PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (**http://bmjopen.bmj.com/site/about/resources/checklist.pdf**) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Intimate Partner Violence and Breastfeeding: A Systematic Review
AUTHORS	Normann, Anne Katrine; Bakiewicz, Aleksandra; Kjerulff Madsen, Frederikke; Khan, Khalid; Rasch, Vibeke; Linde, Ditte

VERSION 1 – REVIEW

REVIEWER	Jennifer Yourkavitch University of North Carolina at Greensboro
REVIEW RETURNED	21-Oct-2019

GENERAL COMMENTS	Abstract: causal language is used to describe the findings from studies--is that appropriate? You should revise to indicate "association with" rather than "effect on" as needed. Outcomes: when you say "IPV shortened duration," what is the comparison? Shorter duration compared to guidelines or compared to other people in the study? P. 5, line 52: DAG is "directed acyclic graph." P.6, line 26: Miller-Graff needs a citation. Results: Data in some tables are not clearly presented. You could remove the "N/A" rows from tables S.2.3, S.2.4, S.2.5. Please add outcome to Tables 1 and S.2.1. Table S.2.4.: was no time period specified for Miller-Graff? Table S.2.3.: Was Holland's time period pre/during/and after pregnancy? Figure 1: How did you get from 1634 to 49? Full-text articles excluded n should be 33. Figure 3 is nice! Discussion: there's no discussion of reasons why/how IPV could affect breastfeeding. This section is written mainly as a recount of the review mechanics. Depression is mentioned but not explored fully. Does IPV affect maternal/infant bonding? Does partner not support breastfeeding, or is controlling of the mother-infant relationship? What are other possible mechanisms/pathways? Please review for English grammar and punctuation.

REVIEWER	Helen E Morgan Cardiff University UK I have no conflict of interest regarding the publication of this article.
REVIEW RETURNED	24-Feb-2020

GENERAL COMMENTS	I have only reviewed the methodology used in this manuscript as requested by BMJ Open.

	Question 5: Please provide further details of the quality assessment process, was assessment of each study conducted by one author or 2 authors independently in duplicate? Question 10: it would be useful if table 1 included a column regarding methodological concerns of each study and figure 3 indicated study design. Question 2 and 11: In the discussion line 54/55 the authors state 'Our meticulous quality assessment judged the majority of studies included as being good quality' however only one cohort study was assessed as being of good quality the rest were cross-sectional, I believe the statement to be misleading as it fails to discuss study design. Therefore the results section of the abstract should also be adjusted to acknowledge study design, also the abstract conclusions are misleading in terms of the study quality and design and do not match the main conclusions of the manuscript. Question 13: Authors have not completed all parts of checklist even if N/A best to state this. Please can the authors check the numbers of the study selection flow diagram, total number of records should be 2062 not 2065. Also it appears that the number of records screened at title/abstract are missing, jumps from 'removing duplicates' to 'Full text'. Also article either missing from excluded or included,32 + 16 =48 not 49. Page 8, line 30/31 'Future research should focus on longitudinal studies in with' - remove 'in'

REVIEWER	Joanna Leaviss University of Sheffield, UK
REVIEW RETURNED	09-Mar-2020

GENERAL COMMENTS	The authors have conducted a systematic review exploring the association between intimate partner violence (IPV) and breastfeeding outcomes. The review places itself in context with a recent similar (2018) review, attempting to build on this by a more thorough exploration of the potential confounding factors and a different method of quality assessment. The abstract does not clearly summarise the methods and findings of the review. The conclusions are stated to be based on well-controlled results from quality studies, however the results section of the abstract reports that most studies finding associations between IPV and breastfeeding outcomes were of fair to low quality. Greater clarity of reporting is needed, in particular the review does not methodologically distinguish itself sufficiently from the previous review. For example the authors mention that the previous review does not conduct a 'proper' quality assessment - presumably this is because they adapt the STROBE risk of bias instrument compared to use of the validated NOS, however the relative strengths and benefits of each method are not discussed. There is insufficient detail of the inclusion/exclusion criteria for the reader to fully understand the differences between the number and nature of the studies included in each review. The authors report almost double the amount of included participants in their review. These numbers are driven by two very large scale studies - Silverman and Wallenborn. Only Silverman is included in the previous review.

	Both these studies use data from PRAMS. These studies contribute over 200,000 participants however the potential importance of this is not made clear. How are these two studies related to one another? Is there a chance of cross-over in the data? How do the two studies have different quality ratings if they are from the same study? The authors have produced a Forest Plot for illustrative purposes, however they do not illustrate the weight of the studies. Looking at these, most of the studies (using adjusted ORs) appear to show insignificant associations, in particular for breastfeeding duration, however the conclusions state the majority of studies indicate IPV is associated with impaired breastfeeding. The authors have gone to great efforts to extract information on confounders however the narrative in the results section does not go far enough in synthesising this data. It would be interesting to focus on the pattern of results found in the two very large scale studies and how they compare with the smaller studies, and the potential sources of these differences. The discussion introduces some themes such as childhood abuse, but this has not been outlined in the results section. A more in-depth focus on confounders would set the review apart from the previous review. Overall the review would benefit from greater clarity in reporting of methods, with a focus on the differences between the two reviews. If the biggest difference is the extraction of the data on confounders, this should be the key narrative running throughout the paper.

VERSION 1 – AUTHOR RESPONSE

Reviewer: Jennifer Yourkavitch	
Comment	**Response**
Abstract: causal language is used to describe the findings from studies--is that appropriate? You should revise to indicate "association with" rather than "effect on" as needed.	Thank you for this valid comment. We have changed the wording of the abstract to that it now states: *'IPV exposure appears to associate negatively with some breastfeeding outcomes. Individual patient data meta-analysis is required to quantify the magnitude of the association for specific IPV-outcome combinations. More high-quality studies and definition of core confounders is warranted.'* (ll. 73-75)
Outcomes: when you say "IPV shortened duration," what is the comparison? Shorter duration compared to guidelines or compared to other people in the study?	Thank you for this question. We agree that the previous phrasing was unclear. We have now specified that we mean ¨compared to other participants in the study' as each study set their own limit for duration of breastfeeding.

	Table S2.3 now has a foot note, where the duration of BF for each study is mentioned (please see the supplementary file document) Further, we have specified our result section so that it now states: '*The definition of ¨shortened duration of breastfeeding¨ differed as each study sat their own time limit (Table S2.3).'* (ll. 191-192)
P. 5, line 52: DAG is "directed acyclic graph."	Thank you for pointing this error. We have corrected the spelling mistake (l. 200)
P.6, line 26: Miller-Graff needs a citation.	Thank you for pointing this error. We have inserted the citation in the manuscript (l. 216)
Results: Data in some tables are not clearly presented. You could remove the "N/A" rows from tables S.2.3, S.2.4, S.2.5.	Thank you for this suggestion. We have removed N/A from table S2.3, S2.4, S2.5 (please see the supplementary file document)
Please add outcome to Tables 1 and S.2.1.	Thank you for this suggestion. We have changed table 1 (l. 518) and S2.1 accordingly (please see the supplementary file document)
Table S.2.4.: was no time period specified for Miller-Graff?	Thank you for this comment. In the article of Miller-Graff they stated that they asked participants about IPV for the past year. Participants were first interviewed during pregnancy and again approximately 6 weeks postpartum. Results do not distinguish between violence before and after pregnancy and therefore we choose to only make one line in the table since the result is for both periods. In order to clarify this element, we have added a note to table S2.4 marked with an A under the table: '[A] *Participants were interview during pregnancy and again approximately 6 weeks postpartum. Results don't distinguish between violence before and after pregnancy'* (please see the supplementary file document)
Table S.2.3.: Was Holland's time period pre/during/and after pregnancy?	Thank you for this comment. In the article the author state that mailing occurred at approximately 5-7 months postpartum and included items about the prepregnancy, prenatal and postpartum period. There has been added a note to table S5.3 in the revised document: *'Interview of participants included items about the prepgrancy, prenatal*

	and postpartum period' (please see the supplementary file document)
Figure 1: How did you get from 1634 to 49? Full-text articles excluded n should be 33.	Thank you for pointing out this error, which was also raised by the second reviewer. By mistake we had uploaded a previous version of the figure, which did not specify all steps in data selection process. We apologise for this error. We have now uploaded an updated version of the PRISMA figure, which contains all the information (Fig. 1, l. 523)
Figure 3 is nice!	Thank you so much for your support, we highly appreciate it.
Discussion: there's no discussion of reasons why/how IPV could affect breastfeeding. This section is written mainly as a recount of the review mechanics. Depression is mentioned but not explored fully. Does IPV affect maternal/infant bonding? Does partner not support breastfeeding, or is controlling of the mother-infant relationship? What are other possible mechanisms/pathways?	Thank you for this valid suggestion. We have added plausible pathways between IPV and breastfeeding. It now states: *'Mediational models exploring childhood abuse and the negative association with breastfeeding have found it to stem from shame and the reaction to touch, in the postnatal period, which can lead to possible re-traumatization (10).'* (ll. 286-287) And further: *'Sorbo et. al (34) concluded, that depression could not explain early cessation of breastfeeding, whilst other studies (43, 44), found that depression had a negative impact on breastfeeding duration in women suffering from depression. The mechanism between breastfeeding and depression is poorly understood, but research of failed lactation and perinatal depression theorise that it may be the manifestation of neuroendocrine perturbations in gonadal and lactogenic hormones (45)'* (ll. 307-311)
Please review for English grammar and punctuation.	Thank you for this comment. We have reviewed the manuscript for English grammar and punctuation.
Reviewer: Helen E Morgan	
Question 5: Please provide further details of the quality assessment process, was assessment of each study conducted by one author or 2 authors independently in duplicate?	Thank you for this comment. We have added the following elaboration to the quality assessment part of the revised manuscript: *Two authors (AKN and FMK) conducted the quality assessment*

	independently and compared results. Disagreement were solved through discussion. (ll. 157-158)
Question 10: it would be useful if table 1 included a column regarding methodological concerns of each study and figure 3 indicated study design.	We appreciate your suggestions. We have changed Table 1 accordingly, adding a column concerning study quality (l. 518) Your suggestion of adding adding study design to figure 3 would add some value to te figure, but we fear it may also add complexity and it less intuitive. The study quality is included in the figure and studies are ordered according to quality. To emphasise your point, we have added in the figure title: '*ordered according to descending quality'* (l. 540)
Question 2 and 11: In the discussion line 54/55 the authors state 'Our meticulous quality assessment judged the majority of studies included as being good quality' however only one cohort study was assessed as being of good quality the rest were cross-sectional, I believe the statement to be misleading as it fails to discuss study design. Therefore the results section of the abstract should also be adjusted to acknowledge study design, also the abstract conclusions are misleading in terms of the study quality and design and do not match the main conclusions of the manuscript.	Thank you for this comment, which was also raised by the other reviewers. We agree that the statement could be seen misleading and has changed conclusion and discussion accordingly. The abstract now sates: ***Results:*** *A total of 16 studies (participants n= 414,393) were included and they adjusted for a total of 48 different confounders. The majority of studies were cross-sectional (n= 11) and most studies were judged to be fair/low quality. Four out of seven studies found that IPV exposure shortened breastfeeding duration (aORs= 0,22 (95 % CI: 0,05-0,85), 1,18 (95 % CI: 1,01-1,37), 5,92 (95 % CI: 1,72-27,98), 1,28 (95 % CI: 1,18-1,39)) Further, 5/10 studies found that IPV led to early termination of exclusive breastfeeding (aORs= 1,53 (95 % CI: 1,01-23,1), 0,83 (95 % CI: 0,71-0,96), 1,35 (95 % CI:1,07-1,71), 0,17 (95 % CI: 0,07-0,4), 1,839 (95 % CI: 1,61-2,911)) and 2/6 studies found that IPV significantly reduced breastfeeding initiation (aOR= 2,00 (95% CI: 1,2-3,3), 0,81 (95% CI: 0,7-0,93)).* (ll. 64-71) ***Conclusion****: IPV exposure appears to associate negatively with some breastfeeding outcomes. Individual patient data meta-analysis is required to quantify the magnitude of the association for specific IPV-outcome combinations. More high-quality studies and definition of core confounders is warranted.* (ll. 73-75)

	The discussion now states: *This systematic review summarised the most recent evidence, between exposure to IPV and breastfeeding practices. A total of 16 studies were included of which 11 were cross-sectional and five were cohort studies. Forty-eight different confounders were controlled for in the studies. Only one cohort was judged as being of good quality, hence, the overall quality of the studies was fair to low. The majority of studies found that exposure to IPV in any form and at any stage had a significant negative association with breastfeeding duration, early termination of exclusive breastfeeding, but it did not reduce initiation.* (ll. 242-247)
Question 13: Authors have not completed all parts of checklist even if N/A best to state this. Please can the authors check the numbers of the study selection flow diagram, total number of records should be 2062 not 2065. Also it appears that the number of records screened at title/abstract are missing, jumps from 'removing duplicates' to 'Full text'. Also article either missing from excluded or included,32 + 16 =48 not 49.	Thank you raising this point. The issue with the PRIMSA flow chart was also raised by the first reviewer. By mistake we had uploaded a previous version of the figure, which did not specify all steps in data selection process. We apologise for this error. We have now uploaded an updated version of the PRISMA figure, which contains all the information (Fig. 1, l. 523) Further, we have ensured that there are no empty spaces in the checklist and specified N/A when relevant.
Page 8, line 30/31 'Future research should focus on longitudinal studies in with' - remove 'in'	Thank you for pointing out this spelling mistake. We have changed the phrasing according to other reviewers' comments, so the manuscript now states: *'… future research should aim to define core outcome measures and include longitudinal studies of high quality with pre-defined confounders.'* (ll. 315-320)
Reviewer: Joanna Leaviss	
The authors have conducted a systematic review exploring the association between intimate partner violence (IPV) and	Thank you for this reflection and summary of our study, and we appreciate your valid comment, which was also raised by the other

breastfeeding outcomes. The review places itself in context with a recent similar (2018) review, attempting to build on this by a more thorough exploration of the potential confounding factors and a different method of quality assessment. The abstract does not clearly summarise the methods and findings of the review. The conclusions are stated to be based on well-controlled results from quality studies, however the results section of the abstract reports that most studies finding associations between IPV and breastfeeding outcomes were of fair to low quality.	reviewers. We agree that it is an important finding that needs to be highlighted in the conclusion. The conclusion in the abstract now states: ***'Conclusion****: IPV exposure appears to associate negatively with some breastfeeding outcomes. Individual patient data meta-analysis is required to quantify the magnitude of the association for specific IPV-outcome combinations. More high-quality studies and definition of core confounders is warranted.'* (ll. 73-75)
Greater clarity of reporting is needed, in particular the review does not methodologically distinguish itself sufficiently from the previous review. For example the authors mention that the previous review does not conduct a 'proper' quality assessment - presumably this is because they adapt the STROBE risk of bias instrument compared to use of the validated NOS, however the relative strengths and benefits of each method are not discussed. There is insufficient detail of the inclusion/exclusion criteria for the reader to fully understand the differences between the number and nature of the studies included in each review. The authors report almost double the amount of included participants in their review. These numbers are driven by two very large scale studies - Silverman and Wallenborn. Only Silverman is included in the previous review. Both these studies use data from PRAMS. These studies contribute over 200,000 participants however the potential importance of this is not made clear. How are these two studies related to one another? Is there a chance of cross-over in the data? How do the two studies have different quality ratings if they are from the same study?	Thank you for this valuable comment. We agree that it is important to emphasise how this review distinguishes itself from other previous reviews, hence we have changed the discussion of strengths and limitations so that it now addresses the methodologically differences. The section now states: *'…whilst Mezzavilla et al. (13) used STROBE to asses quality through bias susceptibility of included studies. However, STROBE is not a proper quality assessment tool as this is a reporting guideline for observational studies (35, 36), hence, the quality assessment conducted in this review is more meticulous. Yet, a limitation of NOS is that the quality assessors need to adapt the scale to specific research designs, which can lead to the possibility of low agreement between quality assessors (37, 38). Nevertheless, as our quality assessment was conducted by two independent reviewers, we judged this issue to be minor. Further, the two versions of the NOS scale do not consider that cohort studies are superior to cross-sectional studies in the evidence hierarchy, hence, this is a separate parameter to take into consideration when judging the overall quality of evidence according to NOS'* (ll. 264-272) Regarding inclusion/exclusion criteria we have concretized the section of eligibility criteria, so that it now states: *'Eligible studies were original publications that reported exposure to IPV and breastfeeding practices in according with*

	WHO's recommendations and exposure to IPV. We included studies with women exposed to violence one year prior to pregnancy, during pregnancy, and in the postpartum period and excluded studies of women who had experienced childhood abuse. Further, we excluded studies of violence perpetrated by women against men.' (ll.130-134) This is also discussed in 'Interpretation of findings', so that it now states: *In contrast to Mezzavilla et. al, they also reported significant results from studies investigating women exposed to lifetime history of IPV. This may indicate that exposure to any time of violence may affect breastfeeding patterns.'* (ll. 280-282) Further, we changed strength and limitations, which now states: *Yet one should bear in mind that the participants in this review primarily come from two large scale studies that both used data from Pregnancy Risk Assessment Monitoring System (PRAMS) (21) (30) whilst only one of these studies (30) was included in the previous review. However, as there is no overlap in data - Silverman et. al. (30) used data from women participating in the PRAMS study between 2000-2003, whereas Wallenborn et. al. (21) used data from women participating from 2004-2014, we considered them as separate studies – we believe it to be a strength of this review that both studies are included.'* (ll. 257-262)
The authors have produced a Forest Plot for illustrative purposes, however they do not illustrate the weight of the studies. Looking at these, most of the studies (using adjusted ORs) appear to show insignificant associations, in particular for breastfeeding duration, however the conclusions state the majority of studies indicate IPV is associated with impaired breastfeeding.	Thank you for this comment. Figure 3 only illustrate the effect of physical violence on breastfeeding, not all different results reported in the paper. We believe there are many methods to weight studies when performing meta-analysis. We did not perform meta-analysis. Our Forest plot has ordered studies according to quality, which can be seen as a form of weight. We hope this explanation is sufficient.

	We have changed the headline of the figure accordingly and added: '*…ordered according to descending quality'* (l. 540)
The authors have gone to great efforts to extract information on confounders however the narrative in the results section does not go far enough in synthesising this data. It would be interesting to focus on the pattern of results found in the two very large scale studies and how they compare with the smaller studies, and the potential sources of these differences. The discussion introduces some themes such as childhood abuse, but this has not been outlined in the results section. A more in-depth focus on confounders would set the review apart from the previous review.	Thank you for this valid suggestion, we agree that it is important to elaborate further on the challenge of confounders. The result section now states: '*Overall, the included studies adjusted for 48 different confounders within the following domains: maternal sociodemographic, relationship characteristics, maternal lifestyle and health, economy, pregnancy and postpartum related problems, child characteristics, support during pregnancy and postpartum, violence or stressful life events, pregnancy intention, caste and religion. The most common confounding factors were maternal lifestyle and health, maternal sociodemographics, and relationship characteristics. The majority of studies did not justify their choice of confounders (20-24, 26, 28-30, 33). Sorbo et al. and Madsen et. al. used the directed acyclic graph (DAG) to justify the confounders adjusted for in their analysis, and Sorbo et. al. also made a sensitivity analysis to determine whether or not the association between IPV and breastfeeding practices was mediated primarily through postpartum depression. They found that depression could not explain early cessation of breastfeeding (34).'* (ll. 195-203)
Overall the review would benefit from greater clarity in reporting of methods, with a focus on the differences between the two reviews. If the biggest difference is the extraction of the data on confounders, this should be the key narrative running throughout the paper.	Thank you for this valuable comment. We have changed the strengths and limitations section in order to clarify the differences between the two reviews. (ll. 250-275) Further we have changed the section of interpretation of findings, also to point out differences between the two reviews. It now states: '*Overall, our study results support the findings of the recent review by Mezzavilla et. al (13) despite our review is mainly being based on different studies and have different exposures. In line with Mezzavilla et. al we found that the most investigated outcome was exclusive breastfeeding, and that studies varied in quality. In contrast to Mezzavilla et. al, they also reported significant results from studies*

	investigating women exposed to lifetime history of IPV. This may indicate that exposure to any time of violence may affect breastfeeding patterns.' (ll. 278-282)

VERSION 2 – REVIEW

REVIEWER	Helen E Morgan Cardiff University UK
REVIEW RETURNED	10-May-2020

GENERAL COMMENTS	Table 1 - Miller-Graff is reference 19 not 18 Revised Results - clarity is required in the text in order to understand the studies included in figure 3. Figure 3 is concerned with physical violence but it is not clear which studies considered physical violence with each of the 3 breastfeeding outcomes. Please add reference numbers to figure 3. Exclusive breastfeeding change to 'statistical association'. Also out of 10 studies, 5 demonstrated association and 5 did not. Therefore 'Discussion' lines 245-247 needs to reflect this, not true 'The majority of studies found that exposure to IPV in any form and at any stage had a significant negative association with early termination of exclusive breastfeeding,'

REVIEWER	Joanna Leaviss University of Sheffield, UK
REVIEW RETURNED	22-May-2020

GENERAL COMMENTS	The authors have made good improvements to the manuscript. I only have a few minor points with regards the responses to my previous comments. 1) The authors have made clarifications re. their inclusion criteria however I do feel that these would be clearer presented in a formal PICO style framework. What were the specific inclusion/exclusion criteria for e.g. study design; follow-up timescales; definitions of violence; outcomes etc.? 2) Can the authors substitute the word 'proper' when discussing the relative rigour of their own versus other quality assessments for a more appropriate word? 3) Thank you for confirming that there is no crossover of data for the PRAMS studies. 4) I would tend to disagree that quality is an appropriate proxy for weight. I do appreciate that the authors did not conduct a meta-analysis and therefore it is problematic to present the data in a way that reflects the weight of the studies. I'm inclined to think that the inclusion of the Forest plot as it is is confusing in light of this.

	5) I still think it would be useful to discuss the pattern of results found in the two very large studies compared to the smaller ones for the different outcomes.

VERSION 2 – AUTHOR RESPONSE

Reviewer: Helen E Morgan	
Table 1 - Miller-Graff is reference 19 not 18	Thank you for pointing out this error. The reference is now changed to (20), since we have added a reference according to comments from another reviewer.
Revised Results - clarity is required in the text in order to understand the studies included in figure 3.	Thank you for this comment. We have added a short comment in relation to the text regarding the forest plot in the methods section, which now states: '*Studies that investigated physical violence were presented to emphasize the most reported form of IPV'* (ll. 179-180)
Figure 3 is concerned with physical violence but it is not clear which studies considered physical violence with each of the 3 breastfeeding outcomes.	Thank you for this comment. Each breastfeeding outcome is stated in the left side under 'Outcome/Author'. We hope that this is sufficient.
Please add reference numbers to figure 3.	Thank you for this comment. References are added to figure 3. Please see the document 'Figure 3 – marked copy'
Exclusive breastfeeding change to 'statistical association'.	Thank you for pointing to the spelling mistake. Statistically is changed to statistical and it now states: *'Exclusive breastfeeding* *Ten studies assessed exposure to violence in relation to risk of early termination of exclusive breastfeeding and five studies found a statistical association (aORs= 1,53 (95 % CI: 1,01-23,1), 0,83 (95 % CI: 0,71-0,96), 1,35 (95 % CI:1,07-1,71), 0,17 (95 % CI: 0,07-0,4), 1,839 (95 % CI: 1,61-2,911) and five studies found no statistical association(Fig. 3) (Table S2.5)'*

	(l. 254 and l. 256)
Also out of 10 studies, 5 demonstrated association and 5 did not. Therefore 'Discussion' lines 245-247 needs to reflect this, not true 'The majority of studies found that exposure to IPV in any form and at any stage had a significant negative association with early termination of exclusive breastfeeding,'	Thank you for raising this point. We believe that these 10 studies you refer to are the 10 studies investigating exclusive breastfeeding, where 5 demonstrated an association. We included 16 studies. 10 studies showed a significant association between IPV exposure in any form (physical, emotional sexual) and with early termination of breastfeeding and exclusive breastfeeding, but IPV in any form did not reduce initiation of breastfeeding.
Reviewer: Joanna Leaviss	
The authors have made good improvements to the manuscript. I only have a few minor points with regards the responses to my previous comments.	Thank you so much for this comment and support.
1) The authors have made clarifications re. their inclusion criteria however I do feel that these would be clearer presented in a formal PICO style framework. What were the specific inclusion/exclusion criteria for e.g. study design; follow-up timescales; definitions of violence; outcomes etc.?	Thank you for this valid suggestion. We agree that our inclusion criteria could be clearer presented. The methods section is changed and it now states: *'A PICO-model was made to develop the search strategy and selection of the literature. We included studies with women exposed to violence one year prior to pregnancy, during pregnancy, and in the postpartum period which met the following criteria; (a) men as perpetrators of violence against women, (b) women in an intimate relationship over one month during previous pregnancies, current pregnancy and postpartum, (c) women who breast fed from the first hour and until 6 months after giving birth, (d) women exposed to IPV but also perpetrators of violence against men, (e) women exposed to other forms of violence (e.g. gang violence bulliyng). We excluded (a) women in intimate relationships of less than one month of duration (during previous pregnancies, current pregnancy or postpartum), (b) women who gave birth to twins or triplets, (c) women with absolute counter indication for breast feeding, (d) women who were not able to breastfeed (e.g. due to mastectomy), (e) women with eating disorders or chronical illness (e.g. HIV), (f) women with*

	substance abuse (e.g. alcohol, drugs), (g) studies with only sexual minorities (e.g. bisexual, homosexuals). *IPV was defined as the following: Physical violence (i.e slapping, hitting, kicking, beating), sexual violence (including forced sexual intercourse or other forms of sexual coercion), psychological violence (humiliation, insults, intimidation, threats of harm), economic violence (i.e restricting access to financial resources, education, employment and medical care) and controlling behaviours (i.e isolating a person from friends and family, controlling their movements, restricting access to education and employment). Outcome was breastfeeding practices in according with WHO's recommendations defined as; (a) intention to breastfeed (when the woman showed interest in offering breast milk), (b) start of breastfeeding/duration (when the woman offered the child breast milk in the postpartum period), (c) exclusive breastfeeding of children from first day of life and up to 6 months (exclusive breastfeeding defined as the infant only receiving breast milk without any additional food or drink, not even water), (d) duration of exclusive breastfeeding. Eligible studies for inclusion were original publications of observational studies.'* (ll. 130-151)
2) Can the authors substitute the word 'proper' when discussing the relative rigour of their own versus other quality assessments for a more appropriate word?	Thank you for pointing this out. The word 'proper' is now changed in the text and now states: *'Yet, the review did not involve an appropriate quality assessment...' (l. 113)* *'…we conducted an appropriate quality assessment..' (l. 285)* And further: *'…However, STROBE is not an accepted quality assessment tool…' (l. 287)*

3) Thank you for confirming that there is no crossover of data for the PRAMS studies.	Thank you for this comment.
4) I would tend to disagree that quality is an appropriate proxy for weight. I do appreciate that the authors did not conduct a meta-analysis and therefore it is problematic to present the data in a way that reflects the weight of the studies. I'm inclined to think that the inclusion of the Forest plot as it is is confusing in light of this.	Thank you for this valid comment and reflection. We have tried to address the issue of weights by stating the following in the discussion part: *'The individual results were presented in a forest plot, without meta-analysis to illustrate the heterogeneity across studies. The forest plot was ordered in the vertical axis by the risk of bias in a manner that places higher-quality study findings above those with lower quality. This approach is in line with the recommendation to exploit the plot's vertical dimension should be used to illustrate differences in important study characteristics such as risk of bias.'* (ll. 272-276) We still believe that the figure gives value to the article and the complexity of the topic regarding IPV.
5) I still think it would be useful to discuss the pattern of results found in the two very large studies compared to the smaller ones for the different outcomes.	Thank you for this valuable comment. We have now addressed the patterns of the two large scale studies compared to the smaller studies already mentioned in the discussion, which now states: *'The lack of consensus in identification of potential confounders and their influence on the association between IPV and breastfeeding is also illustrated in two large scale studies by Wallenborn et. al. and Silverman et. al. Hence, Wallenborn et. al. adjusted for marital status, education and insurance status, whilst Silverman et. al. adjusted for race, age, marital status, education and smoking. Their data were from the same surveillance project (PRAMS), but interestingly, Silvermann et. al. did not find any significant association when controlling for confounders, opposite Wallenborn et. al., who found a significant association, but also found that stress and smoking affected breastfeeding when controlling for IPV, which provides evidence that stress and smoking are mediators*

	and should not be treated as confounders.' (ll. 333-340)

VERSION 3 – REVIEW

REVIEWER	Joanna Leaviss University of Sheffield, UK
REVIEW RETURNED	03-Sep-2020

GENERAL COMMENTS	My second review comments have been adequately addressed.