PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Recent intimate partner violence against women and health: a systematic review and meta-analysis of cohort studies
AUTHORS	Bacchus, Loraine; Ranganathan, Meghna; Watts, Charlotte; Devries, Karen

VERSION 1 – REVIEW

REVIEWER	Sheila Sprague McMaster University, Canada
REVIEW RETURNED	30-Oct-2017

GENERAL COMMENTS	Overall Comments: This manuscript is a systematic review and meta-analysis of cohort studies examining the magnitude and temporal direction of the association between recent IPV (defined as IPV that occurred within the past 12 months) and a variety of health outcomes. This study uses sound methodology to address an important clinical question and the manuscript is well written. However, the manuscript could be strengthened by addressing the below comments Abstract: - The authors should define health in the abstract. - The authors should ensure that they are clear about the direction of the relationship they are discussing with each mention of association throughout both the abstract and entire manuscript. There are a number of examples where direction is not clear. For example, the second sentence of the results section should be revised to read: “Eight studies showed evidence of a positive association between recent IPV and SUBSEQUENT depressive symptoms…”. Background: - The authors mention that the relationship between recent IPV and health outcomes may be influenced by duration and severity of IPV, but their study does not account for this. However, the authors do mention this as a limitation and potential source of bias in the discussion. As the authors bring this point up in the background section, it would be helpful for readers if they provided rationale as to why this was not considered in their systematic review. Methods: - It is unfortunate that the authors were not able to complete the abstract screening and data extraction in duplicate due to time constraints. While this does not constitute a fatal flaw to the study methodology, it would be helpful if the authors included a discussion of other strategies they used to ensure that abstracts were not excluded in error and that data extraction was accurate. The authors may also want to consider doing random quality checks on 25% of

	the extracted studies. - The author's should clarify if they used an existing tool for quality appraisal (e.g. ROBINS), or if not, state why an existing tool was not used and provide additional details about how the quality review was completed (e.g. what criteria were used to determine if measures were reliable and valid). - The authors should provide additional details about criteria for determining what effect estimates could and could not be converted to odds ratios. Results: - The authors state " Three studies were based on sub-populations of women including those who were receiving…", however, 4 different studies are referenced. Additionally, this sentence indicates that reference 44 focussed on pregnant women, whereas the proceeding sentence indicates that an additional 6 references focussed on pregnant women, but does not include reference 44. - The authors state "…one included women and young girls", but site 2 references. - The authors state that table 2 summarizes quality issues in relation to the 34 included papers, however, this table appears to include study characteristics as opposed to a quality assessment. - Forest plots should be expanded to show the entire confidence interval as opposed to errors indicating the interval continues. General Comments: - The authors should complete and submit a PRISMA checklist - The authors should check the results section carefully for inconsistencies - The authors should also review all abbreviations and ensure each one is defined (only once) and at the time of first use.

REVIEWER	Sian Oram Institute of Psychiatry, Psychology & Neuroscience; King's College London. UK.
REVIEW RETURNED	09-Nov-2017

GENERAL COMMENTS	This review has been conducted and written to a high standard and requires only minor revisions to be suitable for publication. Abstract - p value missing for postpartum depression meta-analysis. - statement regarding confounders in conclusion is confusing as the issue is not mentioned in either the methods or results sections of the abstract. Background - Rationale for the review would be strengthened if it was stated explicitly that Devries et al reported on lifetime IPV and depression. Methods - Did the authors conduct citation tracking or reference list screening of included papers and relevant systematic reviews (e.g. Devries 2013, Howard 2013, Trevillion 2012)? This is considered good practice for the conduct of systematic reviews and not having done so may have led to the omission of relevant studies. If not conducted the authors should state this in their discussion of review limitations and justify their decision to not have done so. - The authors should state what quality appraisal instrument was used and how adapted for this review, if applicable. It also should be

	stated whether and how the quality appraisal data was used. A copy of the instrument should be provided as supplementary information/online appendix. - Criteria for conducting meta-analysis should be clearly stated in the analysis section, i.e. criteria for eligibility in meta-analysis, minimum number of studies required for meta-analysis to be conducted. Figure 2 - reasons for exclusion of full text studies should be presented. Currently the authors present reasons for exclusion of studies at the abstract only (not normal practice). Results - page 10 line 34/39: pregnant women referred to twice but referenced only once. - page 11 line 51: "waves ranged from two to 10 years" - think this should be from two to ten? - for all outcomes, it would be helpful to present the information about outcome measurement prior to the results, as information about how outcomes are measured is relevant to the interpretation of the results. - for measurement of depression, useful to highlight that the majority of included papers used screening questionnaires rather than diagnostic tools. - for depression, the authors note that Chowdhary and Patel's exclusion of women with baseline depressive disorder in their analysis may have mean the remaining cases were not representative of women experiencing IPV. The forest plot shows that their results were noticeably different from those of the other papers in the analysis; did the analysis plan include provision for sensitivity analyses? Discussion - the authors compare their findings on postpartum depression to those of Howard et al. The meta-analysis reported by Howard et al includes three longitudinal studies of depression and IPV, only one of which is included in the current paper. The authors should highlight this when comparing their results and state why two longitudinal studies included by Howard (Ludermir et al 2010 and Patel et al 2002) were not included in their review. Implications - message for services could be strengthened i.e. findings indicate that women with depression may be at recent/ongoing risk of IPV, and services should be ready to identify and respond appropriately. Tables are very well presented.

REVIEWER	William Turner PhD School for Policy Studies, University of Bristol, 8 Priory Road, Bristol BS8 1TZ, UK
REVIEW RETURNED	15-Nov-2017

GENERAL COMMENTS	BMJ Open Re: Recent intimate partner violence against women and health: a

	systematic review and meta-analysis of cohort studies Firstly, I would like to offer my sincere apologies for the delay in providing my review to this manuscript subscription. I have read the manuscript a few times; generally, having carefully considered the case for the rationale for the conduct of the study, the reporting and discussion of the findings, I have no doubt that the paper adds nicely (and importantly) to our knowledge base about the detrimental effects of IPV with particular reference to the temporal relationship between recent IPV and different health outcomes (most notably, depressive symptoms, postpartum depression, and drug use). Below I offer some observations about ways that the manuscript could be further strengthened: • The section titled 'Quality appraisal' (p. 7, lines 3-41) is, in my opinion, mislabelled. It is stated that 'the quality of each effect was appraised' (line 5) yet the narrative that follows describes (important) data analytic considerations rather than 'appraisals' of study quality. The same observation applies to the title of Table 2 of the manuscript reading 'Quality assessment of the 34-papers reporting on 33 studies included in the review'. The Table offers a good description of the characteristics of included studies but I could not identify a 'quality appraisal' element' (e.g. low, medium, high or similar). • In the 'Data Analysis' section, esp. page 8, the authors state that "some studies reported multiple estimates using overlapping definitions of IPV on the same sample of participants" (p. 8, line 12-13) and proceed to describe how they dealt with such cases in their data (p. 8, lines 18-27). While reference to the way authors dealt in those instances is important, it would be good if there was explicit reference in how many instances (e.g. for how many of the included studies) this took place. • In my version of the manuscript Figure 2 would need some attention before publication, more specifically the graphics. Additionally, I have some questions about the description offered for one of the bullet-list section in the box labelled 'Records excluded', more specifically the last one reading "study sample consists only of abused women (n=17)". Here some more specificity about the reason(s) for excluding these studies would be welcome.

	• One of the findings of the current review which contrasts with the authors' previous review refers to the lack of a bi-directional relationship of IPV with alcohol (p. 26, lines 8-12). Yet, there is no discussion as to the possible reason(s) for this finding and some brief reference to this respect would be welcome here. Generally, this is well-conducted and reported systematic review which I have no hesitation endorsing its publication to the journal. Thank you for giving me the opportunity to review the manuscript which will make an important contribution to the literature about the health effects of IPV and further contribute to public health policies aiming to safeguard women and their children.

REVIEWER	DR Jones University of Leicester, UK
REVIEW RETURNED	19-Dec-2017

GENERAL COMMENTS	This paper has some potential to add a little further more targetted exploration to the earlier reviews by the authors. However, the current draft needs substantial changes and improvement - particularly in respect of a) clarity of presention of results, and b) exploratory analyses - before being published. Essential improvements include: 1) Completion of MOOSE and other relevant checklists, and reporting this to be the case. Justification of any limitations re compliance with recommendations therein - e.g. here re the number and characteristics of primary studies not in English. 2) Redrafting of textual summaries of results so that they correspond exactly and clearly with the results presented in the figures, with reasons for deviations/exclusions of studies being clearly indicated. [For example, on p15 l21 5 studies contributing to the depression and subsequent IPV result set are identified but the results presented in the meta-analyses in Fig 3 are not from the same set of studies. There may be exclusions because of continuous variables etc, but these should be explicitly indicated. Why, on the other hand, is a result from study 37 included? Similar considerations apply to other summaries and analyses presented.] 3) Exploration of possibility of inclusion of results from primary studies using continuous measures. Whilst transformation of results in this way should be undertaken cautiously, it may be useful if details of the approaches used (e.g. cut-points when dichotomising) can be justified externally, and are subjected to sensitivity analysis. 4) p6 l14 Inclusion criteria would be clearer if presented as bullet points. 5) p8 l18 The implementation of the algorithm for dealing with multiple ORs is not quite explicit. Where the criteria implemented in

	the sequence indicated? 6) Reason for the exclusion of the studies excluded after evaluation of full-text articles should be tabulated in a supplementary table. 7) There is some consideration of confounding and adjustment in the primary studies. However, it is not quite clear that adjusted ORs were used whenever available and that ORs were unadjusted whenever they needed to be calculated from raw data. If possible, adjusted/undajusted types should be indicated in the summaries/figures of results as well. (See comment 12) below.) Other issues needing consideration and amendment include: 8) There is some overemphasis on statistical significance of the primary study results, 9) Fig 1 appears to be missing; it is perhaps wrongly labelled as Fig 2 in this draft. 10) p7 ll10,19 The sentences beginning 'Firstly..' and 'Secondly..' do not have main verbs. 11) p11 l33 and following; p15 l32 Incorrect use of commas. Other optional issues: 12) Exploration of more sophisticated graphical presentation of SR results, e.g. using different symbols to represnt primary studies with different characteristics (of population studied, measures used, and so on) may be useful. 13) Similarly, graphical presentation of results of studies in the systematic review may be useful even if they are not suitable for combination in a formal meta-analysis.

VERSION 1 – AUTHOR RESPONSE

Response to BMJ Open

Manuscript ID bmjopen-2017-019995: Recent intimate partner violence against women and health: a systematic review and meta-analysis of cohort studies

Dear Dr Clark,

Thank you for your email of the 5th January 2018. We found the reviewer's guidance and suggestions extremely helpful in strengthening the paper. I am pleased to upload a revised copy of the manuscript. Amendments are in red text. I outline the responses to the individual reviews below.

Associate Editors comments

1) Thank you for bringing out attention to the new Halim et al. 2017 systematic review on intimate partner violence during pregnancy and perinatal mental health disorders. We have commented on this review in the discussion on page 38. Most of the studies included in their review are cross-sectional and consider partner violence experienced during pregnancy. The authors report the findings narratively do not conduct a meta-analysis. We included one additional study identified in this review in our meta-analysis of postpartum depression published in 2017. In the discussion on page 33 and 34 we have added a sentence on what

our review adds to existing evidence including the Beydoun et al (2012) which you also highlight. The Beydoun et al (2012) systematic review includes primarily cross-sectional studies with only a few prospective studies (n=5) which we captured an d included if they met our inclusion criteria. Furthermore, the Beydoun review is not limited to recent exposure to IPV and considers lifetime exposure to IPV which may have occurred in the distant past.

2) *The quality assessment is not clear and Table 2 does not really seem to reflect any overall assessment of quality.*
Thank you for this feedback. We have created a new Table 1 (page 10) which focuses on key domains of study quality in relation to the associations we examine. These include: IPV measurement and time frame, health outcome measurement and time frame, length of follow-up period, number of waves, attrition rate at last wave, mode of administration of survey, whether or not there was adjustment for Time 1 levels of the outcome variable, whether or not the study adjusted for childhood sexual abuse (which is a recognised confounder in the violence against literature) and a list of other variables adjusted for. These correspond to the major relevant domains of potential bias in the quality assessment tools (eg ROBINS-I) suggested by the reviewers.

   We do not use an overall quality assessment score. Whilst quality assessment tools are used in a range of systematic reviews, the Cochrane Collaboration cautions against their use, and we follow the Cochrane approach here. This is because overall quality scores are based on the addition of a range of different dimensions of study quality, all of which are likely to relate to actual bias in a non-linear way. That is, a study scoring poorly on quality because of 2 dimensions, (for example, the level of attrition is slightly higher and the follow-up duration is slightly shorter) might actually give an estimate closer to the true estimate relative to a study which scores poorly on one dimension (for example, it has only has a slightly lower level of attrition). Therefore, adding up quality scores to reflect overall quality is problematic and not recommended. Instead, we describe key dimensions of study quality, including (listed above), to give readers a detailed picture of the different elements of study design and conduct that might bias estimates. The content of what we describe does correspond to the key areas where bias may result in relation to our particular review question. These are quality criteria tailored specifically for the associations examined here.
3) It is suggested that we could give more detail on key confounders adjusted for. We do provide details in the results section of each health outcome of key confounders that were adjusted for. In the newly created Table 1 (page 10), we have also included a list of all other variables adjusted for.
4) All figures (forest plots and flow diagram of review process) have been removed from the manuscript and are uploaded as separate files.

Reviewer 1: Sheila Sprague

1) The reviewer asks us to define use of ‘health’ in the abstract. We have clarified in the abstract that we are focussing on a range of health outcomes or health risk behaviours that may result in adverse health outcomes.
2) We have clarified the direction of the relationship between intimate partner violence and the health outcome, both in the abstract and throughout the entire manuscript where this was not already clear.
3) The reviewer requests that we explain why we do not take into account duration and severity of intimate partner violence in the meta-analysis, since we state in the background that they may influence the relationship with health outcomes. It was not possible to test this because although many studies used the Conflict Tactics Scale (CTS) or CTS-like questions, violence was conceptualised as physical, sexual, verbal or emotional, with most using a combination of types of violence and modelling this as ‘ever versus ‘never’ experienced in the past year’. Only one study provided estimates of minor and severe violence. Studies reported the period in which the violence occurred (e.g. past year, past six months etc…), but not duration. We have included this explanation in the discussion under limitations of included studies on page 42.
4) *Whilst it is not a fatal flaw that double screening and data extraction did not occur, the authors should report on other strategies used to ensure that abstracts were not included in error and that data extraction was accurate. The authors may want to consider doing a random quality check on 25% of the extracted studies.* Although it was not possible to undertake double screening on all abstracts, LJB and MR did undertake double screening of all full text included

papers. KD reviewed all full-text articles when there was uncertainty about their inclusion. We have clarified this further on page 7.

5) *The authors should clarify if they used an existing quality tool for quality appraisal (e.g. ROBINS) or if not, state why and provide additional details about how the quality review was completed (e.g. what criteria were used to determine if measures were reliable and valid).* Thank you for highlighting this. We did not use an existing quality appraisal tool for reasons explained in point (2) to the associate editors above. However, we did extract data on key quality dimensions specific to our review question. We note that the use of overall quality scores is not recommended to appraise the quality of articles, following the approach of the Cochrane Collaboration. Please also see responses to Associate Editor comments.
6) We have removed the sentence from the data analysis section of the methods "where studies did not report odds ratios (ORs), these were calculated from raw data where possible". It now states "Adjusted odds ratios (ORs) were extracted directly from the papers with the exception of one unadjusted OR which was calculated for a study on perceived stress". See page 8.
7) On page 27, we have amended the paragraph on studies based on sub-populations of women. We indicate in the text where two papers report on the same sample. Initially, there was a hanging sentence referring to pregnant women and no references.
8) We have created a new Table 1 (see page 10).

9) *Forest plots should be expanded to show the entire confidence interval as opposed to errors indicating the interval continues.*
We are happy to make this change if the editor feels it is appropriate. However, this will have the effect of shrinking the other estimates relative to the area of the graph. We felt that the current presentation is preferable as it allows a closer view of the main range of estimates, and the upper and lower confidence estimates are displayed numerically in the right hand column for interested readers.
10) We have completed and uploaded a PRISMA checklist.
11) We have checked the results section (and entire manuscript for errors or inconsistencies).
12) We have reviewed all abbreviations and have ensured that each one is defined only once and at the first time of use.

Reviewer 2: Sian Oram

1) In the abstract, we have inserted the missing p value for the postpartum depression meta-analysis.
2) In the abstract, we have removed the sentence on confounding from the conclusion.
3) The reviewer recommends that we state explicitly in the background section that the Devries et al. review reported on lifetime intimate partner violence and depression. On page 4 of the background, we do state that those reviews explored the relationship between 'ever' exposure to intimate partner violence and specific health outcomes, then reporting on the depression and alcohol reviews. We have amended 'specific' to 'depressive symptoms and alcohol use'. On page 5 we already problematize estimates of 'ever' exposure (i.e. heterogeneous and includes past year, before the past year and more distance experiences).
4) We did not conduct citation tracking and have stated this as a limitation of the review in the discussion on page 39. However, we did undertake reference list screening of key systematic review papers. We have added this on page 6 under 'literature searches'.
5) *The authors should state what quality appraisal instrument was used and how it was adapted for this review, if applicable. It also should be stated how the quality appraisal tool was used and provided as supplementary information online*. Please see our response to the associate editors (point 2) and Reviewer 2 (point 5).
6) On page 8 in the data analysis section, the criteria for conducting meta-analysis has been included re: minimum number of estimates.
7) In Figure 1 (uploaded separately) we have elaborated the reasons for exclusion of the full text articles.
8) On page 27, the short paragraph on sub-samples and pregnant women has been amended (see point 7, Reviewer 1).
9) On page 28 we have amended "two to 10 years" to "two to ten years".
10) In line with this reviewer's suggestion, for all outcomes we have presented information on measurement prior to the pooled analysis since measurement

11) Although measurement of outcomes is key to interpretation, however, so is the section on risk factors and confounding. We appreciate this reviewer's comment about presenting information on measurement of outcomes first. However, for readability and flow we feel it is best to first report how many studies there were in total for the outcome, the direction in which the associations were measured, followed by the findings. The sections on measurement and risk/confounding follow. None of the other reviewers suggested re-ordering these sections and prefer to keep it as it is.
12) On page 30, we have highlighted that all but one of the depression studies used screening questionnaires that measured depressive symptom as opposed to diagnostic tools.
13) *The authors note that Chowdhary's exclusion of women with baseline depressive disorder in their analysis may have meant the remaining cases were not representative of women experiencing IPV. The forest plot shows that their results were noticeably different from those of the other papers. Did the analysis plan include sensitivity analysis?*
Thank you for highlighting this. We did re-run the meta-analysis excluding the Chowdhary estimate, and it did not materially change the overall pooled estimate. The pooled estimate including the Chowdhary study was (OR=1.76; 95% CI 1.26-2.44, I^2 = 37.5%, p=0.172). The pooled estimate excluding the Chowdhary study (OR=1.83; 95% CI 1.35-2.49, I^2 = 35.1%, p=0.202). We have included this as a foot note under the forest plot.
14) This reviewer requests justification for excluding two longitudinal studies from the postpartum depression analysis (Ludermir and Patel) from our review, which were included in their systematic review. We did include the Patel et al. 2002 study in our review and it appears in the quality assessment Table 1 which is the unadjusted relative risk from the paper. We excluded the Ludemir et al 2010 study from our review. It measures postnatal depressive symptoms using the Edinburgh Postnatal Depression Scale at the 3-6 month follow-up, but it uses the SRQ-20 to assess common mental health disorders during pregnancy (e.g. PTSD, schizophrenia, OCD, phobic disorders as well as depression) and does not provide a clean estimate of prior levels of depressive symptoms. In our review, we considered studies that measure the outcome on at least two occasions. Other studies on postpartum depressive symptoms used the same instrument to assess symptoms on at least two occasions. Please see page 21.
15) On page 43 under 'implications' we have strengthened the section by adding: "Women with depression may be at risk of IPV, including IPV that is ongoing and services should be trained to identify and respond appropriately."

Reviewer 3: William Turner

1) *The section title 'Quality Appraisal' is in my opinion mislabelled. It states that the 'quality of each effect was appraised', yet the narrative that follows describes important data analytic considerations rather than appraisals of study quality. The same observation applies to the title of Table 2 of the manuscript reading 'Quality assessment of the 34 papers….'. The table offers a good description of the characteristics of included studies, but I could not identify a 'quality appraisal' element (e.g. low, medium high or similar).*
Thank you for raising this. Please see our responses to Associate Editor comments and Reviewer 2.
2) With regard to studies using multiples estimates and our algorithm for choosing one estimate, these studies are already indicated in Table 2 using a notation. However, we have included a statement on page 9 in the data analysis section about how many studies (n=3) this applied to.
3) The reviewer queries why studies which consisted only of abused women were excluded (n=15). As our objective was to estimate the magnitude of the association between recent IPV exposure and a range of health outcomes, we needed to include studies which included a reference group not exposed to recent IPV. Although many included studies constructed the reference categories for recent IPV as binary opposites (meaning that some participants in the reference group may have been exposed to other forms of IPV that were not measured or modelled), these reference groups will have included mainly women with no history of partner abuse.
4) This reviewer highlights that the finding on IPV and alcohol contrasts with the authors' previous review and asks for discussion of this. The potential reasons are outlined on page 38 and 39 of the discussion and we have elaborated further.

Reviewer 4: Dr Jones

1) In line with the recommendations of Reviewer 1 (point 10) we have completed and uploaded a PRISMA checklist.
2) We have checked the references in the textual summaries and forest plots for consistency. The inaccuracies were due to a problem with mislabelling of the reference numbers in the Stata file. We have ensured that all textual summaries reflect the figures and that textual summaries are appropriately referenced.
3) *Exploration of possibility of inclusion of result using continuous measures. Whilst transformations of results in this way should be undertaken cautiously, it may be useful if details of the approaches used (e.g. cut-points when dichotomising) can be justified externally and are subjected to sensitivity analysis.* Thank you for this suggestion, but there are relatively few estimates which are continuous and papers mainly do not report enough information to enable us to do this.
4) We have presented the inclusion criteria as bullet points as suggested on page 6.
5) This reviewer queries whether the algorithm for dealing with multiple ORs from the same study was implemented in the sequence indicated. Reviewer 3 felt this was clearly stated. Algorithm implies the set of rules to be followed (which is already numbered in sequence 1 to 4). However, to make this more explicit we have added the line 'implemented in the following sequence' on page 8.
6) We have amended Figure 1 on page to include the reasons for exclusion of the 47 full text articles.
7) We have clarified on page 8 that adjusted ORs have been used and as per Reviewer 1 (point 6) we have removed the sentence from the data analysis section of the methods "where studies did not report odds ratios (ORs), these were calculated from raw data where possible". It now states "Adjusted odds ratios were extracted directly from the papers with the exception of one unadjusted OR which was calculated for a study on perceived stress".
8) *There is some overemphasis on statistical significance of the primary study results.*
   In general, we should account for direction of findings and for all outcomes presented in the results report on how many reached statistical significance.
9) Figure 1 was mislabelled Figure 2. We have amended this.
10) The sentences beginning 'Firstly…' and 'Secondly…' have been amended on page 7.
11) The incorrect use of commas on pages 11 and 15 have been removed.
12) This reviewer makes an optional suggestion of using a graphical presentation of the studies in the review that are not included in the meta-analysis. Other reviewers have commented that the tables are presented well where all estimates (regardless of type) are grouped by health outcome. Although we would ideally like to do this, given the number of different types of estimates it would not be possible to combine them all on a single graph (thus leading to a very large number of graphs). We selected a table format as it was more efficient.

VERSION 2 – REVIEW

REVIEWER	DR Jones University of Leicester UK
REVIEW RETURNED	15-Mar-2018

GENERAL COMMENTS	I am reasonably happy with the responses to most of my earlier comments.

REVIEWER	Sian Oram King's College London, UK
REVIEW RETURNED	03-Apr-2018

GENERAL COMMENTS	The authors have addressed the points raised in my review; thank you.

REVIEWER	Dr. Sheila Sprague McMaster University, Canada
REVIEW RETURNED	17-Apr-2018

GENERAL COMMENTS	Thank you for submitting your manuscript for review. The paper is much improved. A few minor comments: 1) How was the additional paper found during the review period? 2) Was the full literature search repeated? 3) Consider repeating the literature search as 1.5yrs have passed. 4) Consider shortening the manuscript as it is quite lengthy.

VERSION 2 – AUTHOR RESPONSE

Reviewer 1:

1) The additional paper was brought to our attention by the editors during the peer review process. The paper by Halim, published in 2017, is a new systematic review of studies on intimate partner violence during pregnancy and perinatal mental health disorders. The studies in the review were mostly cross-sectional, although there was one cohort study that met our inclusion criteria. We have added a footnote to the flow chart stating where the paper came from and why it was included (i.e. requested by the editor), although it was published outside of our search dates. We have also included another box in the flow diagram. However, your website is experiencing problems with file conversions and uploads. I will email the flow chart as a jpeg separately to you.

2) We did not perform a new search, as this was not requested by the editor or any of the reviewers. Doing so would require substantial time and effort with further delays (as the team are now working full-time on other research studies), and would be highly unlikely to alter our findings. We updated the initial search (up to January 2016) to November 2016, which yielded no new studies. We have clearly indicated the start and end dates of our searches. If the editor feels a new search is essential, we will consider this. However, in view of the fact that there were substantial delays during the peer review process, which have led to delays in potential publication, we would request that the manuscript be published with current search dates.

3) We have added additional detail to the manuscript as requested by the peer reviewers (to the results and discussion) which was amended in line with their suggestions. Therefore, we would prefer to leave the manuscript as it is.