Comparative Effective Review Disposition of Comments Report

Research Review Title:
Breastfeeding Programs and Policies, Breastfeeding Uptake, and Maternal Health Outcomes in Developed Countries

Draft review available for public comment from December 22, 2017 to January 26, 2018.

Research Review Citation: Feltner C, Weber RP, Stuebe A, Grodensky CA, Orr C, Viswanathan M. Breastfeeding Programs and Policies, Breastfeeding Uptake, and Maternal Health Outcomes in Developed Countries. Comparative Effectiveness Review No. 210. (Prepared by the RTI International–University of North Carolina at Chapel Hill Evidence-based Practice Center under Contract No. 290-2015-00011-I.) AHRQ Publication No. 18-EHC014-EF. Rockville, MD: Agency for Healthcare Research and Quality; July 2018. Posted final reports are located on the Effective Health Care Program search page. DOI: doi.org/10.23970/AHRQEPCCER210.

Comments to Research Review

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Comments on draft reviews and the authors' responses to the comments are posted for public viewing on the Web site approximately 3 months after the final research review is published. Comments are not edited for spelling, grammar, or other content errors. Each comment is listed with the name and affiliation of the commentator, if this information is provided. Commentators are not required to provide their names or affiliations in order to submit suggestions or comments.

The tables below include the responses by the authors of the review to each comment that was submitted for this draft review. The responses to comments in this disposition report are those of the authors, who are responsible for its contents, and do not necessarily represent the views of the Agency for Healthcare Research and Quality.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 1	Evidence Summary	ES-5 Line 25 The description of studies retrieved for the reviews combines KQ1 and KQ2 searches. These were two very different questions to answer and literature searches should be describe separately.	Even though the search strategies differed, the abstracts and full-texts were reviewed simultaneously to assess whether they addressed KQ 1 or 2. We describe the number of included studies that met inclusion criteria for each KQ in the article flow diagram. Appendix E gives exclusion reasons for each full-text article.
TEP Reviewer 1	Evidence Summary	Definition of initiation (exclusive breastfeeding initiation) and exclusive breastfeeding need to be included	This comment appears to refer to the eligibility criteria. We allowed any definition of breastfeeding initiation or duration (as described by individual study authors) and noted this in the methods section of the full report.
TEP Reviewer 1	Evidence Summary	ES-12 Ln 29 Limitations of Evidence Base One other limitation KQ-1 was the variability of breastfeeding outcome definitions especially for exclusivity may have affected results.	We have noted that variability of breastfeeding outcome definitions may limit comparability of findings in the ES and Discussion section.
TEP Reviewer 1	Evidence Summary	ES-12 Ln 54 Future Research Need to include KQ-1c extent of intervention-related characteristics as need for future research since no evidence to address this was found.	A sentence was added to future research needs: “In addition, studies are needed to compare types of support – such as manual vs. electric pumps, or interventions delivered by International Board Certified Lactation Consultants vs. Certified Lactation Consultants – to tailor support to the needs of each woman.”

Commentator & Affiliation	Section	Comment	Response
Maya Bunik Univ of CO, Children's Hospital Colorado Public Reviewer 1	Evidence Summary	My RCT bilingual telephone support by nurses for mothers was not listed in this review. I think because it was a BF support intervention it should be included. Are 2 weeks of daily breastfeeding support insufficient to overcome the influences of formula? Bunik M, Shobe P, O'Connor ME, Beaty B, Langendoerfer S, Crane L, Kempe A. Acad Pediatr. 2010 Jan-Feb;10(1):21-8. doi: 10.1016/j.acap.2009.09.014. Methods RCT Intervention was 2 weeks of daily phone calls by bilingual nurse immediately after discharge from birth hospital Some mixed methods of phone follow-up after study completion Results Mothers with prior intent to breastfeed only were able to maintain more predominant breastfeeding (<4 ounces of formula supplementation) compared to usual care No difference in BF in 2 groups Discussion/Conclusion Not sure why it was not included in this review. Please consider adding this to the paper	Thank you. We did not include all studies of breastfeeding support. As stated in our introduction, this review will not address the effectiveness of individual-level primary care interventions to support breastfeeding; this evidence was recently summarized in a systematic review to support the USPSTF. The article cited here was included in that review.
TEP Reviewer 1	Introduction	The introduction was adequate.	Thank you.
TEP Reviewer 1	Introduction	However, Pg 1 Ln 19-30 Breastfeeding exclusively rates are not mentioned but are part of Healthy People 2020 goals and some of the outcomes reported later in the report. Breastfeeding exclusivity rates should be part of this section.	We have added a summary of current breastfeeding exclusivity rates to the introduction.

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 2	Introduction	A more extensive presentation of the potential harms is needed. If the only harm is considered a physical one (eg. fractures), then call it as such rather than imply a more extensive list of harms.	This comment appears to refer to the key questions. We list all eligible outcomes in the methods section and do not agree that a more extensive list of potential harms is needed.
TEP Reviewer 3	Introduction	Would be good to make a more compelling argument for why this review/report were a good use of taxpayer $.	Thank you. We've added the following text to the Introduction related: "Such knowledge is needed to inform allocation of resources in order to enable more women to achieve their infant feeding goals." "This review will inform the extent to which breastfeeding may be an effective primary prevention strategy for women's health."
TEP Reviewer 3	Introduction	Consider modelling your introduction after the Surgeon General's 2014 Call to Action to Support Breastfeeding and mentioning previous cost-analyses which indicate that the US could save billions of dollars by improving rates of breastfeeding [PMID: 27647492]	We are not attempting to demonstrate or assess the cost-savings associated with increasing rates of breastfeeding. The introduction is tailed to the scope of our review.
TEP Reviewer 5	Introduction	Clear and succinct introduction.	Thank you.
TEP Reviewer 5	Introduction	I think the authors should mention the WIC program here related to both the low prevalence of breastfeeding among women who participate in WIC as well as what breastfeeding supports are provided to women.	Thank you, we've added the following text to the introduction that relates to the low prevalence of BF among women participating in WIC: "In addition, a key system-based program relevant to breastfeeding is the Special Supplemental Nutrition Program for Women, Infants and Children (WIC), which serves 53% of infants born in the United States Because WIC reaches more than half of US infants, its programs have considerable impact on population health"
Peer Reviewer 7	Introduction	The introduction is generally well-written.	Thank you.
Peer Reviewer 7	Introduction	I would recommend two additions. 1) As above in General Comments, I would recommend either including an assessment of potential infant harms related the interventions, or explicitly excluding infant harms from this review.	Our eligible population is "childbearing women and adolescents." In Table 2, we describe the eligible outcomes and give examples of potential harms. We do not feel that additional detail related to harms is needed in the Introduction section.

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 7	Introduction	2) The Ten Steps of BFHI are applicable globally, but many of the specific requirements for BFHI certification vary from country to country and have been updated over time. This is important because studies examining the impact of the Ten Steps may be directly comparable to each other because the same predictors are used, but studies examining the impact of BFHI certification may be less comparable because the predictors may differ from study to study. A brief sentence describing how BFHI certification requirements may differ from country to country and over time could be inserted on ES-1 line 43.	We have added the following to the Introduction: "In each country, a BFHI Coordination Group is charged with designating facilities as Baby Friendly. As a result, details of implementation vary from country to country. The Baby Friendly USA "10 Steps to Successful Breastfeeding for Hospitals" are listed in Table A.
TEP Reviewer 8	Introduction	p=page; l-line p 2, l 3: revision suggested "...with a biennial maternity care practice survey of all U.S. facilities where births occur, results of which are distributed to each facility."	Thank you. We have made this edit to the Introduction as suggested.
TEP Reviewer 8	Introduction	p 2, l 32: I think it is important to add to the sentence that the effectiveness and harms are uncertain due to the unavailability of studies examining these issues.	We have edited the intro to note the uncertainty is due to limited evidence.
TEP Reviewer 8	Introduction	p 2, l 33-34: It is not clear if the authors relied on the 2012 review finding no controlled trials of if another search was conducted as part of the current review. Please clarify.	We did not rely on this review and only cite it in the Introduction to note that other reviews have found no controlled studies. We've edited the Introduction so that this is clear.
TEP Reviewer 9	Introduction	The introduction is well organized and helps to focus the reader as to what will be covered (or excluded) in the remaining report.	Thank you.
TEP Reviewer 9	Introduction	The inclusion of the analytic framework strengthens this section.	Thank you.
TEP Reviewer 1	Methods	The inclusion and exclusion criteria are well documented and justifiable for this report.	Thank you.
TEP Reviewer 1	Methods	The appendix has the exact search strategy used and are logical in presentation.	Thank you.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 1	Methods	However, Pg 12 Ln 8-30 Why are the literature search results for both KQ1 and KQ2 combined? In methods you stated that they were two separate searches. This search results should be presented. separately.	Yes, they were two separate searches for the same review. We summarize the literature search in one article flow diagram and note the number of full text articles relevant to each key question.
TEP Reviewer 1	Methods	Definitions: Pg 19 Ln 20 Please define exclusive vs non-exclusive initiation? Do you mean exclusively breastfeeding at discharge?	This has been clarified in the text by adding "Six of these reported on rates of any breastfeeding initiation (exclusive or nonexclusive) at hospital discharge."
TEP Reviewer 1	Methods	Pg 32 Ln 18 Initiation of exclusive breastfeeding is confusing. It is not clear if this is referring to exclusive breastfeeding at discharge. Does the study include initiation which is ever put the baby to the breast?	The text has been clarified to note that studies measures exclusive breastfeeding during a maternity stay or at discharge. No studies specified "ever put baby to the breast" as an eligible measure of initiation.
TEP Reviewer 1	Methods	Does exclusive breastfeeding mean no supplemental non-breastmilk feeding such as water during hospital stay?	We use the definition as provided by authors of individual studies. In the limitations section, we note that there is heterogeneity across studies in terms of definitions. In the detailed results section, we provide more granular definitions from each individual study.
TEP Reviewer 1	Methods	Statistical methods were used appropriately.	Thank you.
Peer Reviewer 2	Methods	Yes.	We interpret this to be the answer regarding appropriateness of statistical methods.
Peer Reviewer 2	Methods	However I would urge the writing group to consider an updated search now that the report has been fully assembled. By the time of public comment and then publication, the literature search will be more than 12 months old.	We have completed an update search and any newly identified studies will be incorporated before the final report is posted.
Peer Reviewer 2	Methods	The reviewer understands there indeed needs to be a cut-off, but given the brevity of available studies within some KQ, it would appear to be justified to do so.	We're not sure what this comment refers to. It likely pertains to the literature search date, we have conducted an update search and will incorporate any newly identified eligible studies.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 3	Methods	The methods seem appropriate but it is unclear why the search strategy used for looking at the relationship between breastfeeding and hypertension did not identify the PROBIT maternal health data is concerning (i.e. "Effects of an intervention to promote breastfeeding on maternal adiposity and blood pressure at 11.5 y postpartum: results from the Promotion of Breastfeeding Intervention Trial, a cluster-randomized controlled trial." PMID: 23945719)	We identified this study and it was excluded for ineligible country setting; for KQ2, to be consistent with the prior AHRQ review, we limited to studies set in very high HDI countries. This has been clarified in multiple sections of the text. We also discuss this study in the Discussions section (under limitations).
TEP Reviewer 5	Methods	Yes, the inclusion/exclusion criteria are reasonable and justifiable; search strategies are explicitly stated and logical; and definitions are appropriate as are statistical methods.	Thank you.
TEP Reviewer 5	Methods	Yes, the statistical methods are appropriate and it was encouraging to see that meta-analyses are considered.	Thank you.
TEP Reviewer 5	Methods	My questions are regarding reviewer training, i.e. how were these two reviewers selected, and was the kappa determined and/or what constituted an acceptable level of agreement?	The team was assembled based on clinical and methodological expertise; all members participated in the review. We pilot tested title/abstract, full-text, and abstractions to ensure a common understanding. Because of the iterative nature of the full-text review process that included team-wide group discussions on conflicts in multiple pilot test rounds, kappa scores across the review would not be meaningful.
Peer Reviewer 6	Methods	ES-15. It is noted here that KQ2 was limited to very high-income countries. This is an important limitation and should be described earlier in the presentation of results.	We limited to very high income countries to ensure applicability to populations of women in the US, and to be consistent with the 2007 AHRQ report.
Peer Reviewer 6	Methods	It is not clear why the impact of breastfeeding on maternal outcomes in high-income countries would be irrelevant to the key question so it is not clear why they were excluded.	This comment is confusing. We believe the reviewer may have meant that it's not clear why we limited to studies conducted in very high HDI countries. Since the KQ 2 on maternal outcomes is an update to a prior AHRQ review on this topic, our country setting eligibility criteria for KQ2 was made to be consistent with that prior report..
Peer Reviewer 6	Methods	P6, Table 2. The table does not include any description of "time frame" as required by the PICOTS framework.	This is not relevant to this topic. We did not limit to outcomes reported within a specified time frame.

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 6	Methods	P6, Table 2. The table indicates that only English literature was reviewed. This is not discussed in the executive summary and is not even mentioned in the text until page 124. It is a significant limitation that should be described earlier as part of the methods.	We note the English-only eligibility criteria in the methods and note this as a limitation in the Discussion section. We have also added this to the Evidence Summary.
Peer Reviewer 6	Methods	P123, l26-27. It says here that "interventions focused on critically ill infants were beyond the scope of this review". However, Table 2 gives no indication that the search was limited to infants who were not ill. This needs to be clear in the methodology section.	In Table 2 (eligibility criteria) we note that interventions specific to NICU care are excluded.
Peer Reviewer 7	Methods	The search strategies are explicitly stated and logical.	Thank you.
Peer Reviewer 7	Methods	The definition of the outcome measures appears sufficient for the purposes of this report; a more nuanced examination of the topic might include an assessment of the outcome "infant fed at breast" in contrast to those fed expressed breast milk.	This distinction was not made in any of the included studies relevant to KQ 1 or 2.
Peer Reviewer 7	Methods	The statistical methods used are appropriate, and I agree that given the heterogeneity of included studies, the interpretation of meta-analyses would be difficult.	Thank you.

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 7	Methods	I do have a substantial concern about the rigor with which the inclusion and exclusion criteria were applied for this report. Two important inconsistencies in the application of inclusion and exclusion criteria are: 1) Regarding the inclusion/exclusion criteria of study design, it is of high importance that study design be evaluated by assessing the methods described for each study. For example, in the included studies Hawkins et al 2014 and Hawkins et al 2015, data from the study was obtained by surveying mothers at 2-6 months postpartum regarding infant breastfeeding. The study design was described by the authors as "quasi-experimental."	We base our study design terminology on methods guidance designed for AHRQ's Evidence-based practice center. We feel our categorization of the Hawkins 2015 study as a cohort study is appropriate. The term "quasi-experimental" is potentially misleading; this study evaluated outcomes among cohorts of hospitals that varied by BFHI status.
Peer Reviewer 7	Methods	However, an identical methodology was used by Brodribb et al, Baby Friendly Hospital Initiative Accreditation, In-Hospital Care Practices and Breastfeeding, Pediatrics 2013, and was described as a survey study. These three studies should all be included, or they should all be excluded.	We excluded this study for ineligible design; authors analyze results (surveys of women) based on whether they recalled receiving certain hospital practices related to BFHI.
Peer Reviewer 7	Methods	2) Regarding the inclusion/exclusion criteria of population, the PROBIT study was conducted in a country that had a score of 0.659 and 0.666 on the Human Development Index, which is below the HDI threshold for inclusion as country with "high" or "very high" human development. This has important implications for this systematic review, because it speaks to the issue of generalizability. This inclusion criteria should either be revised, or the PROBIT study should be excluded from the analysis.	Belarus is currently listed on the "High" HDI country list. We use a standard list to assess all potentially eligible studies, and do not go back in time to match the year of participant enrollment with past WHO country categories. The applicability of PROBIT is addressed in the Discussion section.
Peer Reviewer 7	Methods	Another, more minor, inconsistency is that the population studied in the 2014 Hawkins paper is entirely included in the population studied in the 2015 paper, so these papers should not both be included as separate studies.	We disagree and have checked the methods of both studies.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 8	Methods	p 7, l 15-22: Please clarify what the criteria was for the 2007 AHRQ review with regard to countries. I recall that it included developed countries only but was it limited to high and very high development or only very high development?	Yes, the 2007 report included developed countries only and no further details were provided in the methods section about how this was defined. However, all studies included in that report were limited to "very high" HDI countries and so our current eligibility criteria is consistent.
TEP Reviewer 8	Methods	p 7, l 26-33: Please address how systematic reviews were selected and reviewed or clarify that what is written here applies to both individual articles and systematic reviews.	We clarified in the methods that the study selection criteria applies to both individual studies and published systematic reviews.
TEP Reviewer 8	Methods	p 8, l 42-43: I think it would be helpful to state here that you included only those SRs rated as low or unclear risk. This information is included in the following paragraph and reads as if it had been stated earlier.	We state this under "risk of bias assessment of systematic reviews"; to avoid redundancy, we will not repeat this under the study selection section.
TEP Reviewer 8	Methods	p 9, l 9: How did you handle SR reviews with overlapping studies?	We included systematic reviews (SR) published within the past 5 years that were rated low or unclear ROB; reported the number of included studies that overlapped with other included published SRs and summarized results in a table; and described the most recent or comprehensive review (which was also considered for strength of evidence grading [SOE]). For ovarian cancer, there were 4 recent SRs (Table 20). There was substantial overlap among the 4 SRs and we elected to synthesize a 2015 SR with new primary studies because it included the largest number of studies (n=41) and substantially overlapped with a smaller, more recent SR that only included 15 studies. For overall breast cancer, there was only 1 recent SR. We also included the 3 SRs and meta-analyses that had been included in the 2007 AHRQ report. Though the overlap among the 4 SRs and meta-analyses was not substantial, the results were similar. As such, we present all of the SRs in Table 15 and synthesize the recent SR with new primary studies.
TEP Reviewer 8	Methods	p 10, l 7-43: I admire the authors' diligent and time consuming work grading the strength of evidence. This was not an easy task.	Thank you.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 8	Methods	I spent a considerable amount of time trying to differentiate between evidence graded as insufficient and evidence graded as low, specifically in cases when only one study was available. I believe it was the assessment of precision of the effect estimate that differentiated the evidence and resulted in a lower grade (from low to insufficient).	This is one example of why a SOE grade might be "low" rather than "insufficient" when only one study was available for a particular intervention or comparison - i.e., the effect estimate was precise instead of imprecise.
TEP Reviewer 8	Methods	But I had a difficult time determining why the study was assessed as imprecise, even after going through appendix D.	Without knowing which SOE rating this comment refers to, we are not able to give a specific rationale.
TEP Reviewer 8	Methods	The authors comment "When one study reported an outcome of interest (with unknown consistency and imprecision arising from nonsignificant results or wide confidence intervals spanning the null), we usually graded the SOE as insufficient." It would be helpful to understand when it would not have been rated as insufficient.	We do not feel additional edits are needed in the methods. This sentence implies that consistent or precise results, if free from study limitations, could have been higher than insufficient.
TEP Reviewer 8	Methods	The authors also mention nonsignificant findings. Please clarify if this refers only to nonsignificant findings in an underpowered study?	In the methods (under SOE assessment), the phrase "…*imprecision arising from nonsignificant results or wide confidence intervals spanning the null*" generally refers to a single study that underpowered. We did not encounter studies that were adequately powered to test for no differences, for which we could potentially rate confidence intervals spanning the null as precise.
TEP Reviewer 8	Methods	Finally, when assessing consistency, was a different assessment given to a body of evidence in which many, but not all, of the studies revealed a significant association in the same direction (the others being null) versus a body of evidence in which studies revealed an effect in different directions?	No. An assessment was made regarding overall consistency of the literature addressing each KQ as it relates to the direction of effect and magnitude of association.
TEP Reviewer 9	Methods	I appreciated the inclusion of the full search criteria for Key Questions in the appendix.	Thank you.
TEP Reviewer 9	Methods	I also noted the efforts to search sources outside of the selected databases (Medline, CINHAL) to discover studies that may not be in the peer reviewed literature.	Thank you.

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TEP Reviewer 9	Methods	Table 2 very clearly outlines inclusion and exclusion criteria.	Thank you.
TEP Reviewer 9	Methods	The methods for assessing risk of bias are clearly articulated.	Thank you.
TEP Reviewer 9	Methods	The criteria for determining strength of evidence is comprehensively described.	Thank you.
TEP Reviewer 1	Results	The results section was well organized.	Thank you.
TEP Reviewer 1	Results	I appreciated having the tables with the text for each section.	Thank you.
TEP Reviewer 1	Results	The study characteristics were easy to identify from the tables.	Thank you.
TEP Reviewer 1	Results	The Key messages are stated in the executive study and in the results and discussion sections.	Thank you.
TEP Reviewer 1	Results	One major problem I had with the KQ1 results was that the breastfeeding outcome of exclusive initiation was not defined. This meant that I did not know what the actual benefit of each strategy was with regards to initiation. Exclusive initiation could mean that no supplements were introduced before initiation or it could mean exclusive breastfeeding during hospital stay or at discharge.	We have added text to the KQ1 results noting the variation in how breastfeeding initiation (and exclusive initiation) were defined across included studies. This is also addressed in the Discussion section.
TEP Reviewer 1	Results	Below are comments about the KQ2 Maternal Outcomes results section Pg 52 Ln 33 Reference 90 does not appear to be a primary source for the statement "Since breastfeeding is associated with increased parity, we examined the association between it and breast and ovarian cancer." How is breastfeeding associated with increased parity? It does indicate at least once.	We have revised the sentence for clarity. Like parity, breastfeeding (among parous women) is associated with a shortened menstrual history, which has been shown to be associated with a decreased risk of both breast and ovarian cancer.
TEP Reviewer 1	Results	Pg 52 Ln 49 I am assuming this initial analysis is for any type of breast cancer. I think it would be important to specify this at the start of the section.	Yes- the initial results text is for any type. We have clarified this in the results text.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 1	Results	Pg 53 Ln 12 I think the word "forever" is a typo and should be "for ever".	This was a typo and has been corrected.
TEP Reviewer 1	Results	Pg 55 Lns 30-33 "The authors reported that ORs were lower among case-control than among cohort or population-based case- control studies, but also acknowledged the potential for confounding by study location because the case-control studies were conducted in Asia." What is the source of this confounding bias? Is there a different risk of breast cancer inherent in being from Asia vs United States? Or is it a risk related to diagnosis protocols in Asia that differ?	The authors of the systematic review asserted that confounding by study location could have biased results but did not otherwise elaborate. It is not clear what the authors meant so we simply noted the small number of studies in the subgroup analysis.
TEP Reviewer 1	Results	Pg 66 Ln 44 I think the word "forever" is a typo and should be "for ever".	This is a typo and has been corrected.
Peer Reviewer 2	Results	The tables in the full report are adequate.	Thank you.
Peer Reviewer 2	Results	Below are some suggested changes to improve the clarity and readability of tables in the Executive Summary.	Specific edits are addressed below.
TEP Reviewer 3	Results	as noted above, the lack of mention of the PROBIT maternal health data is concerning (i.e. "Effects of an intervention to promote breastfeeding on maternal adiposity and blood pressure at 11.5 y postpartum: results from the Promotion of Breastfeeding Intervention Trial, a cluster-randomized controlled trial."PMID: 23945719)	This is not eligible for KQ2 due to ineligible country setting (and was also not included in the 2007 review). We have mentioned this in the Discussion section of the full report (the Limitations section).
Peer Reviewer 4	Results	It would be beneficial to have a set of figures (one for each key question) showing the flow of the literature search results.	We disagree. Our current article flow diagram shows reasons for exclusion (and more detail is given in the excluded studies appendix); the current article flow diagram also outlines the number of included studies by key question. Although we conducted separate searches, the abstracts and full-text articles were reviewed simultaneously.
Peer Reviewer 4	Results	Tables that summarize key findings: it would be of benefit to insert citations so reader can refer to the studies mentioned.	These tables are intended as a high level summary. Citations are noted in the summary text above, and in the detailed results section of the main report.

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 4	Results	For several tables, the SOE entry states "precise" but only one study described. Is precise appropriate?	Yes; the SOE have also been reviewed after the review period. Precision refers to the degree of certainty surrounding an effect estimate with respect to a given outcome, based on the sufficiency of sample size and number of events.
Peer Reviewer 4	Results	Table G. Summary of key findings for Breast cancer, and related tables. Row 1 (breast cancer overall) reports 17 studies, all finding a protective effect. This doesn't seem to align with the SOE entry of "Low, inconsistent, precise." It seems to fall in "medium, consistent" for this reviewer.	This has been edited; the row now reads “consistent” (in terms of direction of effect.
Peer Reviewer 4	Results	Also, unlike cardiometabolic outcomes, breast cancer diagnosis risk is higher with higher SES, so if anything, confounding is in the reverse direction. (also something to consider for the discussion of limitations/bias.	We do not have sufficient data to make this statement in the Discussion.
Peer Reviewer 4	Results	For postpartum weight loss, the evidence appears to show no benefit. Results are very inconsistent. This doesn't mean insufficient, right? Likely to be no benefit when there isn't consistency.	We disagree. Some studies do show a difference. The problem is the underlying heterogeneity in the evidence which makes it hard to understand why the results vary so much.
TEP Reviewer 5	Results	The amount of detail presented is appropriate, there are sufficient study details included, the key messages are explicit, and figures/tables are adequate.	Thank you.
TEP Reviewer 5	Results	My question relates to the country which studies can be conducted in order to be included. In multiple places it states that studies were from high and very high income countries. However, there are studies from China, Czech Republic, etc. This is unclear.	KQ 1 and 2 have different country setting eligibility criteria. This is explained in Table 2 of the methods and in the methods text. The Czech Republic is listed on the “very high” income country list (so studies set in this country would be eligible for both KQs), and China is rated “high” (studies set in China would be eligible for KQ1).

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 6	Results	ES-3 and p3. Key Question 2b is presented in both of these places but is never mentioned again. If there was no evidence found, this should be stated explicitly as is done with KQ 1c. However, it seems hard to believe that none of the hundreds of studies reviewed, none would have examined effects on maternal health stratified by the proposed characteristics.	We have chosen to present KQ 2a and KQ 2b together in the text so that results for each maternal health outcome are described together. Edits have been made to the text to make this clear.
Peer Reviewer 6	Results	P13, l8-12. These lines should be a bullet under the previous section, not a title for the following section.	Thank you. The formatting has been corrected.
Peer Reviewer 6	Results	P21, l44-48. It is unclear what the difference is between “accreditation” and “certification.”	When possible (i.e., when reported by individual included studies) we have clarified in tables and text the definition of BFHI “accredited” (or “certified”). We did not come up with definitions for these terms to categorize studies assessing BFHI interventions.
Peer Reviewer 7	Results	The amount of detail presented in the results is appropriate.	Thank you.
Peer Reviewer 7	Results	Figures, tables and appendices are excellent.	Thank you.
Peer Reviewer 7	Results	However, the descriptions of the characteristics of the included and excluded are inconsistent. For example, as stated above, the study design of Hawkins et al 2014 and Hawkins et al 2015 is described as a prospective cohort study, whereas a paper using the same methodology from Brodribb et al 2013 is excluded because of study design. These three studies should either all be included, or should all be excluded.	We disagree. The study noted here was excluded due to the way BFHI exposure was analyzed- authors analyze results (surveys of women) based on whether they recalled receiving certain hospital practices related to BFHI.
Peer Reviewer 7	Results	Given that the included studies range over a period of several decades, and given the data presented that breastfeeding rates in the U.S. have risen dramatically over that time period, it would be interesting to present an analysis of how the effect of interventions on breastfeeding prevalence has or has not varied over time.	We agree. However, given the heterogeneity of included studies, we are not able to describe whether there has been a change over time in terms of the findings of similar studies.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 8	Results	The amount of detail presented is appropriate.	Thank you.
TEP Reviewer 8	Results	It would be helpful to have the Tables list the studies in alphabetic order. This would facilitate finding study details quickly.	We have reviewed the tables and ensured they are consistently presented in alphabetical order.
TEP Reviewer 8	Results	Also, while the country information is included in the characteristics table, it would be helpful to have country included consistently on the results tables. It sometimes appears, but not always. There is room in the first column if this could be explanded to Author, Year, Country, ROB. Adding country would facilitate finding the relevant study details quickly when reading the text in the document. Adding country would facilitate finding the relevant study details quickly when reading the text in the document. I think this is because the results section often refers to studies as "the studies in the United Kingdom" or the "study in Croatia."	We have not added country information to the results tables; the text in the results section has been shortened/ condensed and fewer studies are referred to individually by country setting.
TEP Reviewer 8	Results	And while the study citations are provided, it would be easier and faster to locate the study with country added.	Thank you, we've added the country to Results tables in KQ1.
TEP Reviewer 8	Results	p 12, 30: "...impacts from 4 observational studies ..."	We have edited this bullet for clarity but have not used the word "impacts"; it now states that "…4 observational studies set in the US and the UK found greater rates of any breastfeeding initiation among those giving birth in BFHI certified facilities than those in non-certified facilities, although differences were not statistically significant."
TEP Reviewer 8	Results	p 19 l 25-30: In citations 66 and 67, the results appear to be statistically significant.	We checked the results; the tables are correct. The differences span the null in both cases.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 8	Results	p 24, l 30: I am familiar with another study that examined # of BFHI steps and breastfeeding at 6 weeks among mothers who intended to breastfeed for more than 2 months (DiGirolamo, Grummer-Strawn, Fein. Pediatrics 2008;122:S43–S49. The study may not have met your review criteria because the outcome is described as discontinued breastfeeding at 6 weeks rather than breastfeeding at 6 weeks but I want to mention.	Thank you. This study is not eligible due to the design and classification of "BFHI". Women were asked about their birth experiences retrospectively (the first interview was 1 month after birth) and "BFHI" steps were categorized based on the number of practices mothers experienced.
TEP Reviewer 8	Results	p 32, l 35: Hoddinott tested adoption of a policy to provide BF groups but adoption of a policy did not necessarily mean groups were offered. Table footnote indicates 10 groups were offered at baseline in intervention and control group. Did number of groups increase in intervention relative to control group after adoption of the policy?	We agree. Our text says that the study assessed a policy to provide BF groups to women (not that all women were offered BF groups). The number of breastfeeding groups increased in the intervention group (from 10 to 27) and the number in the control group remained 10. We've added this to the footnote.
TEP Reviewer 8	Results	p 35, l 5: Add citations at end of sentence,	Citations have been added at the end of this sentence.
TEP Reviewer 8	Results	p 37, l 48: Where are the findings from Madden study summarized in the final SOE tables in the executive summary?	We have not described this study in the ES for efficiency; it is unlikely to be relevant to current practice.
TEP Reviewer 8	Results	p 51, l 32: typo (delete and)	We've corrected this. The sentence was missing a comma.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 8	Results	p 52, 35: What did the authors do about systematic reviews with overlapping studies?	We included systematic reviews (SR) published within the past 5 years that were rated low or unclear ROB; reported the number of included studies that overlapped with other included published SRs and summarized results in a table; and described the most recent or comprehensive review (which was also considered for strength of evidence grading [SOE]). For ovarian cancer, there were 4 recent SRs (Table 20). There was substantial overlap among the 4 SRs and we elected to synthesize a 2015 SR with new primary studies because it included the largest number of studies (n=41) and substantially overlapped with a smaller, more recent SR that only included 15 studies. For overall breast cancer, there was only 1 recent SR. We also included the 3 SRs and meta-analyses that had been included in the 2007 AHRQ report. Though the overlap among the 4 SRs and meta-analyses was not substantial, the results were similar. As such, we present all of the SRs in Table 15 and synthesize the recent SR with new primary studies.
TEP Reviewer 8	Results	p 53, l 12: typo (for ever). This occurs more than once in text.	This typo has been corrected.
TEP Reviewer 8	Results	p 54, l 48: SRs with overlapping studies addressed here, but it would be helpful to add sentence on approach to methods as well.	We have edited this section to make it clear that we identified one new SR, but summarize 3 others that were included in the prior AHRQ report since there was not complete overlap in the included studies.
TEP Reviewer 8	Results	p 62, l 55: Didn't authors also include one Systematic Review in evaluating this body of evidence?	We included 1 systematic review that evaluated the association between breastfeeding and breast cancer defined by hormone receptor status (luminal, HER2, and triple negative). The 2016 review by Lambertini and colleagues included 11 studies is described in the results text of the report and is considered in the strength of evidence grading (Appendix D).
TEP Reviewer 8	Results	p 63, l 32: typo (s)	This typo has been corrected.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 8	Results	p 66, l 47: in high and very high income countries?	We included primary studies from very high human development countries for KQ2. Chowdhury et al. included 41 studies in their recent SR of breastfeeding and ovarian cancer; 35 were from high income countries. We rated this SR as relevant and noted that a few of the included studies were from lower mid-income countries in the relevance table in Appendix C. Note that the UN's human development index and the World Bank's Classification system are slightly different from each other.
TEP Reviewer 8	Results	p 72, l 26: Minor point, but the sections headings are no longer consistent. I would expect to see "Relationship between Maternal Cardiovascular Disease and Hypertension" here and "Individual Studies" below. Comment refers to future sections as well.	The section headings have been reviewed and edited for consistency.
TEP Reviewer 8	Results	p 78, l 40: Why is Stuebe 2009 (6) not mentioned at start of paragraph? This study examined duration of lactation and incident CHD, Nurses' Health Study.	This was a typo; the reference for the study has been added.
TEP Reviewer 8	Results	p 79, l 4-6: It is important to note that while the overall results were not statistically significant with respect to breastfeeding duration and incidence of CVD, there was an interaction with age. Among women who were age 50 to 59 years at baseline, those with a lifetime lactation of 7-12 months or ≥24 months were less likely to develop CVD (HR 0.80, 95% CI 0.67, 0.95 and 0.68, 95% CI 0.52, 0.89, respectively). Was this evidence considered in the grading the body of evidence for lactation and CVD? It is important because other evidence suggests that the association may not persist as women age (eg Natland Fagerhaug 2013). This has biological plausibility considering the competing risk factors as women age.	We have clarified this in the results text. Yes, this evidence was considered in the SOE grade.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 8	Results	p 79, l 29: typo "than mothers who breastfeed for at least 3 months"	Thank you. This typo has been corrected.
TEP Reviewer 8	Results	p 85, l 5: Suggestion adding citation (154) after reference to the one study showing an association.	We've added this citation.
TEP Reviewer 8	Results	It would be helpful to describe the direction of the association, which is not evident in the table entry for Hwang.	We've added text to describe the direction of the trend.
TEP Reviewer 8	Results	Although other studies were not significant, it may be of interest to summarize the general direction of the association between breastfeeding and fracture. It looks like most studies showed a decreased odds but with confidence intervals overlapping 1.0.	We have summarized the general direction of association: "The majority of studies reported lower odds of fractures with greater breastfeeding duration, but the results were generally not statistically significant."
TEP Reviewer 9	Results	The results section is very detailed for each key questions.	Thank you.
TEP Reviewer 9	Results	The tables have enough information to stand alone and the key variables and conditions for each study outlined in the tables provides enough information to get a strong sense of the study's setting, participant demographics, intervention, risk of bias and outcomes of interest.	Thank you.
TEP Reviewer 9	Results	I did wonder why a report by Abt and Associates on breastfeeding impacts from the WIC food package changes was not considered in the review? The title is Evaluating the Birth Month Breastfeeding Changes to the WIC Food Package? Was it not located or located and then excluded?	We will review this study and ensure that it was captured in our database.
TEP Reviewer 1	Discussion/ Conclusion	I think that the Discussion section was adequate but sometimes did not go far enough in describing how these results translate into policy or practice.	Thank you. We have edited the results to describe in more detail how they translate into policy and practice.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 1	Discussion/ Conclusion	One of the biggest limitations not discussed in this review is the differences in the desired breastfeeding outcomes of organizations and federal agencies. One of the biggest issues is defining exclusive breastfeeding at six months since some new research encourages the introduction of solids before six months if developmentally indicated (Robert Wood Johnson report) but others like the American Academy of Pediatrics state that exclusive breastfeeding to six months is optimal.	We sought to answer pre-specified key questions regarding the effect of interventions on breastfeeding outcomes (KQ1) and the association between breastfeeding exposure and maternal health (KQ2). Our objective was not to adjudicate the importance of the exclusive breastfeeding at 6 months as an outcome.
TEP Reviewer 1	Discussion/ Conclusion	Other comments Pg 113 Ln 16-18 The description of studies retrieved for the reviews combines KQ1 and KQ2 searches. These were two very different questions to answer and literature searches should be describe separately.	Although we conducted separate searches, the literature was reviewed for both KQs simultaneously. The text (and article flow diagram) show the number of included studies for each KQ.
TEP Reviewer 1	Discussion/ Conclusion	Pg 114 Table 29 Ln 12 Please define exclusive initiation.	Throughout the report, we have tried to clarify that this generally means that mothers were exclusively breastfeeding either during their hospital stay or at discharge following birth.
TEP Reviewer 1	Discussion/ Conclusion	Why aren't the study citation numbers included in the Table nor the accompanying text? I see the studies have one citation at the beginning of summary text but need to be included in the table and when referred to in the text. These tables and text should be able to stand alone.	We have added citations to the summary tables in the Evidence Summary and Discussion sections of the report.
TEP Reviewer 1	Discussion/ Conclusion	Pg 120 Maternal Outcomes Summary No citation numbers in text nor Table 34 For maternal outcomes SOE are only Low or Insufficient. Do we just assume that it refers to positive benefit?	We have added citations to summary tables in the Evidence summary and Discussion section. For KQ2, SOE grades refer to association between breastfeeding and the maternal health outcome. We have clarified when the grade refers to no association (for fracture).

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 2	Discussion/ Conclusion	Given the low SOE for most of the KQ and lack of evidence, the discussion section, particularly Future Research Needs (pg. 125) could be expanded.	This section has been expanded.
Peer Reviewer 2	Discussion/ Conclusion	Suggested standardization as well for reporting of breastfeeding data is suggested to be included in the discussion section. Much of the problem within this body of literature is inconsistency in reporting across studies.	Thank you, we added the following to the Discussion section of the report: "More generally, standardized definitions of breastfeeding, as well as consistent methods of collecting these data, are needed to facilitate future systematic reviews and meta-analyses."
TEP Reviewer 3	Discussion/ Conclusion	More detail would be helpful here, specifically (a) whether early weaning causes maternal risk of CVD or is a marker of maternal risk of CVD (or other diseases), parous women with a history of limited lactation should be offered interventions which have been shown to reduce risk of CVD/diabetes/htn/breast cancer, etc	This suggestion goes beyond what is appropriate given the limitations of the literature. Although lactation is associated with lower rates of hypertension and Type 2 diabetes, early weaning cannot be considered a "cause" of CVD and interventions targeting women for this factor alone (outside of other CVD risk factors) may not be appropriate. We are aware of no studies that assess this.
TEP Reviewer 3	Discussion/ Conclusion	(b) it would be appropriate to mention support for the hypothesis that lactation affects maternal visceral adiposity, blood pressure and maternal cardiac function provided by a rigorous animal study [see PMID: 24905416]	Our goal was to focus on human maternal health outcomes (not to support a particular hypothesis). We have added a sentence to the Discussion section noting that our conclusions (for hypertension and Type 2 diabetes) appear to be consistent with the "reset hypothesis".
TEP Reviewer 3	Discussion/ Conclusion	(c) it would be helpful to be more explicit (p159, line 25) -about which confounders should standardly be controlled for in future research	We added text to the Discussion noting these potential factors: breastfeeding intention, birth complications, diet, physical activity, tobacco use, mental health and social support.
Peer Reviewer 4	Discussion/ Conclusion	The discussion and conclusions are very nicely written.	Thank you.
Peer Reviewer 4	Discussion/ Conclusion	Additional factors to consider in recommendations for future research: Yes, Clarifying BF definitions. This cannot be emphasized enough. For example, what does breastfeeding initiation mean (does it count as never breastfed if tried once and baby didn't latch?).	We noted the issue of heterogeneity in breastfeeding definitions in the discussion; it's beyond our scope to make a claim as to whether initiation should refer to any attempt to latch.
Peer Reviewer 4	Discussion/ Conclusion	A need for studies to more consistently documenting breastfeeding intentions prior to childbirth.	We note that this should be recorded by future studies.

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 4	Discussion/ Conclusion	A need for studies to more consistently be standardizing measurements of cardiometabolic risk so this can be better tracked. For example the metabolic severity z-score by DeBoer and Gurka.	We note the need for standardization in outcome measures of maternal health.
TEP Reviewer 5	Discussion/ Conclusion	KQ 1b, page 118, I think the authors should state explicitly the SES factors that should be collected in surveys (education, income, health insurance status, WIC status) and subsequently analyzed. The way it is written, the text sometimes conveys it was the author who did not analyze by subgroup but sometimes data are not sufficient or available to analyze by subgroup.	We expanded this section to note some potentially important SES factors; these may differ across populations. We are only able to note that no evidence on certain subgroups exists in the published literature. We cannot make a claim as to whether the individual study authors had sufficient data to report on subgroups. We agree that sometimes data may not have been sufficient to analyze and results by subgroups of women (i.e., if a study sample had no significant variation in age/race/ethnicity, or if study sample was too small).
TEP Reviewer 5	Discussion/ Conclusion	KQ 1c, While there was no evidence to address this question, the authors should elaborate and describe the data that need to be collected as well as the types of study questions that need to be examined.	We note this under "Limitations of the Evidence Base" in the discussion (i.e., the lack of studies relevant to KQ1c). Under future research needs, we note the need for future studies to compare different types of breastfeeding support interventions.
TEP Reviewer 5	Discussion/ Conclusion	Future research needs, page 125, should expand on SES and list other factors that need to be collected in surveys.	We expanded this section to list some important factors.
TEP Reviewer 5	Discussion/ Conclusion	While the introduction raises the ACA, the discussion section should link back to the relevance of this work to the ACA discussion.	Our conclusions do not have direct relevance to the ACA. We note in the discussion that conclusions in favor of benefit for healthcare system-interventions and WIC peer support programs support portions of the ACA that aim to support breastfeeding.

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 6	Discussion/ Conclusion	ES-16, l4-7. It is hard to interpret this critique of the literature on KQ2. Since the outcomes of interest for maternal health generally occur in later life whereas breastfeeding generally occurs among younger women, the studies necessarily must look at breastfeeding many years ago. This is not a limitation but rather the nature of any exposure-outcome relationship that occurs over a lifetime. The same problem would occur even if there were an RCT or cohort design. On page 153, this is described in terms of secular trends in breastfeeding rates—however, it is unclear why increasing the percent of women who are in the breastfeeding group should affect the relative risk of breastfeeding versus non-breastfeeding.	Our comment was less about recall and more specific to secular trends in breastfeeding. We added text to the Discussion to clarify this point:, "in 1970, only 26.5% of women initiated breastfeeding, compared with more than 80% of women today. Because of these secular changes, confounders of the association between breastfeeding and maternal health have changed over time…"
Peer Reviewer 6	Discussion/ Conclusion	ES-16, l49-51. Given the earlier critique that the breastfeeding behaviour occurred decades before the maternal health outcomes were measured, it is hard to understand how poor maternal health could have prevented breastfeeding. This is understandable for postpartum depression and maternal weight gain, but for other outcomes, the hypothesis seems to be a stretch given the long time lag. This is inappropriately raised as a significant problem in the final conclusions, when reverse causality is actually unlikely.	We have edited this to add the following: Added language: In 1970, only 26.5% of women initiated breastfeeding…compared with more than 80% of women today. Because of these secular changes, confounders of the association between breastfeeding and maternal health have changed over time…women who initiated and maintained breastfeeding decades ago may differ from women breastfeeding today.
Peer Reviewer 7	Discussion/ Conclusion	The Discussion section clearly summarizes the results as they are currently written.	Thank you.
Peer Reviewer 7	Discussion/ Conclusion	However, the discussion section will need modification after the needed revisions to the Results section (above).	Yes, we have modified the Discussion as needed based on any changes to the Results section.

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 7	Discussion/ Conclusion	Of note, the recent systematic review by Patnode et al for the USPSTF included a discussion of the PROBIT study, although it did not meet their inclusion criteria either. The authors of this report could use a similar approach once the Results section is modified.	We do include PROBIT in this review for KQ1. For any key studies mentioned by reviewers that did not meet our eligibility criteria, we will consider whether they should be mentioned in the Discussion section.
Peer Reviewer 7	Discussion/ Conclusion	With respect to future research needs, this section is well-written.	Thank you.
Peer Reviewer 7	Discussion/ Conclusion	Two components might refine this section slightly: 1) On page 125, sentence 4 could include a statement about the need to study interventions to promote breastfeeding in both areas of low breastfeeding prevalence and areas of high breastfeeding prevalence. For example, "…whether certain interventions are more effective for groups of women who differ by socioeconomic factors or by local breastfeeding prevalence."	We have added the phrase "local breastfeeding prevalence" as recommended.
Peer Reviewer 7	Discussion/ Conclusion	2) If the Analytic Framework is modified as suggested above, page 125 Paragraph 2 could include a brief mention of the need for studies to assess adverse infant outcomes as well as adverse maternal outcomes.	We feel our methods make it clear that we would include any adverse outcomes attributable to the intervention itself (regardless of who experiences the outcome).
TEP Reviewer 8	Discussion/ Conclusion	p 114, l 46, Probit enrolled only mothers who intended to breastfeeding and who initiated breastfeeding.	Yes. We have noted this in the Applicability section of the discussion.
TEP Reviewer 8	Discussion/ Conclusion	p 114, l 48: The cohort study (N=1,417) compared rates of breastfeeding at 8 weeks among women discharged from hospitals that differed in the number of BFHI steps implemented ...lower rates of weaning by 8 weeks than implementation of less than 4 steps.	We are not sure what this comment refers to.
TEP Reviewer 8	Discussion/ Conclusion	Table 29: When the authors describe the Probit RCT exclusive breastfeeding outcomes in the first paragraph of the second row, they could point out that the comparison is to breastfeeding women in control hospitals.	We have edited these tables for brevity; the intervention and comparison are noted in the first row.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 8	Discussion/ Conclusion	Table 30: -I am trying to understand the basis for the rating of low for no benefit for education/staff training and initiation. Based on consistent findings from the studies (2 RCTs and 2 NRCTs) I expected moderate evidence for no benefit (similar to the moderate rating for benefit on BFHI and duration in Table 29). Rating requires clarification. -For the description of evidence for education and staff training and duration, didn't studies find significantly higher BF rates among the intervention group than controls?	Results are imprecise and the studies are associated with limitations. We have reviewed this rating and do not feel it should change.
TEP Reviewer 8	Discussion/ Conclusion	-For the last two sections of the table, I spent some time trying to understand why the evidence was low for benefit rather than insufficient as in other ratings (eg electric breast pump and peer support for father in Table 31) with only one well designed study but I believe it is because the evidence was rated as precise. However, the evaluation of precision is not entirely clear as I commented in the methods section.	We have reviewed this SOE grade. Due to the study limitations and imprecise findings, we feel the current grade is appropriate.
TEP Reviewer 8	Discussion/ Conclusion	Table 31: I am also having difficulty understanding the strength rating for WIC mother peer support and breastfeeding initiation/duration. The body of evidence includes 1 RCT, 1NRCT, and 1 cohort, findings are consistent and precise. The rating requires clarification.	There are study limitations and imprecise findings; we have clarified this in the table.
TEP Reviewer 8	Discussion/ Conclusion	Table 33: For the third row on mother peer support suggest adding to the overall summary of outcome " for subgroups of low income women defined by language " because the study was among women receiving WIC.	Outcomes for subgroups have been moved to a separate table.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 8	Discussion/ Conclusion	p 119, l 53: heterogeneity of results in terms of magnitude of effect? I believe the studies showed a consistent protective effect.	Yes, and this refers to statistical heterogeneity. We have edited the text to make this clear.
TEP Reviewer 8	Discussion/ Conclusion	p 119, l 54: I would state that the evidence for BRCA subtypes was primarily due to insufficient body of evidence. There was only one study.	This has been clarified in the text and table; we now rate SOE separately for some hormone receptor and BRCA subtypes.
TEP Reviewer 8	Discussion/ Conclusion	p 120: The summary for CVD is missing.	Thank you, this has been added.
TEP Reviewer 8	Discussion/ Conclusion	Table 34: -Authors need to clarify low for benefit and low for no benefit in the strength of evidence column.	We have clarified this in the table; specifically, we note that “low” for fracture refers to no association.
TEP Reviewer 8	Discussion/ Conclusion	-Breast cancer - findings were consistent in terms of finding a protective effect yet the strength of evidence lists inconsistent because magnitude of effect varies. Was this the reason for downgrading the strength to low?	For overall breast cancer, magnitude and direction of effect varied by breastfeeding duration; for any breastfeeding and shorter durations of breastfeeding, results were reported on both sides of the null.
TEP Reviewer 8	Discussion/ Conclusion	-Breast cancer in situ - but didn't all studies find no association? -BC Hormone receptor subtypes - same comment about how consistency was evaluated. ER result summary is missing.	We have revised all SOE ratings that refer to Breast cancer outcomes to make clear our final rating.
TEP Reviewer 8	Discussion/ Conclusion	-Ovarian Cancer - it is not clear in the table entry was is inconsistent. The studies in the SR?	We have changed this to consistent (meaning the direction of effect). The magnitude varies across studies.
TEP Reviewer 8	Discussion/ Conclusion	-CVD - I think the rating of insufficient evidence for CVD needs to be re-visited. Based on the evidence presented and the comment made earlier, I would like the authors to provide clarification for why the rating was not low for benefit.	We have clarified that studies report on very different composite outcomes which limits our ability to assess consistency.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 8	Discussion/ Conclusion	-Type 2 diabetes - I believe the SR was of 6 studies. Here magnitude of effect varies but rating cites consistent evidence.	We have revised this. Consistency refers to direction of effect.
TEP Reviewer 8	Discussion/ Conclusion	-Postpartum depression - is this rated with unknown consistency because the direction of effect is unknown (reverse causality limitation)?	The direction of effect varies by exposure, outcome measure and study design; due to heterogeneity in these factors, we could not assess consistency.
TEP Reviewer 8	Discussion/ Conclusion	p 124, l 48: suggested edit "poor maternal health may prevent the initiation or continuation of breastfeeding"	Thank you. We have made this change.
TEP Reviewer 9	Discussion/ Conclusion	The limitations of the current review are well stated and comprehensive.	Thank you.
TEP Reviewer 9	Discussion/ Conclusion	The Future Research section provides a multitude of topics for further study and I appreciated the sentence about examining methodology as well as outcomes. I also noted the section on page 156 covering deficiencies in methods.	Thank you.
TEP Reviewer 1	Clarity and Usability	This report is well structured except for items noted above.	Thank you.
TEP Reviewer 1	Clarity and Usability	The Discussion section should include more study citations so that it is clear which studies are referred to in the tables and the text.	These tables are meant to be a high level summary of our conclusions. Citations are in the text and Results sections (including Results tables) of the main report.
TEP Reviewer 1	Clarity and Usability	The main points are stated consistently.	Thank you.
TEP Reviewer 1	Clarity and Usability	Sometimes the Strength of Evidence is confusing since in the section for KQ1 description is Low for benefit or Low for no benefit but in the KQ2 section it is just Low.	We have clarified whether SOE ratings refer to “low for no association”, for example, or “low for beneficial association” in the KQ2 summary of evidence tables.
TEP Reviewer 1	Clarity and Usability	I would have liked more specifics about the future research needed to bring the insufficient results to a level where a more definitive recommendations could be made.	We have added additional details, including specific recommendations, regarding future research needs.
Peer Reviewer 2	Clarity and Usability	The report is very thorough and I have few comments.	Thank you.

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 2	Clarity and Usability	Since majority of its readers will only view the executive summary, I think that this section would be enhanced with inclusion of the following: 1. Table 3 from the body of the report (pg 10) table summarizing the grades for strength of evidence. Care also needs to be taken to consider SOE as an acronym versus written out - there are many inconsistencies.	Strength of evidence has been spelled out in the text above the table to avoid confusion. We have ensured that the acronym is used consistently.
Peer Reviewer 2	Clarity and Usability	2. The summary tables in the executive summary would greatly benefit from the insertion of references for each of the discussed studies. Currently there is no way to link the studies in the tables to the text where the citations can be found.	These tables are meant to be a high level summary. We have not added citations since these are available in the full Results section (including Tables).
Peer Reviewer 2	Clarity and Usability	3. It is critically important to define how the authors understood breastfeeding and how this search term was considered in their literature review. Breastfeeding in many of the supportive data presented simply implies any breastfeeding and does not specify intensity or duration. The report leaves the reader with a much healthier perspective of breastfeeding rates, when this simply reflects whether or not an infant received breast milk from the breast, even one time.	As noted in our Methods (table of eligibility criteria), we looked for evidence on whether interventions increased the initiation, duration and/or intensity of breastfeeding (for KQ1) and whether breastfeeding improved health outcomes (for KQ2). We did not set a definition for breastfeeding but rather used the definitions of outcomes (for KQ1) or exposure (for KQ2) reported in the literature. In the Discussion, we have added text that notes the heterogeneity of definitions in the literature and the extent to which these may be clinically meaningful.
Peer Reviewer 2	Clarity and Usability	The type of breast feeding should be clarified throughout to convey accurate reporting of the studies cited.	When possible, we have noted the actual definitions used in the literature.
Peer Reviewer 2	Clarity and Usability	Who is EPC -pg ES-3?	It refers to the review team who conducted the review, or Evidence-based Practice Center. The acronym has been spelled out.
TEP Reviewer 3	Clarity and Usability	The differences in this report from prior AHRQ reports on this topic are not clearly presented highlighted.	We have highlighted how our conclusions differ in the Discussion section.

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 4	Clarity and Usability	Yes, and yes and yes.	Thank you.
Peer Reviewer 4	Clarity and Usability	Great job.	Thank you.
TEP Reviewer 5	Clarity and Usability	Yes, the report is well-organized and clear.	Thank you.
TEP Reviewer 5	Clarity and Usability	Conclusions are relevant, but as noted above, I think more detail needs to be provided on future research. In order to have the necessary variables to address health disparities, multiple SES-related factors need to be collected.	Additional detail has been added to the future research needs section.
Peer Reviewer 6	Clarity and Usability	This report presents very useful information.	Thank you.
Peer Reviewer 6	Clarity and Usability	However, the presentation is highly repetitive, with the same messages repeated 7 times: key messages on page iii, the structured abstract on pages vii-viii, the executive summary, the "key points" sections under each key question, the results themselves, the discussion section starting on page 113, and conclusions on page 125. Summaries are useful, but 7 times is excessive.	We have edited the report to make it more succinct and less repetitive.
Peer Reviewer 6	Clarity and Usability	Overall, the report is overly negative about the status of the literature.	We feel the report provides an unbiased summary of the literature addressing the key questions.

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 6	Clarity and Usability	While it is appropriate for a scientific review to point out the limitations of the studies, much of this is excessive. For example, in the final conclusion section, it is stated that observational studies do not clearly establish the magnitude of benefit of BFHI. This would be an important limitation if the purpose of the review were to conduct a meta-analysis on the overall magnitude of effect of BFHI. But given the variety of study designs, settings, and fidelity of application of the Ten Steps across the studies, difference in magnitude of effect is to be expected.	We have revised the document to reduce repetitiveness, but as a general principle, we disagree that failure to establish the magnitude of benefit matters only if a meta-analysis is being conducted. Information on magnitude helps us interpret the strength of association and the final grade.
Peer Reviewer 6	Clarity and Usability	As stated earlier, the possibility of reverse causality is not a likely issue for many of the maternal health outcomes—the possibility needs to be acknowledged, but the case is overstated.	We feel the possibility of reverse causality is important for certain maternal health outcomes (i.e., weight change and depression) and disagree that this is overstated. As noted in response to the earlier comment, while poor maternal health may not be a major driver of the decision to initiate breastfeeding in earlier historical periods, it is a potential determinant of breastfeeding duration, regardless of secular trends.
Peer Reviewer 6	Clarity and Usability	Similarly, it is not necessary to repeatedly state "low strength of evidence" on every finding. Recognizing limitations and being honest about strength of evidence is fine, but it doesn't need to be repeated over and over again.	We've edited the discussion to make it more succinct and less repetitive.
Peer Reviewer 7	Clarity and Usability	The report is quite well-structured and organized.	Thank you.
Peer Reviewer 7	Clarity and Usability	Including the Brodribb 2013 study would greatly improve this review's relevance to policy.	This study was excluded for ineligible design. Authors analyze results (surveys of women) based on whether they recalled receiving certain hospital practices related to BFHI.
TEP Reviewer 8	Clarity and Usability	The report is well structured and organized.	Thank you.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 8	Clarity and Usability	Most of my questions related to the final grading on the body of evidence for some of the questions. Providing additional clarity on these questions will improve the report.	We have addressed these specific questions regarding the strength of evidence assessment.
TEP Reviewer 8	Clarity and Usability	This is a critical review with important policy and programmatic implications.	Thank you.
TEP Reviewer 9	Clarity and Usability	The report is well organized and has a logical flow to the presentation of information.	Thank you.
TEP Reviewer 9	Clarity and Usability	The key points are described with sufficient clarity to make them accessible to both clinical and non-clinician readers.	Thank you.
TEP Reviewer 9	Clarity and Usability	The updated findings on strength of evidence for breastfeeding's impact on risk for any and specific types of breast cancer and fracture risk are helpful but I do not know if they will impact practice or policy for women of reproductive age as I cannot see clinicians recommending against breastfeeding even if there is low strength of evidence for some health outcomes. Likewise I do not think it will discourage health care provider training to support breastfeeding.	The findings of "low strength of evidence" do not imply that clinicians would recommend against breastfeeding. The conclusion is that breastfeeding is associated with reduced breast cancer risk and is not associated with fracture risk. The revised text in the Discussion and Results makes clear the direction of effect.
TEP Reviewer 9	Clarity and Usability	However, they do present new information on these health outcomes along with the newly added content on BFHI, WIC and workplace interventions to support BF.	Thank you.
TEP Reviewer 1	General Comments	This manuscript is clinically meaningful for breastfeeding policies and practices.	Thank you.
TEP Reviewer 1	General Comments	The population for this report is presented as populations from highly developed countries and the audience is key stakeholders, care givers, health care professionals, researchers and policy makers who work with breastfeeding women and their communities.	This is correct.
TEP Reviewer 1	General Comments	The key questions are appropriate and explicitly stated many times throughout the report.	Thank you.
Peer Reviewer 2	General Comments	Yes.	Thank you.

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 2	General Comments	The use of reporting 'harms' of breastfeeding is not fully understood by the reviewer. The only harm presented is fracture risk. Could this be stated specifically rather than in general as harms. Are there other possible harms? Financial burden on families? Social pressures on mothers to breastfeed?	We have clarified this in the methods; ”harms” differ by KQ.
TEP Reviewer 3	General Comments	1. I would strongly suggest splitting this document into 2 separate reports: (a) Effects of Breastfeeding Programs and Policies on infant feeding in developed countries (b) Maternal Health Outcomes linked to breastfeeding in Developed Countries	This is not feasible due to time and resource constraints.
TEP Reviewer 3	General Comments	2. Need to make clear very early on that this was NOT designed as a comprehensive review of maternal health outcomes related to lactation (as noted p158, lines 21-24). Otherwise, the fact that your search strategy missed research related to other cancers including endometrial cancer [PMID: 26384296] and esophageal cancer [PMID: 26886236] and thyroid cancer [PMID: 28426980] and lung cancer [PMID: 28440542], seems to be a glaring hole in the credibility of this work.	We disagree that the exclusion of some maternal outcomes reduces the credibility of this work. Our aim was to focus on the health outcomes considered most closely associated with breastfeeding. Our methods list the specific outcomes included and the Discussion notes that there are other outcomes that may be associated with breastfeeding (but have more limited evidence).
TEP Reviewer 3	General Comments	Also the lack of mention of the PROBIT maternal health data is concerning (i.e. "Effects of an intervention to promote breastfeeding on maternal adiposity and blood pressure at 11.5 y postpartum: results from the Promotion of Breastfeeding Intervention Trial, a cluster-randomized controlled trial." Oken E, Patel R, Guthrie LB, Vilchuck K, Bogdanovich N, Sergeichick N, Palmer TM, Kramer MS, Martin RM.Am J Clin Nutr. 2013 Oct;98(4):1048-56. doi: 10.3945/ajcn.113.065300. Epub 2013 Aug 14.PMID: 23945719)	This study is not eligible for KQ2 due to ineligible country setting (KQ2 is limited to countries that are categorized as “very high” HDI).

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 3	General Comments	NOTE page numbers indicated here are of the 181 numbered pages which include the preface and Key Messages as part of the larger document 1. Page 2, Key messages: Suggest swapping the order of the 4th and 5th bullets	Thank you for your comment. Our preference is that the bullet on future research is the last bullet because it follows the order of the report.
TEP Reviewer 3	General Comments	2. Page 2, Line 12-reword as a complete sentence	Thank you for your comment. Per AHRQ guidance, the intent of key messages is to convey the purpose and important findings of the review to the reader quickly and concisely.
TEP Reviewer 3	General Comments	3. Page 2, line 17-consider rewording as "maternal risk of cancer (breast and ovarian)..." Need to make clear here that this was NOT a comprehensive review of maternal health outcomes (as noted p158, lines 21-24) as it currently appears to imply that lactation does not protect against endometrial cancer, despite considerable data showing that it does [e.g., PMID: 26384296]	Thank you for your comment. Per AHRQ guidance, the intent of key messages is to convey the purpose and important findings of the review to the reader quickly and concisely. We do not agree that this bullet, as written, implies any conclusion related to breastfeeding and other cancer outcomes.
TEP Reviewer 3	General Comments	4. Page 6, lines 16-25. Be consistent in structure. I.e. Maternal health impacts should either be first or second throughout title and document (although I still strongly prefer the idea of creating 2 separate reports)	This comment appears to refer to the Data sources section of the abstract. We describe the search (for KQ1 then KQ2) and then the study design eligibility (for KQ1 then KQ2). We do not feel that reorganization would be helpful for this section; it is consistent with other sections of the report.
TEP Reviewer 3	General Comments	5. Page 6, line 55 (page vii)-reword “We rated the evidence for association between breastfeeding and fracture low for no benefit”	We have reworded this line as requested.
TEP Reviewer 3	General Comments	6. Page 7, line 1-2-reword “Due to heterogeneity and inconsistent results, ++it remains unclear++ whether breastfeeding is associated with postpartum depression, cardiovascular disease, or postpartum weight change	We have reworded this section, also with the aim of making the abstract more succinct. It now reads: “For maternal health outcomes, low SOE supports the conclusion that ever breastfeeding or breastfeeding for longer durations may be associated with lower rates of breast cancer, epithelial ovarian cancer, hypertension, and type 2 diabetes, but not fractures.”

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 3	General Comments	7. Page 7, lines 13-16—reword "Although low SOE supports the association between breastfeeding and improved health outcomes (breast cancer, ovarian cancer, hypertension, and type 2 diabetes), methodological limitations specific to observational study designs does not establish that there is a causal association between breastfeeding and maternal health." As something like: "The identified associations between breastfeeding and improved maternal health outcomes are not supported by evidence that allows proof of causal relationships."	We have edited this sentence with minor changes. It now reads: "The identified associations between breastfeeding and improved maternal health outcomes are supported by evidence from observational studies, which cannot determine cause and effect relationships ."
TEP Reviewer 3	General Comments	8. Page 11, line 8-delete the words before the semi-colon. The background provided does NOT currently provide a compelling argument for why this work was needed.	The first line of the Introduction has been revised based on other comments. It now reads: "In reproductive physiology, lactation follows pregnancy; evidence supports the association between breastfeeding and better health outcomes for both infants and mothers." We have also added additional text to the Introduction section that provides a rationale for why this work is needed.
TEP Reviewer 3	General Comments	9. Page 11-2nd and 3rd paragraphs can be deleted as they discuss tangents	We disagree that these paragraphs are tangents (2nd and 3rd paragraph of the introduction). These describe current breastfeeding rates in the U.S. and Healthy People goals. Other reviewers have asked for additional details on breastfeeding rates to be added (e.g., rates of exclusive breastfeeding).
TEP Reviewer 3	General Comments	10. Page 15, line 26-35 is hard to follow the numbers of studies (e.g. "of these 128 studies" without prior mention of the number 128	We have clarified the number of studies in the ES and main report. The article flow diagram is also referenced (in an Appendix) for readers who want additional details.
TEP Reviewer 3	General Comments	11. Page 17, Line 27-missing a word "BHI +steps+ implemented"	Thank you. We've added the word "steps" after BFHI as noted.
TEP Reviewer 3	General Comments	12. Page 17, line 48-missing an s "suggest+S+"	We have revised this line as requested.
TEP Reviewer 3	General Comments	13. P22, line 42. KQ 2 It is odd that discussion of HARMS includes no mention of mastitis, which is commonly understood to be associated with breastfeeding.	Our scope does not include complications or expected physical manifestations of lactation (this is noted in our eligibility criteria- mastitis is listed as an excluded outcome in Table 2). KQ2 of this report is an update to a previous AHRQ report on this topic that did not consider mastitis an eligible outcome.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 3	General Comments	More broadly, the fact that your search strategy missed research related to other cancers including endometrial cancer [PMID: 26384296] and esophageal cancer [PMID: 26886236] and thyroid cancer [PMID: 28426980] and lung cancer [PMID: 28440542], seems to be a glaring hole in the credibility of this work unless you clearly state why you didn't look for data on these topics	We disagree that the exclusion of these outcomes is a "glaring hole." The current report is an update to a previous report that only considered breast and ovarian cancer. Our aim was to update the prior report and summarize evidence on outcomes considered most strongly associated with lactation. Therefore, other cancers including endometrial and lung are outside of scope.
TEP Reviewer 3	General Comments	14. Page 24, table G. I'm confused by statements in the 3rd column that findings were consistent, that are then followed by a 4th column statement of inconsistent evidence (eg 1st breast cancer row, 3rd row, ovarian cancer)	This has been revised. The 3rd column and 4th column of the SOE summary for breast cancer note the consistency in the direction of effect.
TEP Reviewer 3	General Comments	15. Page 35-Background-first sentence should be edited as suggested above (i.e., delete the words before the semi-colon)	This has been revised. The first paragraph now reads: "In reproductive physiology, lactation follows pregnancy; evidence supports the association between breastfeeding and better health outcomes for both infants and mothers
TEP Reviewer 3	General Comments	16. Page 44, line 52-extra "to" "We paid close ++ attention to"	This line has been revised.
TEP Reviewer 3	General Comments	17. Page 44, Lines 53-56—clarify what breastfeeding rates you mean [I presume breastfeeding initiation]	We edited this sentence to insert the word "initiation"; however, in some parts of the review we use the more general term "breastfeeding rates" when referring to rates of initiation and duration more broadly.
TEP Reviewer 3	General Comments	18. Page 46-numbers are easier to follow than page 15, but do not match page 15	The literature search numbers in the ES have been revised to match those in the Results chapter.
TEP Reviewer 3	General Comments	19. Page 85, line 32—extra "and"	This Key point has been revised.
TEP Reviewer 3	General Comments	20. Page 86 (page 51) line 53-missing an "a"	This line has been revised.
TEP Reviewer 3	General Comments	21. Page 86- consider specifically looking at data regarding lactation and visceral adiposity in addition to simply obesity	Thank you for your comment. The current report is an update to a previous report that did not consider visceral adiposity (or other intermediate metabolic outcomes) eligible. Therefore, the current report did not include data regarding lactation and visceral adiposity because it was outside of scope.
TEP Reviewer 3	General Comments	22. Page 97, line 32 has an extra "s"	This section has been edited based on an update literature search; there is no longer a typo in this sentence.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 3	General Comments	23. Page 114, line 3-first word should be confound+ers+	Thank you. This typo was corrected.
TEP Reviewer 3	General Comments	24. P120, line 23- typo “vs. never” appears twice	Thank you. This typo has been corrected.
TEP Reviewer 3	General Comments	25. P123, line 8 “women” should be qualified as “postpartum women”	We have added “postpartum” to this sentence for clarity.
TEP Reviewer 3	General Comments	26. page 146, line 16 “weight-height index” is more standardly referred to as “body mass index”	In this instance, we are using the terminology of the measure as it was reported in the study.
TEP Reviewer 3	General Comments	27. p147 and on-Discussion, currently reads more like a restatement of the results than a true discussion of the findings in the context of what was already known prior to this review	The Discussion section has been revised extensively, both to shorten the summary of results and to add more discussion of the implications of our findings.
TEP Reviewer 3	General Comments	28. p156, line 33-need to discuss the ethical challenges that may preclude randomized trials related to infant feeding	We have added additional details here, but do not go into an in-depth discussion regarding the ethical challenges of randomized trials of breastfeeding.
TEP Reviewer 3	General Comments	29. p158, line 10-missing the word “to” , i.e. should read “For KQ2, we chose +to+ include recent”	This line has been revised.
TEP Reviewer 3	General Comments	30. p158, lines 21-24 are critically important and need to be stated in the introduction	Thank you. We note that these are excluded outcomes in the Methods section but do not feel that it is necessary to list exclude outcomes in the Introduction section.
TEP Reviewer 3	General Comments	31. page 158, line 55-whether early weaning causes maternal risk of CVD or is a marker of maternal risk of CVD, women with this history should be offered interventions which have been shown to reduce risk of CVD	We do not feel that the results of our review indicate that early weaning is a factor that should determine whether women are offered interventions that have been shown to reduce risk of CVD (beyond other known factors such as family history, smoking, diabetes, etc.).
TEP Reviewer 3	General Comments	32. p159, line 25-be more explicit about which confounders should standardly be controlled for	We have added additional text noting which factors future studies should consider.
TEP Reviewer 3	General Comments	33. page 159, line 39-consider mentioning support for this hypothesis from animal models [see PMID: 24905416]	We have expanded this section but have not cited evidence from animal models.
Peer Reviewer 4	General Comments	Overall, the report is very well written.	Thank you.

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 4	General Comments	The report is clinically meaningful, the target population and audience are explicitly defined and the key questions are appropriate and explicitly stated.	Thank you.
Peer Reviewer 4	General Comments	With that being said, there are minor considerations that will assist readers in navigating through the report. These are itemized below. 1. Is it possible to publish as two reports? Key question 1 is very different from key question 2 and the audiences may be very different.	No, it is not possible to publish two reports.
Peer Reviewer 4	General Comments	2. The tables will benefit from more thorough footnoting of format. Every table should have its own set of footnotes below it for anytime an abbreviation is used. Also, every table should footnote an explanation of how the column heading "study limitations" links to the entry options of Low, etc.	The table formatting has been reviewed. All tables have abbreviations.
Peer Reviewer 4	General Comments	3. I appreciate the reference to the Strength of Evidence alogrithm, but I would prefer more detail on how this committee applied the guidelines. Perhaps a table that shows when "Low" versus "Moderate" etc. are used. This table can then be referenced as a footnote to each table with the SOE column heading.	We have a full appendix that gives more detail about our SOE conclusions. The ES and discussion are meant to be a summary.
Peer Reviewer 4	General Comments	4. Regarding SOE, is it possible to enter low-moderate? For some outcomes, particular breast cancer, the evidence is consistently showing benefit of lactation.	No. We use only Low, Moderate or High.
Peer Reviewer 4	General Comments	For some outcomes, particular breast cancer, the evidence is consistently showing benefit of lactation.	We have revised the SOE for breast cancer to note consistency in the direction of effect.

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 4	General Comments	It seems like the bucket for Low SOE is very broad.	This literature (which is all observational) is associated with significant limitations (e.g., confounding) which makes it difficult to come to more certainty in the findings. We emphasize this in the Discussion section.
TEP Reviewer 5	General Comments	Yes, the report is meaningful and provides an excellent update to the Ip et al. review.	Thank you.
Peer Reviewer 6	General Comments	This is a useful literature review on two quite distinct issues on breastfeeding.	We agree.
Peer Reviewer 6	General Comments	The two key questions are so distinct that it seems strange to combine them into a single report.	We are considering two separate journal manuscripts for each KQ.
Peer Reviewer 6	General Comments	If at all possible, I would recommend rewriting this as two separate reports.	This is not possible.
Peer Reviewer 6	General Comments	For key question 1, the grouping of interventions into 4 categories (BFHI, other health-care, WIC, and community-based interventions) doesn't really make sense given the heterogeneity within several of these groups. In general, the interventions grouped under WIC represent more of a population group than an intervention type.	We agree there is heterogeneity within categories, however, we feel this categorization is potentially helpful to decision-makers across different settings.
Peer Reviewer 6	General Comments	It would be much more logical to create a category for peer counselling interventions (that would include Lovera 2010, Reeder 2014, Schafer 1998, Shaw 1999, and Morrow 1999) since it doesn't matter who sponsored the peer counselling programme.	This was not our scope; we exclude some peer counseling interventions delivered as part of routine primary care.
Peer Reviewer 6	General Comments	Similarly, it is essentially irrelevant that the breast pump study by Hayes (2008) was done in a WIC setting so it should not be grouped with other WIC interventions.	We disagree. Our aim was to describe interventions delivered in WIC settings.

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 6	General Comments	Just as the "WIC interventions"category includes a variety of different intervention types, there are several dissimilar types of interventions lumped together under "other health care" interventions. It doesn't matter that 9 "other health care" interventions report initiation outcomes and 8 report duration outcomes when there are several types of interventions within this category.	We agree and acknowledge the heterogeneity of intervention types within (and across categories).
Peer Reviewer 6	General Comments	It is confusing to read about initiation outcomes for training-based interventions, then training with expanded services, then service delivery changes followed by duration outcomes for training-based interventions, then training with expanded services, then service delivery changes.	This section has been edited for clarity. We feel this is helpful for decision-makers at a health care systems level.
Peer Reviewer 6	General Comments	The paper would be easier to follow if health care provider training interventions were handled as a category, and "expanded health-care services" were a category (including the MacLachlan 2016 study since the intervention was carried out by an MCH nurse.)	We disagree. Within the broader category, we do describe these sets of studies separately and make separate SOE grades.
Peer Reviewer 6	General Comments	The criteria for inclusion and exclusion regarding KQ1 are not clear.	We have clarified the criteria based on specific comments.
Peer Reviewer 6	General Comments	There is a significant body of literature on the effectiveness and harms of programs and policies that has been excluded.	We note that this review excludes interventions delivered as part of routine primary care (i.e., those included in the recent review to support the USPSTF recommendation on breastfeeding support interventions).
Peer Reviewer 6	General Comments	The review only considers exposure to programs or policies from the perspective of who actually provides such programs or changes policies. It ignores all of the literature that assesses exposure from the perspective of the mother.	We disagree. All measures of breastfeeding were self-reported by mothers. Our goal for KQ1 was to evaluate healthcare system and community interventions.
Peer Reviewer 6	General Comments	Data sources that ask the mother about her exposure to programs, such as the Infant Feeding Practices Study, the Pregnancy Risk Assessment Monitoring System, or NHANES are cited for KQ2, but not KQ1.	This is not true. When an otherwise eligible study for KQ2 uses a population data source for breastfeeding outcomes, we have included such studies (e.g., the study assessing the WIC food policy change).

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 6	General Comments	For example, a number of studies have examined the impact of the BFHI by asking mothers about whether they experienced the Ten Steps to Successful Breastfeeding and examined the associated breastfeeding outcomes, but these studies are not mentioned.	We excluded retrospective cohort studies that surveyed mothers about their breastfeeding hospital care after discharge.
Peer Reviewer 6	General Comments	Such studies should be added to the review (preferred option) or else text should be added on why these studies were excluded.	Specific reasons for why studies have been excluded are noted in the appendices. Our methods outline inclusion criteria for KQ1.
Peer Reviewer 6	General Comments	There are other studies that have been excluded for reasons that are not clear. For example, on page ES-15, it is stated that the 2012 Cochrane review found no RCTs or controlled trials investigating the effect of workplace interventions for promoting breastfeeding in employed women and yet reference 32 for Cohen et al clearly describes the outcomes of a workplace intervention. The Cohen article is not a controlled trial, but there is nothing in the methods section that states that only controlled trials would be reviewed.	We have reviewed this article and it is not an eligible study design.
Peer Reviewer 6	General Comments	The WIC food package study was clearly not a controlled trial but it is included.	Pre- post studies with repeat measures of breastfeeding before and after the intervention are eligible. This is stated in our methods and in Table 2 (eligibility criteria).
Peer Reviewer 7	General Comments	This report is clinically meaningful, and the audience is clearly defined.	Thank you.

Commentator & Affiliation	Section	Comment	Response
Peer Reviewer 7	General Comments	Key question 1 may be lacking some clarity. As currently phrased, KQ1 is: "What are the effectiveness and harms of programs and policies on initiation, duration, and exclusivity of breastfeeding?" However, the rest of the report assesses only maternal harms and does not consider harms to infants. Therefore, I would recommend rephrasing KQ1 as "What are the effectiveness and harms for women of programs and policies on initiation, duration, and exclusivity of breastfeeding?" If revised in this way, the report could then clarify the target population as women only, not including infants.	At this stage in the review, we will not change our key questions. The eligibility criteria makes clear the intended population and harms. If an eligible study reported infant harms related to an intervention, we would have described those.
Peer Reviewer 7	General Comments	Alternatively, the Analytic Framework could be revised so that the leftmost text read "Mother-infant dyads" and the portion on "Adverse Effects of the Intervention" included adverse infant outcomes (e.g. neonatal hyperbilirubinemia.)	We are not able to change our key questions or analytic framework at this stage in the review. If an eligible study would have reported harms (specific to an intervention) we would have described those.
Peer Reviewer 7	General Comments	For Key Question 1b, an important subpopulation of women to consider is populations with low expected rates of breastfeeding (i.e., women giving birth in communities with low breastfeeding rates), and also populations with high expected rates of breastfeeding (i.e. women giving birth in communities with high breastfeeding rates)	We are not able to add additional subgroups of women at this stage in the review process.
TEP Reviewer 8	General Comments	This is a critically important review.	Thank you.
TEP Reviewer 8	General Comments	The key questions are appropriate and the review of the evidence is timely.	We agree.
TEP Reviewer 9	General Comments	The report is well done.	Thank you.
TEP Reviewer 9	General Comments	It clearly outlines the rationale for the review, describes what has been done in the previous review and identifies the concepts that have been updated or newly included in this review.	Thank you.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 9	General Comments	The key questions are generally well stated. However, without the provided examples in parenthesis I would not have understood the intent of question 1C.	Thank you. We believe the examples, and explanation in the text, makes the intent of KQ 1c clear.
TEP Reviewer 9	General Comments	The methods have sufficient detail to adequately assess the completeness of the search.	Thank you.
TEP Reviewer 9	General Comments	I believe the findings may spur efforts to improve the methodological strength of future studies to improve the strength of evidence for the outcomes of interest.	Thank you. We agree.
Kandice A. Kapinos, Ph.D. RAND Corporation Public Reviewer 2	General Comments	Dear Dr. Iyer, I am writing to provide comments on the review: https://effectivehealthcare.ahrq.gov/sites/default/files/pdf/breastfeeding-draft-report.pdf In particular,I have noted the exclusion of numerous studies on healthcare system-based interventions. It is unclear why these studies have been excluded, but these are studies on "changes in health service delivery or policies that relate to breastfeeding" (pg 25 of Chapter 3) that were not included.	We address specific comments on studies below. Most were excluded due to other reasons (e.g., no comparison group, ineligible study design) and not because we didn't consider the intervention to be "healthcare system-based."
Kandice A. Kapinos, Ph.D. RAND Corporation Public Reviewer 2	General Comments	First, there is a body of work that looks at maternity leave policies and their effect on breastfeeding – this may be excluded because it is not necessarily a "healthcare system-based" change, but then maybe these would be considered community based? At any rate, it seems that a review on breastfeeding policies in developed countries would not exclude evaluations of maternity leave policies and programs.	We have reviewed these citations to ensure that they were assessed for relevance. We did not exclude evaluations of maternity leave policies, but found none that met our inclusion criteria.
Kandice A. Kapinos, Ph.D. RAND Corporation Public Reviewer 2	General Comments	Without having a full list of studies, I suggest starting with the following: Roe, B., et al. (1999). "Is there competition between breast-feeding and maternal employment?" Demography 36(2): 157-171.	This study has been reviewed and was determined to be ineligible due to wrong study design. The study is a retrospective cohort study and does not evaluate a workplace intervention. Authors look at the associations between length of maternity leave and breastfeeding duration using administrative data from a population cohort, but there is no specific change in breastfeeding policy or workplace intervention being studied.

Commentator & Affiliation	Section	Comment	Response
Kandice A. Kapinos, Ph.D. RAND Corporation Public Reviewer 2	General Comments	Chatterji, P. and K. D. Frick (2005). "Does returning to work after childbirth affect breastfeeding practices?" Review of Economics of the Household 3(3): 315-335.	This study has been reviewed and was determined to be ineligible due to wrong study design. The study examines the effect of the timing and intensity of returning to work after childbirth on the probability of initiating breastfeeding and the number of weeks of breastfeeding. Data come from the National Longitudinal Survey of Youth. There is no change in workplace policy or a new intervention being studied. Women who chose to breastfeed may have taken a longer leave from work.
Kandice A. Kapinos, Ph.D. RAND Corporation Public Reviewer 2	General Comments	Baker, M. and K. Milligan (2008). "Maternal employment, breastfeeding, and health: Evidence from maternity leave mandates." Journal of health economics 27(4): 871-887.	This study has been reviewed and was determined to be ineligible due to wrong study design and wrong comparator. The study estimates rates of breastfeeding duration based on a large population cohort pre- and post- changes in Canadian maternity leave mandates. There is only one pre- and one post- estimate in breastfeeding outcomes. Our criteria state that studies using a pre-post comparator (and no concurrent control group) must report multiple pre- and post- outcome measures.
Kandice A. Kapinos, Ph.D. RAND Corporation Public Reviewer 2	General Comments	Second, there is a small body of work that has examined state laws in the U.S. related to breastfeeding accommodations in the workplace - and relatedly, there were changes to the ACA that mandated all employers with more than 50 employees provide adequate time and space.	We did not identify eligible studies reporting on how these mandates affected rates of breastfeeding initiation or duration.
Kandice A. Kapinos, Ph.D. RAND Corporation Public Reviewer 2	General Comments	Perhaps there are similar international studies, but here are a few studies on this in the U.S., though again, this is not an exhaustive list: Hawkins, S. S., et al. (2013). "Do state breastfeeding laws in the USA promote breast feeding?"Journal of epidemiology and community health 67(3): 250-256.	We have reviewed these citations and ensure that they were assessed for relevance.

Commentator & Affiliation	Section	Comment	Response
Kandice A. Kapinos, Ph.D. RAND Corporation Public Reviewer 2	General Comments	Slusser, Wendelin M., Linda Lange, Victoria Dickson, Catherine Hawkes and Rona Cohen. 2004."Breast Milk Expression in the Workplace: A Look at Frequency and Time." Journal of Human Lactation, 20(2), 164-69.	This study has been reviewed and was determined to be ineligible due to wrong comparator. This study has an ineligible comparator (no control group), and it does not assess the benefit of a specific workplace intervention on improving rates of breastfeeding. The aim is to evaluate barriers to breastfeeding (primarily factors related to breast milk expression) among women working full-time at one corporation providing employee benefits. No specific intervention/policy is being evaluated.
Kandice A. Kapinos, Ph.D. RAND Corporation Public Reviewer 2	General Comments	Given that 75% of women of childbearing age are in the workforce, it seems like reviewing these studies is again highly relevant for this review.	We have reviewed the citations and explained why they were not included in this review in Appendix XXX.
Kandice A. Kapinos, Ph.D. RAND Corporation Public Reviewer 2	General Comments	There also seems to be no discussion of studies on access to lactation consultants, which is a huge problem, at least in the U.S. The ACA has mandated coverage of those services (see my studies below), but there is also a supply-side issue in that there are shortages of this expertise so many women might have difficulty getting these services from a qualified consultant even if cost was not an issue.	We did not find eligible studies that reported outcomes based on access (or no access) to lactation consultants.
Kandice A. Kapinos, Ph.D. RAND Corporation Public Reviewer 2	General Comments	Kapinos, K. A., et al. (2017). "Lactation Support Services and Breastfeeding Initiation: Evidence from the Affordable Care Act." Health Services Research 52(6): 2175-2196. Gurley-Calvez, T., et al. (forthcoming). "Effect of the Affordable Care Act on Breastfeeding Outcomes." American Journal of Public Health.	These studies were published after our updated literature search and were not incorporated into the final report.

Commentator & Affiliation	Section	Comment	Response
Kandice A. Kapinos, Ph.D. RAND Corporation Public Reviewer 2	General Comments	Finally, there seems to be little discussion of studies on home visiting programs, which in the U.S. have been funded extensively in recent years. (see https://mchb.hrsa.gov/maternal-child-healthinitiatives/ home-visiting-overview). I believe there are studies that have evaluated these programs and would be surprised if none of them examined effects on breastfeeding. In this review, I see a few studies discussed in conjunction with other interventions (e.g. BFHI AND home visits or WIC interventions), but I am guessing there is a larger literature on this. Here is one (older) review on home visiting that looks at breastfeeding as an outcome, for example: https://www.journalslibrary.nihr.ac.uk/hta/hta4130#/abstract	The scope of our review did not include all studies of home-visiting programs. We excluded studies assessing breastfeeding interventions delivered as part of routine primary care.
Kandice A. Kapinos, Ph.D. RAND Corporation Public Reviewer 2	General Comments	Thanks for considering my suggestions. This is a much-needed review and I look forward to seeing the final version. Best, Kandice Kapinos	Thank you for these comments.
TEP Reviewer 1	Quality of the Report	Superior	Thank you.
Peer Reviewer 2	Quality of the Report	Superior	Thank you.
TEP Reviewer 3	Quality of the Report	Good	Thank you.
Peer Reviewer 4	Quality of the Report	Superior	Thank you.
TEP Reviewer 5	Quality of the Report	Superior	Thank you.
Peer Reviewer 6	Quality of the Report	Superior	Thank you.
Peer Reviewer 7	Quality of the Report	Good	Thank you.
TEP Reviewer 8	Quality of the Report	Superior	Thank you.

Commentator & Affiliation	Section	Comment	Response
TEP Reviewer 9	Quality of the Report	Superior	Thank you.
Maya Bunik Univ of CO, Children's Hospital Colorado Public Reviewer 1	Quality of the Report	Good	Thank you.