s response to reviews

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

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Author’s response to reviews:

We thank the editors and the reviewer for their comments. For the majority of the comments we made the required adjustments in the text. There were a couple of comments that were unclear and we did not respond to, if there are further questions we will be happy to address them.

Reviewer’s comments:

• 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."

While we do agree that the PBQ was originally developed for the detection of bonding impairments (and hence the cut-offs used in this study), it has since been used in different studies as a measure of bonding, utilizing its continuous nature. We believe that by adding the extra analysis regarding the positive items, as well as by acknowledging this limitation, we provide the reader with the data needed as to the possible conclusion of this study.
We have also added a paragraph in the introduction explaining the preference for this instrument, including reference to the work of Van Bussel, Spitz, & Demyttenaere (2010) and Wittkowski, Wieck, & Mann (2007) that validated the PBQ against other scales which emphasized positive bonding, and further stressed this issue in the limitations section.

• 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).

These lines refer to findings from a large ethnographic study by Burns, Schmied, Sheehan & Fenwick (2010), which documented interviews with 500 women from six Western countries.

• 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.

We changed the sentence to the following: “… such bonding is likely achieved via multiple modes of interaction, in which the role of breastfeeding still needs to be established. “

In addition, we added the suggested wording: “In fact, these findings indicate that 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 not to breastfeed.”

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.

We treated breastfeeding as multilevel variable: exclusive/partial, past (i.e., not currently breastfeeding), and never, and included it in the correlation analyses a continuous estimate. Our power analysis is based on this type of analysis.

In addition, we used stratified bootstrap ANOVAs and regressions to adjust for different groups sizes.

With regards to Bayesian analysis, when there are unequal sample sizes, the uncertainty increases. Moreover, unequal sample sizes are not necessarily a problem in traditional statistics either, unless the different samples are otherwise biased.

That said, we highlighted the small number of 'never breastfed' respondents in the limitations.

• 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.

An explanation regarding the cut-off was added.

• The description of the new analysis using only PBQ positive items belongs in analysis section, not results.

• Done 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.

We asked respondents for a subjective measure of their income, noting in the question what the median income was at the time. This strategy was employed to minimize refusal to answer.
• 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.

We are not entirely clear what this refers to.

• 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.

This section has been corrected.

• 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.

We completely removed this sentence per the editor’s request to shorten the legends.

• 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.

To be quite accurate, our assertion is that “while oxytocin maybe associated with parental behaviors, the significance of breastfeeding in oxytocin-mediated maternal emotional bond is unclear.” Thus, we are not arguing that the effects on OT release are similar, rather that that breastfeeding related OT release may not have additive effects to OT release associated with other parental interactions.
We have clarified this in the text.

Editorial comments:

• Please reduce the legend of all tables. In this case please move some explanations to the text.

Done, we hope sufficiently.

• Because authors stated that they only used the positive items of PBQ and I could not see any validation regarding this questionnaire, please state this issue in the limitations of study. There is need for more explanation of scoring of PBQ in the method section.

(1) we added a reliability analysis to the results suction. (2) we added the following to the limitation section: “Moreover, while we attempted to overcome this limitation by selectively analyzing only the positive items in the PBQ, such a scale has not been independently validated in the literature.”

• Please add small explanation of Bayesian method that you have used in your statistic for better understanding of analysis.

We clarified the existing explanation in the methods section.

• I'm afraid your manuscript will need additional language editing. We recommend that you ask a native English speaking colleague to help you copyedit the paper. If this is not possible, you may need to use a professional language editing service. Use of an editing service is neither a requirement nor a guarantee of acceptance for publication.

Although the first author of the paper is a native US citizen, a native English speaker and a Stanford alumni, we used a professional English editor.
• Please clarify whether the ethics committee had also approved of the consent procedures used for your study.

This has been added. If the phrasing needs further clarification we will be happy to oblige.

• Please note that each additional file should be uploaded as separate additional files in the file inventory.

OK.

• Please amend the formatting around table 1, so that the text is not wrapped around the table.

Done