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)	The relationship between Intimate Partner Violence reported at the first antenatal booking visit and obstetric and perinatal outcomes in an ethnically diverse group of Australian pregnant women: A population based study over 10 years
AUTHORS	Dahlen, Hannah; Munoz, Ana Maria; Schmied, V; Thornton, Charlene

VERSION 1 – REVIEW

REVIEWER	Maeve Wallace, PhD Tulane University, USA
REVIEW RETURNED	21-Sep-2017

GENERAL COMMENTS	The purpose of this study was to determine the incidence of domestic and family violence among pregnant women seeking care at a major health facility in Western Sydney. The purpose was also to determine relationships between intimate partner violence and a range of maternal and perinatal outcomes. The study is based on 10 years of medical records for over 33,000 women. The multicultural diversity of this sample of women and the large sample add strength to this paper, but I feel a more focused analysis would reveal more robust and novel findings. In general, the focus of this analysis is unclear and the reported findings are difficult to follow. I found that the description of the analysis lacked sufficient detail to understand the specific associations that would be tested. The Variables section could be expanded and edited to include detail about the comparison groups and outcomes examined. For example, it says "The relationship between psychosocial risk and health outcomes were also examined." However, I do not know what health outcomes are being referred to, and furthermore, I am not sure where those results are shown. I understand from reading on that Tables 2 and 3 represent crude bivariate comparisons contrasting Australian and non-Australian born women, and that Tables 4, 6, and 7 are results from crude bivariate comparisons contrasting women experiencing DFV and those that did not. I see that table 7 includes adjusted odds ratios (would suggest including a footnote to remind readers to what covariates were included in adjusted models), but it seems as though the crude ORs correspond to data already presented in Table 4. I am unclear about the data presented in Table 3 for gestation – I believe the percentages should add to 100, the proportion born at 37 weeks and greater look too low. The authors themselves state that the advantage of the Obstetrix

	database is the large number of variables it contains (including socioeconomic factors) that would allow for testing for interactions. I am curious as to why they did not test for any interactions as they may have potentially revealed significant effect modification (for example, by country of origin) for some of the key outcomes (preterm birth, for example). This is where I feel the authors could delve deeper into a more focused analysis that may represent a more substantial contribution to what is known. Relatedly, in general I do not believe body mass index and psychosocial factors are viewed as socioeconomic factors (although they may be related to socioeconomic status), and I do not know why they are considered as such in the discussion. I think the manuscript would benefit from some close reading and copy editing. There are typos throughout – missing periods and commas, in the title the word "Australian" should be "Australia", and run-on sentences or awkwardly worded sentences. For example "WHO stated that all forms of interpersonal violence lead to negative health outcomes and should be addressed by the health system and they identified health services as an appropriate entry point for addressing this." Also there may be a typo in the title of Table 7 – the results suggest that the reference group is women without DFV.

REVIEWER	Rosiane Mattar Escola Paulista de Medicina - Universidade Federal de São Paulo, Brazil
REVIEW RETURNED	24-Sep-2017

GENERAL COMMENTS	In the abstract put the time and what are the questions to identify violence. How was the identification about violence in the pregnancy or before if the question was about the last year? Don´t you think it is necessary to ask about violence in the first and in the last trimester of pregnancy, could you discuss this in the paper?

REVIEWER	Jo Spangaro University of New South Wales, Australia
REVIEW RETURNED	11-Oct-2017

GENERAL COMMENTS	This is an important and useful study based on a large cohort, providing consistent and relevant data base yielding impressive results.It is likely to be of utility to the field because of clear implications for intervention in relation to intimate partner violence (IPV).. Weaknessses /Areas for revision The authors use the term "domestic and family violence" which is not widely used in research or practice. In UK the term "domestic violence" tends to be used, however most researchers prefer "intimate partner violence" or "partner abuse" for the greater specificity these terms offer. In any case the authors need to provide a definition suggest replace with "intimate partner violence" and provide a definition – either the local NSW Health definition or that used by the WHO. Need to provide background to IPV including that it is incorporated into the psycho-social screen and how – providing data on what is counted as positive screen for IPV

	Although interesting It is unclear what the data on the proportion of women born overseas adds to a paper focussing on IPV, particularly if this has been previously reported as suggested eg Figures 1 and 2, Table 2-3.. The strength of this paper is in the IPV reporting and links to psych-social risk factors AND birth outcomes. The background section focusses heavily on the Australian context which may be less relevant to all readers. The paper could use a re-structure – there are typographical errors, the sequence is not completely logical, for example it is suggested that the IPV prevalence precede Table 4 on risk factors.. Additional cross- referencing to the tables including eg Table 1 in the Analysis section would be useful for the reader's navigation. Acronyms which are not universally used should be spelt out in full (eg (GDM, TPIL, APH, NICU). Table and Figure headings are not in all instances self-explanatory and should be re-considered. References from 35 on seemed to be missing from the Bibliography. Further analysis of the findings could accompany the tables eg Table 6. Analysis or at least discussion is warranted on why some maternal risk factors were LOWER for women who had experienced IPV eg normal vaginal delivery, tears, episiotomy, hypertension. For example are these potentially a result of the Australian women who are also more likely to disclose IPV being younger? DISCUSSION The results for both risk factors AND adverse outcomes are compelling and more should be made of these in the Discussion. The discussion of possible reasons for higher rates of disclosure among multiparous women is useful. I wonder if another possible explanation is that the hope that a coercive partner will reform once a baby has arrived has proved unfounded.

VERSION 1 – AUTHOR RESPONSE

Editor in Chief BMJ Open

22nd November 2017

Dear Editor,

Thank you for reviewers for providing such a thorough review of our work. We will address the comments in turn.
Thank you for the opportunity to respond to these reviews. We have read carefully each comment and responded below in italics. We thank the reviewers for their time thoughtful approach to our paper.

Reviewers' Comments to Author:

Reviewer: 1
Reviewer Name: Maeve Wallace, PhD
Institution and Country: Tulane University, USA Competing Interests: None declared

The purpose of this study was to determine the incidence of domestic and family violence among pregnant women seeking care at a major health facility in Western Sydney. The purpose was also to determine relationships between intimate partner violence and a range of maternal and perinatal

outcomes. The study is based on 10 years of medical records for over 33,000 women. The multicultural diversity of this sample of women and the large sample add strength to this paper, but I feel a more focused analysis would reveal more robust and novel findings. In general, the focus of this analysis is unclear and the reported findings are difficult to follow.

I found that the description of the analysis lacked sufficient detail to understand the specific associations that would be tested. The Variables section could be expanded and edited to include detail about the comparison groups and outcomes examined. For example, it says "The relationship between psychosocial risk and health outcomes were also examined." However, I do not know what health outcomes are being referred to, and furthermore, I am not sure where those results are shown. I understand from reading on that Tables 2 and 3 represent crude bivariate comparisons contrasting Australian and non-Australian born women, and that Tables 4, 6, and 7 are results from crude bivariate comparisons contrasting women experiencing DFV and those that did not.

We thank the reviewer for their comments and have made the following adjustments in line with comments made to provide clarity.

The specific variables included in the analyses and the specific psychosocial risk (Intimate partner violence) have now been listed to provide clarity. Page 6, paragraph 4. The results of the analysis of IPV and outcomes are shown in Tables 4, 6 and 7.

I see that table 7 includes adjusted odds ratios (would suggest including a footnote to remind readers to what covariates were included in adjusted models), but it seems as though the crude ORs correspond to data already presented in Table 4.

Table 4 presents the statistical comparison of outcomes and health status for women with and without IPV whilst Table 7 presents the regression results for these two categories of women. This data could all be presented on the one table, but that is not common practice to present data in this way and we felt that this may make the table difficult to read if presented as an amalgamated table.

I am unclear about the data presented in Table 3 for gestation – I believe the percentages should add to 100, the proportion born at 37 weeks and greater look too low.

We apologise as there was a data transcription error in Table 3 which has not been rectified – Page 10.

The authors themselves state that the advantage of the Obstetrix database is the large number of variables it contains (including socioeconomic factors) that would allow for testing for interactions. I am curious as to why they did not test for any interactions as they may have potentially revealed significant effect modification (for example, by country of origin) for some of the key outcomes (preterm birth, for example). This is where I feel the authors could delve deeper into a more focused analysis that may represent a more substantial contribution to what is known.

We thank the reviewer for their suggestion. As this focus of this paper was on IPV in Australian and non-Australian born women we felt that further analyses of specific country of origin of maternal birth for other outcomes would best be addressed in future papers.

Relatedly, in general I do not believe body mass index and psychosocial factors are viewed as socioeconomic factors (although they may be related to socioeconomic status), and I do not know why they are considered as such in the discussion.

In the literature there is a large body of research, many of which utilise very large population health datasets, which have reported associations between lower socio-economic status and increased BMI in industrialised countries. This would support the use of BMI as a marker of socio-economic status in our study. The literature would also support the increased prevalence of anxiety, depression and IPV in lower socio-economic groups within our society and for these reasons we have included this in our discussion.

I think the manuscript would benefit from some close reading and copy editing. There are typos throughout – missing periods and commas, in the title the word "Australian" should be "Australia", and run-on sentences or awkwardly worded sentences.

For example "WHO stated that all forms of interpersonal violence lead to negative health outcomes and should be addressed by the health system and they identified health services as an appropriate entry point for addressing this." Also there may be a typo in the title of Table 7 – the results suggest that the reference group is women without DFV.

Thank you for this. We have edited the manuscript carefully now and made several changes, including Table 7.

Reviewer: 2
Reviewer Name: Rosiane Mattar
Institution and Country: Escola Paulista de Medicina - Universidade Federal de São Paulo, Brazil
Competing Interests: None declared

In the abstract put the time and what are the questions to identify violence.

The time period is already in the abstract under design: i.e.2006-2016.

There are a number of questions which are asked at the antenatal booking visit in relation to domestic violence. To place this list in the abstract would not be possible within the word constraints. The specific questions are found in Table 1.

How was the identification about violence in the pregnancy or before if the question was about the last year?

There are two questions both about the last year and currently. This is the standard question set by the Australian government and it enables us to detect current or recent risk relevant to the pregnancy. Everyone in Australia asks the question this way. If however a woman discloses past DV we would still make a note as this is significant

Don´t you think it is necessary to ask about violence in the first and in the last trimester of pregnancy, could you discuss this in the paper?

There are suggestions that this would be a good thing and we have added this now to the discussion. Thanks for this suggestion

There are current discussions amongst health workers and government services that screening women for IPV initially at booking and again during the third trimester could be advisable as IPV may escalate and/or women may feel more comfortable and trusting of their care provider as the pregnancy advances. This may be even more useful in continuity of care models where women are cared for by a trusted midwife who they get to know and trust. Others suggest that questions about

IPV should not be asked at the first visit as is currently done as no relationship has been developed. There is little evidence as to what might be the best approach. There is debate about both the effectiveness of IPV enquiry and the most appropriate time to conduct assessments in pregnancy and after birth (O'Doherty et al., 2015). A number of authors report that when asked, women may choose not to disclose about the abuse at the initial time of asking, for fear of their own safety but asking signifies that she can disclose at a later contact (Salmon et al., 2015). As a result of this debate there is inconsistent and at times poor uptake of screening in antenatal services in Australia (Baird & Mitchell, 2014). Page 17-18

Baird, K. M., & Mitchell, T. (2014). Using feminist phenomenology to explore women's experiences of domestic violence in pregnancy. British Journal of Midwifery, 22(6), 418-426. doi: 10.12968/bjom.2014.22.6.409

O'Doherty, L., Hegarty, K., Ramsay, J., Davidson, L. L., Feder, G., & Taft, A. (2015). Screening women for intimate partner violence in healthcare settings. The Cochrane database of systematic reviews, 7, CD007007. doi: 10.1002/14651858.CD007007.pub3

Salmon, D., Baird, K. M., & White, P. (2015). Women's views and experiences of antenatal enquiry for domestic abuse during pregnancy. Health Expectations, 18(5), 867-878. doi: 10.1111/hex.12060

Reviewer: 3
Reviewer Name: Jo Spangaro
Institution and Country: University of New South Wales, Australia Competing Interests: None to declare

This is an important and useful study based on a large cohort, providing consistent and relevant data base yielding impressive results.It is likely to be of utility to the field because of clear implications for intervention in relation to intimate partner violence (IPV)..

Thank you for your kind comments

Weaknessses /Areas for revision
The authors use the term “domestic and family violence” which is not widely used in research or practice. In UK the term “domestic violence” tends to be used, however most researchers prefer “intimate partner violence” or “partner abuse” for the greater specificity these terms offer. In any case the authors need to provide a definition suggest replace with “intimate partner violence” and provide a definition – either the local NSW Health definition or that used by the WHO.

We have had long debates about the right terminology and as you point out there is debate about what is appropriate. We went with DFV as the WHO uses this term and it is apparent that some of the violence being seen is from family members such as mother-in-laws etc. We are happy however to change to IPV which we had originally. We have changed this throughout the paper.

Need to provide background to IPV including that it is incorporated into the psycho-social screen and how – providing data on what is counted as positive screen for IPV Although interesting It is unclear what the data on the proportion of women born overseas adds to a paper focussing on IPV, particularly if this has been previously reported as suggested eg Figures 1 and 2, Table 2-3.. The strength of this paper is in the IPV reporting and links to psych-social risk factors AND birth outcomes.

Thanks for your comments. The aim of this study was to determine the incidence of IPV in a pregnant multicultural population not born in Australia compared to Australian born women and to determine the relationship between DFV reported at booking interview and obstetric and perinatal outcomes. Hence there were two aims. Figures 1&2 set the scene for those non-Australian readers who may not realise the demographic changes over recent years. If the editor thinks these should go then we are happy to remove them but we feel they illustrate the change in needs and cultures over time. Again

table 2 & 3 provide the overall demographics for the population. We are happy to either remove them or add as supplementary files. We are happy to be advised by the Editor. We agree with you regarding what you consider the strength of the paper.
The background section focusses heavily on the Australian context which may be less relevant to all readers.

We have quite a sizeable section at the front of the discussion on the literature from around the world on IPV. We agree the final section on screening is Australian focused but this was to set the context for this Australian study.

The paper could use a re-structure – there are typographical errors,

Thanks we have now done a thorough edit and fixed these.

The sequence is not completely logical, for example it is suggested that the IPV prevalence precede Table 4 on risk factors.. Additional cross- referencing to the tables including eg Table 1 in the Analysis section would be useful for the reader's navigation. Acronyms which are not universally used should be spelt out in full (eg (GDM, TPIL, APH, NICU).

We have now made better linking in the paper between the tables and made the order more logical. We have checked all the Acronyms and made sure the first time they are used they are spelt out in full.

Table and Figure headings are not in all instances self-explanatory and should be re-considered.

Thanks we have had a closer look at the headings for the tables and figures and made some changes to make clearer.

References from 35 on seemed to be missing from the Bibliography.

We apologise about this. For some reason the endnote references manager stopped working at this point and we have now fixed this

Further analysis of the findings could accompany the tables eg Table 6. Analysis or at least discussion is warranted on why some maternal risk factors were LOWER for women who had experienced IPV eg normal vaginal delivery, tears, episiotomy, hypertension. For example are these potentially a result of the Australian women who are also more likely to disclose IPV being younger?

I think you might be referring to Table 4. And we have now added more analysis to this section.

Likewise the higher number of multiparous women reporting IPV would impact on the higher rates of normal birth seen in this group as a well the lower episiotomy rate and severe perineal trauma rate.
Page 16

DISCUSSION The results for both risk factors AND adverse outcomes are compelling and more should be made of these in the Discussion. The discussion of possible reasons for higher rates of disclosure among multiparous women is useful. I wonder if another possible explanation is that the hope that a coercive partner will reform once a baby has arrived has proved unfounded.

Thanks for this comment and we have now provided more discussion around this issue of the higher disclosure from multiparous women. I think your point is very valid and more work perhaps in the form of case studies needs to be done to understand this better. We have added the following to the discussion.

Another possibility for this higher rate of disclosure of IPV with multiparous women may be due to the fact that hopes that a coercive partner may reform once the baby has arrived are not realised. Perhaps also motherhood shifts loyalty from a non-supportive partner to a baby and energy and affection is channelled more to the baby. This in turn may make reporting easier but may also lead to an escalation of IPV. It is really important more research is done to help understand this. Page 16

We hope that our comments and amendments are satisfactory for publishing in your journal.
Yours sincerely

Professor Hannah Dahlen
On behalf of both authors

VERSION 2 – REVIEW

REVIEWER	Maeve Wallace Tulane University School of Public Health and Tropical Medicine, USA
REVIEW RETURNED	18-Dec-2017

GENERAL COMMENTS	As the authors themselves state, "the focus of this paper was on IPV in Australian and non-Australian born women", and so I am still curious as to why they did not test for effect modification by Australian/non-Australian born status, which is what I meant previously (interaction by country of origin).

VERSION 2 – AUTHOR RESPONSE

Dear Editor,

Thank you once again fro this revision and I note now only one comment to deal with.

"the focus of this paper was on IPV in Australian and non-Australian born women", and so I am still curious as to why they did not test for effect modification by Australian/non-Australian born status, which is what I meant previously (interaction by country of origin).

Testing all effects by the different ethnicities represented would have meant we had very small cell sizes for some of the populations and this could be statistically problematic. We do however ever agree the title is slightly misleading and so suggest changing it to

"The relationship between Intimate Partner Violence reported at the first antenatal booking visit and obstetric and perinatal outcomes in an ethnically diverse group of Australian pregnant women: A population based study over 10 years"

If the editor is happy with this change we think it resolves the issue raised by the reviewer above.

VERSION 3 – REVIEW

REVIEWER	Maeve Wallace Tulane University, USA
REVIEW RETURNED	17-Jan-2018

GENERAL COMMENTS	The title appears to be the only change in this revision.