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Interventions for preventing or reducing domestic violence against pregnant women (Review)

Jahanfar S, Janssen PA, Howard LM, Dowswell T

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[Intervention Review]

Interventions for preventing or reducing domestic violence against pregnant women

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ABSTRACT

Background

Domestic violence during pregnancy is a major public health concern. This preventable risk factor threatens both the mother and baby. Routine perinatal care visits offer opportunities for healthcare professionals to screen and refer abused women for effective interventions. It is, however, not clear which interventions best serve mothers during pregnancy and postpartum to ensure their safety.

Objectives

To examine the effectiveness and safety of interventions in preventing or reducing domestic violence against pregnant women.

Search methods

We searched the Cochrane Pregnancy and Childbirth Group's Trials Register (19 June 2012), scanned bibliographies of published studies and corresponded with investigators.

Selection criteria

We included randomised controlled trials (RCTs) including cluster-randomised trials, and quasi-randomised controlled trials (e.g. where there was alternate allocation) investigating the effect of interventions in preventing or reducing domestic violence during pregnancy.

Data collection and analysis

Two review authors independently assessed trial quality and extracted data.

Main results

We included nine trials with a total of 2391 women; however, for most outcomes very few studies contributed data and results were predominantly based on findings from single studies. There was evidence from one study that the total number of women reporting episodes of partner violence during pregnancy, and in the postpartum period was reduced for women receiving a psychological therapy intervention (risk ratio (RR) 0.62, 95% confidence interval (CI) 0.48 to 0.88). There were few statistically significant differences between intervention and control groups for depression during pregnancy and the postnatal period. Only one study reported findings for neonatal outcomes such as preterm delivery and birthweight, and there were no clinically significant differences between groups. None of the studies reported results for other secondary outcomes: Apgar score less than seven at one minute and five minutes, stillbirth, neonatal death, miscarriage, maternal mortality, antepartum haemorrhage, and placental abruption.

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Authors' conclusions

There is insufficient evidence to assess the effectiveness of interventions for domestic violence on pregnancy outcomes. There is a need for high-quality, RCTs with adequate statistical power to determine whether intervention programs prevent or reduce domestic violence episodes during pregnancy, or have any effect on maternal and neonatal mortality and morbidity outcomes.

PLAIN LANGUAGE SUMMARY

Preventing or reducing partner violence against women during pregnancy

Violence against women by partners during pregnancy is a major public health concern. It can cause physical and psychological harm to women, and lead to pregnancy complications and poor outcomes for babies. It is not clear what sort of intervention best serves women during pregnancy and after giving birth to ensure their safety. Interventions that might work include counselling and psychological therapy to give women more confidence and encourage them to make plans to avoid abuse, or referral to social workers, shelters and other community-based resources. For partners, referral can be made to batterer treatment programs.

Routine prenatal care offers opportunities for healthcare staff to identify women at risk of being abused so that they can offer interventions or refer women. In this review we included nine randomised trials involving a total of 2391 women, seven of which studied pregnant women who were at high risk of partner violence. The interventions examined in the studies included a single brief individualised consultation, case management and referral to a social care worker, and multiple therapy sessions during pregnancy and after birth. Due to the lack of data, and the different way outcomes were reported, we were unable to identify interventions that worked better than others. Studies focused on different outcomes and we were not able to pool information to draw conclusions about the overall effectiveness of the interventions. Most of the studies did not report on whether or not there had been any reduction in episodes of violence. There was evidence from a single study that the total number of women reporting partner violence during pregnancy and after birth was reduced for women receiving a psychological therapy intervention. Several of the studies examined whether women who received interventions were less likely to have depression after the birth of the baby, but the evidence was not consistent. Other outcomes for the baby such as reduced birthweight and preterm birth were reported in only one study, and the results did not show clear evidence that a therapy intervention improved outcomes. None of the studies reported results for important outcomes such as stillbirth, neonatal death, miscarriage, maternal deaths, antepartum haemorrhage, and placental abruption. More information is needed from well-conducted trials before any particular interventional approach can be recommended.

BACKGROUND

Description of the condition

Violence against women and girls is a major health and human rights concern (Beydoun 2010). Women can experience physical or mental abuse throughout their lifecycle, in infancy, childhood, adolescence, during adulthood or older age (Parker 1994; Petersen 1997). Violence has severe health consequences (Feder 2009), and is a social problem that warrants an immediate co-ordinated response from multiple sectors including healthcare providers and social workers (Goodwin 1990; Newberger 1992).

Violence against women by partners is referred to as domestic violence (DV), spousal assault, intimate partner violence (IPV), wife abuse, wife assault, and battered wife syndrome (Bohn 1996; McFarlane 1996). Most researchers define DV as threats of, or actual physical injury from hitting, slapping, punching, choking, kicking, injury with a weapon, or otherwise injuring an intimate partner (Browne 1997; Campbell 1992; Parker 2002; Stark 1999). Assault and coercive behaviours include physical, sexual, and psychological/emotional attacks, and threats against property, children and pets, economic coercion, and many more such acts. Some are injurious and criminal in nature, while others are not (Hedin 2000). The consequences of abuse are varied, and women suffering DV do not present with a particular set of symptoms. Given this variation, the concept of DV should not be conceptualised as a disease or syndrome, nor should it be considered as a specific health problem (e.g. injury or reproductive, physical, or mental health problem). In fact, more often than not, victims demonstrate strength and ability to take care of themselves and their infant in spite of often untenable situations. However, it is clear that abuse

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puts the victim at greatly increased risk of a multitude of physical and mental health problems (Howard 2010).

Although estimating the prevalence of DV is difficult (Ballard 1998) and estimates vary, especially during pregnancy, it is likely that most providers of women's healthcare services will encounter many pregnant women who are survivors of DV. For almost 30% of women who experience DV, the first incident occurs in pregnancy (Rodriguez 2001). The prevalence of physical abuse during pregnancy varies around the world: in Canada, reported prevalence ranges from 1.0% to 10.9% (Daoud 2012); in the United States, 0.9% to 20.1% (Gazmararian 1996); in the United Kingdom, 1.8% at booking, 5.8% at 34 weeks of gestation and 5.0% at 10 days postpartum (Bacchus 2004); in Sweden, 4.3% (Hedin 1999); in South Africa, 6.8% (Jewkes 2001); and Jejeebhoy 1998 has reported high rates of abuse in India. The prevalence of psychological and sexual maltreatment of women during pregnancy has also been reported at between 13% and 60% (Hedin 1999; Jahanfar 2007; Valladares 2005). DV is reported within all socio-economic class groupings, but it is most prevalent within the working and lower middle socio-economic classes (Babu 2009; Nagassar 2010).

Abuse during pregnancy is of particular concern because it is a threat to both maternal and child health (Lewis 2007; Lewis 2011; Shah 2010). It directly (e.g. via trauma to the abdomen) and indirectly affects the mortality and morbidity of fetus and mother. Other health-related problems and adverse economic circumstances enhance the risk of adverse pregnancy outcomes. It is a chronic problem for mothers and infants as violence exposure tends to continue after pregnancy (Taft 2009b).

Studies to date have demonstrated that physical abuse before, during, and after pregnancy is associated with reproductive health problems such as sexually transmitted diseases (Rodriguez 2001), urinary tract infection (Gazmararian 1996), depression, substance abuse (Rose 2010) and other mental health problems (Browne 1997; Canterino 1999; El 2005). Domestic violence is associated with a higher incidence of unwanted pregnancy (Browne 1997; Parker 2002) and intentional abortions (Canadian Centre for Justice Statistics 2000).

There are many negative effects of DV on pregnancy. The following harms have been clearly documented: maternal deaths (Lewis 2007; Lewis 2011; Saltzman 2003), low birthweight (Chamberlain 2000; Jewkes 2001; Lipsky 2003), placental abruption (Hedin 2000), preterm labour and delivery (Harwin 2006), fetomaternal haemorrhage, fetal death (Mezey 2000), intrauterine growth restriction (Janssen 2003), pregnancy complications due to trauma (Jejeebhoy 1998), miscarriage (Chamberlain 2000), maternal infections, and poor weight gain (Wiist 1999). In addition, DV negatively affects pregnant women's health behaviours (World Health Organization 2000) leading to delayed entry into prenatal care or to women seeking no care at all (Diaz-Olavarrieta 2002), and increases behavioural risks such as the use of tobacco, alcohol, and illicit drugs, and poor maternal nutrition (Bacchus 2004; Family Violence Prevention Fund 1999; Ng 2005; Parsons 2000; Wathen 2003).

Physical injuries to fetuses and infants, such as bruising, broken bones, and stab wounds, as well as death, have also been described (Ezechi 2004; Valladares 2005). Child abuse is also reported more often among families with a history of DV (Feldhaus 1997) and antenatal violence is associated with an increased risk of child behavioural problems (Flach 2011).

Description of the intervention

There are a number of interventions that have been examined in relation to violence prevention for pregnant women. A review by Sharps 2008 suggested that perinatal home-visiting programs are likely to reduce the incidence of physical abuse and improve pregnancy and infant outcomes. Several studies show that interventions such as wallet-size cards with community resources listed, spending time in a shelter, individual counselling, and home social support programs, alone or in combination, may decrease physical abuse (McFarlane 2006; Parker 1999). A review focusing on women recruited in DV shelters or refuges suggests that intensive advocacy may reduce physical abuse one to two years after the intervention (Ramsay 2009). There is currently no systematic review examining interventions specifically focusing on pregnant women.

During routine prenatal checkups, the clinician has the opportunity to screen women and then refer to various intervention programs. For women, both screening and intervention programs could lead to referral of identified individuals to appropriate healthcare specialists or agencies for support such as referral to social workers, shelters, counselling or other community-based resources. For partners, referral can be made to batterer treatment programs. The effectiveness of these programs is not clear (Arias 2002).

Available studies to date have investigated the effectiveness of DV screening on reduction of violence or improving women's health outcomes (Feder 2009; Nelson 2012; Spangaro 2010), but these studies have not investigated pregnancy outcomes.

It is clear that unless DV risk is reduced, screening efforts are of little use. Thus reviews investigating the effectiveness of screening alone are relevant to the topic in hand and worth mentioning. Acceptability and effectiveness of screening for women presenting in prenatal clinics has been studied (Ramsay 2002) and findings suggest that screening programs in antenatal clinics generally increased rates of identification of women experiencing DV. More recent studies provide evidence that universal screening is associated with improved pregnancy outcomes (Coker 2012). Screening programs that took a comprehensive approach (i.e., incorporated multiple program components, including institutional support) were successful in increasing DV identification rates (O'Campo 2011). This evidence suggests that screening for DV may be a useful component of routine antenatal assessment (Janssen 2006).

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How the intervention might work

Often, the goal of intervention is to reduce further abuse. Some interventions are designed to improve women's empowerment and to enhance their independence and control. Some attempt to keep women from danger of extreme violence and teach women how to stay safe. Generally, safeguarding women from harm, managing symptoms, conducting a safe communication with others when in an abusive relationship, increasing women's confidence, and improving family networks and relationships are the major objectives of interventional programs (Ford-Gilboe 2011). Healthcare providers may make positive contributions to women's access to special services designed to reduce violence. These interventions may reduce women's exposure to violence and more generally improve women's health (Kramer 2004; McCloskey 2006). Reducing the contact between partners in violent relationships also reduces opportunities for further abuse and potential harmful activities (Dugan 2003).

Why it is important to do this review

Current literature on the subject is inconclusive (O'Reilly 2010). Some reviews have concluded that there is insufficient evidence to show whether or not interventions or screening are effective (Nelson 2012). Conversely, Horiuchi 2009 has suggested that screening and interventions for pregnant women would be beneficial. It is therefore necessary to obtain a more comprehensive review of the existing evidence to identify the benefit or harm attributed to commonly practiced interventions to prevent or reduce DV.

Moreover, pregnancy is a unique window of opportunity to screen for DV. Women may welcome the opportunity to be asked about DV (Gazmararian 1996), although they need to be able to trust the care giver and be assured of confidentiality of the information exchanged (Gazmararian 2000). Healthcare professionals are in a unique position to identify and assist women during pregnancy.

OBJECTIVES

The objective of this review was to examine the effectiveness and safety of interventions in preventing or reducing domestic violence against pregnant women.

METHODS

Criteria for considering studies for this review

Types of studies

Randomised controlled trials including cluster-randomised trials and quasi-randomised controlled trials (e.g. where there was alternate allocation) investigating the effect of interventions in preventing or reducing domestic violence during pregnancy.

Types of participants

Pregnant women of any age at any stage of pregnancy and their partners (if the intervention involved them). We planned to include studies that recruited both pregnant and non-pregnant women, provided that data were reported separately for pregnant women, and would consider the data reported for pregnant women only in our analysis (no such studies were identified for this version of the review).

Types of interventions

Any intervention without screening or with screening (for those who screen positive) provided during pregnancy and aimed at preventing or reducing the number of episodes of domestic violence. Studies could include interventions carried out in any setting, including healthcare services and community-based studies.

Types of outcome measures

Although we focused on interventions during pregnancy, violence during pregnancy has an impact on the longer-term health of women and infants and we have included some outcomes measured in the postnatal period.

Primary outcomes

- Reduction of episodes of violence (physical, sexual, and/or psychological)
- Prevention of violence during and up to one year after pregnancy as defined by the authors of trials

Where data were available, we planned to undertake analysis of subgroups by severity of violence.

Secondary outcomes

Maternal and fetal outcomes

- Depression including prenatal or postnatal depression
- Miscarriage
- Antepartum haemorrhage
- Premature labour
- Abruptio placenta
- Maternal mortality

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Neonatal outcomes

- Birthweight
- Apgar score first minute
- Apgar score fifth minute
- Stillbirth
- Perinatal death.

Search methods for identification of studies

Electronic searches

We contacted the Trials Search Co-ordinator to search the Cochrane Pregnancy and Childbirth Group's Trials Register (19 June 2012).

The Cochrane Pregnancy and Childbirth Group's Trials Register is maintained by the Trials Search Co-ordinator and contains trials identified from:

1. monthly searches of the Cochrane Central Register of Controlled Trials (CENTRAL);
2. weekly searches of MEDLINE;
3. weekly searches of EMBASE;
4. handsearches of 30 journals and the proceedings of major conferences;
5. weekly current awareness alerts for a further 44 journals plus monthly BioMed Central email alerts.

Details of the search strategies for CENTRAL, MEDLINE and EMBASE, the list of handsearched journals and conference proceedings, and the list of journals reviewed via the current awareness service can be found in the 'Specialized Register' section within the editorial information about the Cochrane Pregnancy and Childbirth Group.

Trials identified through the searching activities described above are each assigned to a review topic (or topics). The Trials Search Co-ordinator searches the register for each review using the topic list rather than keywords.

Searching other resources

We contacted organisations that work in the area of violence for further trials. We also checked references of retrieved articles.

We did not apply any language restrictions.

Data collection and analysis

Selection of studies

Two review authors independently assessed for inclusion all the potential studies we identified as a result of the search strategy. We identified titles, abstracts and then full papers individually to retrieve the suitable studies. We resolved any disagreement through discussion or, if required, we consulted a third review author.

Data extraction and management

We designed a form to extract data. For eligible studies, at least two review authors extracted the data using the agreed form. We resolved discrepancies through discussion or, if required, we consulted a third person. We entered data into Review Manager software (RevMan 2011) and checked for accuracy.

When information regarding any of the above was unclear, we attempted to contact authors of the original reports to provide further details.

Assessment of risk of bias in included studies

Two review authors independently assessed risk of bias for each study using the criteria outlined in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011). The tool used for this purpose is a validated standardised instrument. We resolved any disagreement by discussion or by involving a third assessor.

(1) Sequence generation (checking for possible selection bias)

We have described for each included study the method used to generate the allocation sequence in sufficient detail to allow an assessment of whether it should produce comparable groups.

We assessed the method as:

- low risk of bias (any truly random process, e.g. random number table; computer random number generator);
- high risk of bias (any non-random process, e.g. odd or even date of birth; hospital or clinic record number);
- risk of bias unclear.

(2) Allocation concealment (selection bias)

We have described for each included study the method used to conceal the allocation sequence to determine whether intervention allocation could have been foreseen in advance of, or during recruitment, or changed after assignment.

We assessed the methods as:

- low risk of bias (e.g. telephone or central randomisation; consecutively numbered sealed opaque envelopes);
- high risk of bias (open random allocation; unsealed or non-opaque envelopes, alternation; date of birth);
- risk of bias unclear.

(3) Blinding (performance bias and detection bias)

For interventions to prevent or reduce domestic violence, blinding study participants and staff providing care may be very difficult.

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However, we have described for each included study the methods used, if any, to blind study participants and personnel from knowledge of which intervention a participant received. We considered that studies were at low risk of bias if they were blinded, or if we judged that the lack of blinding could not have affected the results. We assessed blinding separately for different outcomes or classes of outcomes.

We assessed the methods as:

- low, high or unclear risk of bias for participants and staff;
- low, high or unclear risk of bias for outcome assessors.

(4) Incomplete outcome data (attrition bias)

We have described for each included study, and for each outcome or class of outcomes, the completeness of data including attrition and exclusions from the analysis. We state whether attrition and exclusions were reported, the numbers included in the analysis at each stage (compared with the total randomised participants), reasons for attrition or exclusion where reported, and whether missing data were balanced across groups or were related to outcomes. Where sufficient information was reported, or supplied by the trial authors, we re-included missing data in the analyses. We assessed methods as:

- low risk of bias (less than 20% missing data);
- high risk of bias (more than 20% missing data);
- risk of bias unclear.

(5) Selective reporting bias

We have described for each included study how we investigated the possibility of selective outcome reporting bias and what we found.

We assessed the methods as:

- low risk of bias (where it is clear that all of the study's pre-specified outcomes and all expected outcomes of interest to the review have been reported);
- high risk of bias (where not all the study's pre-specified outcomes have been reported; one or more reported primary outcomes were not pre-specified; outcomes of interest were reported incompletely and so could not be used; study failed to include results of a key outcome that would have been expected to have been reported);
- risk of bias unclear.

(6) Other sources of bias

We have described for each included study any important concerns we had about other possible sources of bias.

We assessed whether each study was free of other problems that could put it at risk of bias:

- low risk of other bias;
- high risk of other bias;
- risk of other bias unclear.

(7) Overall risk of bias

We have made explicit judgements about whether studies are at high risk of bias, according to the criteria given in the *Handbook* (Higgins 2011). With reference to (1) to (6) above, we assessed the likely magnitude and direction of the bias and whether we considered it was likely to impact on the findings. We explored the impact of the level of bias through undertaking sensitivity analyses - *see* Sensitivity analysis.

Measures of treatment effect

Dichotomous data

For dichotomous data, we have presented results as summary risk ratio (RR) with 95% confidence intervals and for adverse outcomes a RR of less than one signifies that results favour the group receiving the intervention.

Continuous data

For continuous data, we used the mean difference (MD) as outcomes were measured in the same way between trials. We planned to use the standardised mean difference to combine trials that measured the same outcome, but used different methods. In the case of violence measured on the Conflict Tactics Scale a higher score denotes more abuse, and a negative MD therefore indicates that results favoured the experimental group (i.e. reported partner abuse was lower in the group receiving the intervention compared with controls). Similarly, in the case of depression measured as a continuous variable, a negative MD on either the Beck Depression Inventory or the Edinburgh Postnatal Depression Scale (EPDS) indicates that depression scores were lower (better) in the intervention group.

Unit of analysis issues

Cluster-randomised trials

We planned to include cluster-randomised trials in the analyses along with individually-randomised trials. No such trials were identified in this version of the review. However, if such trials are identified for updates we will adjust their sample sizes using the methods described in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011) using an estimate of the intra cluster correlation co-efficient (ICC) from the trial (if possible), from a similar trial or from a study of a similar population. If we use ICCs from other sources, we will report this and conduct sensitivity analyses to investigate the effect of variation in the ICC. If we identify both cluster-randomised trials and individual-randomised trials, we plan to synthesise the relevant information.

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We will consider it reasonable to combine the results from both if there is little heterogeneity between the study designs and the interaction between the effect of intervention and the choice of randomisation unit is considered to be unlikely.
We will also acknowledge heterogeneity in the randomisation unit and perform a sensitivity analysis to investigate the effects of the randomisation unit.

Dealing with missing data

For included studies, we noted levels of attrition. We planned to explore the impact of including studies with high levels of missing data (with more than 20% attrition) in the overall assessment of treatment effect by using sensitivity analysis. We have not carried out this additional analysis in this version of the review as for most outcomes there were insufficient data to allow us to carry out meta-analysis. We will carry out sensitivity analysis in future updates if more data become available.
For all outcomes, we carried out analyses, as far as possible, on an intention-to-treat basis, i.e. we attempted to include all participants randomised to each group in the analyses, and analysed all participants in the group to which they were allocated, regardless of whether or not they received the allocated intervention. The denominator for each outcome in each trial was the number randomised minus any participants whose outcomes were known to be missing.

Assessment of heterogeneity

We assessed statistical heterogeneity in each meta-analysis using the T^2, I^2, and Chi^2 statistics. We regarded heterogeneity as substantial if the I^2 was greater than 30% and either T^2 was greater than zero, or there was a low P value (less than 0.10) in the Chi^2 test for heterogeneity.

Assessment of reporting biases

If there were 10 or more studies in the meta-analysis we had planned to investigate possible reporting biases (such as publication bias) using funnel plots. In this version of the review there were insufficient data to allow us to carry out planned analysis. If more data are available for updates, we will assess funnel plot asymmetry visually. If we suspect asymmetry by visual assessment, we will perform exploratory analyses to investigate and report it.

Data synthesis

We carried out statistical analysis using the Review Manager software (RevMan 2011). We used fixed-effect meta-analysis for combining data where it was reasonable to assume that studies were estimating the same underlying treatment effect: i.e. where trials examined the same intervention, and the trials' populations and methods were judged sufficiently similar. If we had suspected clinical heterogeneity sufficient to expect that the underlying treatment effects would differ between trials, or if we had detected substantial statistical heterogeneity, we planned to use random-effects meta-analysis to produce an overall summary if an average treatment effect across trials was considered clinically meaningful. We would have treated the random-effects summary as the average range of possible treatment effects.
If in future updates we use random-effects analyses, we will present the results as the average treatment effect with its 95% confidence interval, and the estimates of T^2 and I^2.

Subgroup analysis and investigation of heterogeneity

In this version of the review there were insufficient data to allow for any meaningful exploration of heterogeneity or subgroup analysis. If more data are available for updates and we identify substantial heterogeneity, we will investigate it using subgroup analyses and sensitivity analyses.
Data permitting, we plan to carry out the following subgroup analyses.

1. Based on type of violence (physical, sexual, or psychological, or some combination thereof).
2. Based on the type of intervention (e.g. referral to shelter, home visits, community-based interventions, target-based interventions (partner or women, themselves)).
3. Based on socio-demographic characteristics of target population.
4. Based on the effect of low follow-up in the studies.
5. Based on severity of physical, psychological, and sexual violence (trialist defined).

We will use the following outcomes in subgroup analysis.

- Number of episodes of violence or decrease in violence.

Where sufficient information is available, we will assess differences between subgroups by interaction tests available in RevMan (RevMan 2011). We will report the results of subgroup analyses quoting the $\chi 2$ statistic and P value, and the interaction test I^2 value.

Sensitivity analysis

We assessed risk of bias in trials and planned to carry out sensitivity analysis temporarily omitting any trials with high risk of bias from the meta-analysis. In this version of the review, we did not carry out planned sensitivity analysis as too few studies contributed data, and for most outcomes, we did not pool data from more than one study. If more data are available for updates, we will consider trials at high risk of bias in sensitivity analysis if allocation concealment is unclear or at high risk of bias, or if attrition is greater than 20%. We will also carry out sensitivity analysis to explore the effects of fixed-effect or random-effects analyses for outcomes with statistical

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heterogeneity and the effects of any assumptions made, such as the value of the ICC used for cluster-randomised trials.

RESULTS

Description of studies

See: Characteristics of included studies; Characteristics of excluded studies; Characteristics of studies awaiting classification.

Results of the search

The search of the Pregnancy and Childbirth Group Trials Register retrieved 27 reports and, excluding duplicates, further searching identified an additional 10 reports for possible inclusion. A total of 27 trials (37 study reports) were assessed. After assessment we included nine trials and excluded 17. One study is awaiting further assessment (Loree 2008) and more information about this study is in the Characteristics of studies awaiting classification table.

Included studies

We included nine randomised trials involving 2391 women. The studies were predominantly carried out in the USA although there was one trial in Peru (Cripe 2010) and one in Hong Kong (Tiwari 2005).

Participants

Seven of the nine studies randomised women who were assessed during pregnancy and were identified as being at high risk of partner violence (Calderon 2008; Cripe 2010; Curry 2006; Kiely 2010; McFarlane 2000; Tiwari 2005; Zlotnick 2011). In the study by Olds 2004 the women recruited were not specifically at high risk of abuse; women were recruited if they were young (less than 19 years of age), unmarried or in receipt of Medicaid. In the Nagle 2002 trial it was not clear that participants were at high risk of partner violence.

Interventions and comparisons

The interventions in the included trials varied considerably and ranged from single, brief sessions through to multiple intensive sessions during pregnancy and extending into the postnatal period.

- Calderon 2008 described a single brief intervention which involved an interactive computer assessment and tailored advice encouraging women to seek help, and care providers were alerted of a high-risk status.
- In the trial by Cripe 2010, women received a single counselling session by social workers; Tiwari 2005 described a similar brief counselling intervention tailored specifically for Chinese women.
- In the Curry 2006 study, women in the intervention group received video advice and then individually tailored case management by a nurse with referral as appropriate.
- Two trials (Kiely 2010; Zlotnick 2011) described psychological therapy interventions involving multiple sessions during pregnancy with booster sessions in the postnatal period. In the Kiely 2010 trial, sessions were based on cognitive behavioural therapy while Zlotnick 2011 examined an intervention underpinned by theory relating to interpersonal psychotherapy.
- In a three-arm trial McFarlane 2000 compared a brief intervention with a counselling intervention, or a counselling intervention plus home visits.
- Nagle 2002 examined home visits during pregnancy and the postnatal period as did Olds 2004, although in this latter study one group received visits from a nurse and one from para-professionals.

The comparison groups mainly received usual care, although this may have included, or been supplemented by, written information on safety planning and, or a list of local resources where women could seek further help or advice on partner abuse.

Outcomes

A serious problem in this review was the lack of consistency in, and the limited range of outcomes reported, and the varied way that outcomes such as depression or experience of violence were measured. Only one of the included studies (Kiely 2010) reported episodes of partner abuse during pregnancy, and while four studies (Curry 2006; Kiely 2010; Tiwari 2005; Zlotnick 2011) reported some data on partner abuse in the early postpartum period (up to three months after the birth), we were unable to combine data in a meta-analysis as results from each study were reported in different ways. For example while Tiwari 2005 and Zlotnick 2011 both reported scores on the Conflict Tactics Scale in the postnatal period, Zlotnick 2011 reported overall scores whereas Tiwari 2005 reported scores for separate dimensions, and we were unable to collapse these results into a single score. In a further study, results on partner abuse were not reported in a way that allowed us to include them in data and analysis tables (mean results were reported without standard deviations and there was insufficient information to allow us to impute values) (McFarlane 2000); this study also included some data on the use of community resources. Several of the included studies did not report on episodes of abuse. Calderon 2008, for example, focused on whether or not women discussed partner violence with those providing care; it was not clear whether such discussions had any tangible effect. Nagle 2002 predominantly reported process outcomes, while Cripe 2010 focused on safety planning. In the study by Olds 2004, a multiplicity of outcomes were reported over a series of papers. These included

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partner violence along with pregnancy outcomes and long-term developmental outcomes in children, although it was not clear whether these outcomes were prespecified. Overall, there was little information on other review outcomes including depression and stress in pregnancy and the postnatal period, and outcomes for babies including birthweight and preterm birth.

Excluded studies

We excluded 17 studies. The main reason for excluding studies was that the interventions were not aimed at pregnant women. Three studies examined home visitation interventions to support women after the birth of the child; Armstrong 1999 focused on support by child health nurses, while Quinlivan 2003 examined visits by nurse-midwives during the period following the birth; Bair-Merritt 2010 looked at a parent support intervention over three years by para-professionals. None of these trials specifically aimed to reduce or prevent partner violence during pregnancy. In the study by Eddy 2008 the intervention was aimed at professionals rather than pregnant women, and in the Koziol-McLain 2010 trial women were recruited in hospital emergency departments and were not necessarily pregnant. Miller 2011 focused on women attending family planning clinics and the aim of the intervention was to prevent reproductive coercion. The study by Taft 2009a focused on women at high risk of abuse but included women with children under five, pregnant women and other women who were perceived as being at risk. Separate results were not reported for pregnant women.

In five studies, while the focus was on partner violence, participants were not randomly allocated to groups (Janssen 2003; Lipsky 2003; Macy 2007; McFarlane 1996; Parker 1999).

The remaining studies were excluded because the intervention was not designed specifically to prevent or reduce abuse (Blackmore 2006; Bullock 2009; Marcenko 1994). While Kataoka 2010 did focus on partner violence, the aim of the intervention was to identify the best means of increasing disclosure of abuse by women rather than to prevent abuse. One of the reports identified by the search was a trial registration for a study that did not take place (Janssen 2011).

Risk of bias in included studies

The studies were mixed in terms of overall risk of bias; while many of the studies used methods of sequence generation and allocation concealment that were at low risk of bias, blinding and sample attrition were frequent problems.

Allocation

Six of the included studies used methods of sequence generation that we assessed as low risk of bias (Calderon 2008; Kiely 2010; Nagle 2002; Olds 2004; Tiwari 2005; Zlotnick 2011); methods included computer-generated randomisation sequences, or the use of external randomisation services. In two trials the methods for generating the randomisation sequence were not clear (Cripe 2010; Curry 2006), and in one study the method was assessed as high risk of bias (McFarlane 2000) where group assignment was according to clinic.

Six studies were judged to use methods at low risk of bias for concealing allocation at the point of randomisation. Consecutively numbered opaque sealed envelopes were used in the trials by Tiwari 2005 and Zlotnick 2011, and external telephone randomisation services were utilised by Kiely 2010, Nagle 2002 and Olds 2004. In Calderon 2008 an automated interactive computer programme carried out randomisation. Methods were unclear for Cripe 2010 and Curry 2006, and the quasi-randomisation approach used for sequence generation in the McFarlane 2000 trial meant that it was judged as high risk of bias for allocation concealment as it was possible that allocation could be anticipated by those carrying out randomisation.

Blinding

Blinding women, clinical staff, staff providing interventions, and those collecting outcome data is very difficult for this type of intervention. Blinding was not attempted or not mentioned in five of the included studies, although Cripe 2010, Kiely 2010, Olds 2004; and Tiwari 2005 all reported attempting to blind outcome assessment. It was not clear whether this was successful. The overall impact of lack of blinding in these studies is difficult to assess. It is possible that for outcomes such as self-reported episodes of partner violence, the lack of blinding may have caused some response bias. For other outcomes, such as preterm birth, lack of blinding may have been less of a problem.

Incomplete outcome data

Loss of women to follow-up did not appear to be a serious problem in the studies by Cripe 2010, Olds 2004, Tiwari 2005, and Zlotnick 2011, although there were missing data for some outcomes in the Olds 2004 trial, and Zlotnick 2011 did not provide information on reasons for loss to follow-up. In the study by Curry 2006 more than 1000 women were randomised but results were reported only for the small sub-sample assessed as being at high risk of DV. In the trials by McFarlane 2000 and Nagle 2002 there were high levels of sample attrition. Loss of women to follow-up may be a serious problem even with low or modest sample attrition; it is possible that those women most at risk of poor outcomes (such as abuse) would be more likely than others to be lost to long-term follow-up (Higgins 2011).

In two studies women were assessed for a broad range of risk factors including smoking and other factors associated with poor pregnancy outcomes (Calderon 2008; Kiely 2010). The women randomised to partner violence interventions may have formed only

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a small proportion of the total sample randomised, and women may have received interventions for more than one risk factor. The impact of this on particular outcomes was difficult to assess. Multiple interventions may have had a synergistic or interactive effect, and for some outcomes, if women had more than one intervention, it would be difficult to ascertain which intervention led to any possible differences between groups. For example, women may have smoked and have been at high risk for DV, and therefore received multiple interventions; under these circumstances it would not be easy to disentangle which intervention, if any, influenced outcomes such as infant birthweight or preterm birth.

Selective reporting

It was difficult to assess outcome reporting bias without access to trial registrations and study protocols and most studies were judged to be at unclear risk of bias because we only had access to published study reports. We have already mentioned the difficulty interpreting outcomes with stratified samples and multiple interventions in the studies by Calderon 2008 and Kiely 2010. Interpreting findings from the Olds 2004 study was also hampered by possible outcome reporting bias. Several papers have been published on this trial and different papers focus on different outcomes, it was not clear that all outcomes reported were pre-specified, nor was it clear how different aspects of the interventions were associated with particular outcomes.

Other potential sources of bias

Most of the studies appeared to have comparable groups at baseline in terms of participant characteristics. Some of the studies provided little information on methods so assessment of overall risk of bias was difficult. We have set out findings for overall risk of bias in Figure 1 and for individual studies in Figure 2.

Figure 1. 'Risk of bias' graph: review authors' judgements about each risk of bias item presented as percentages across all included studies.

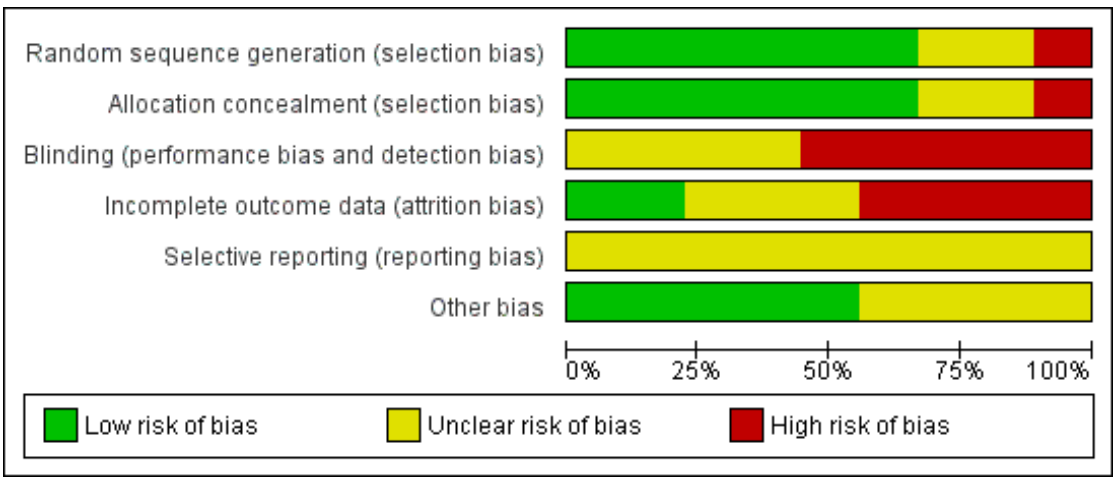

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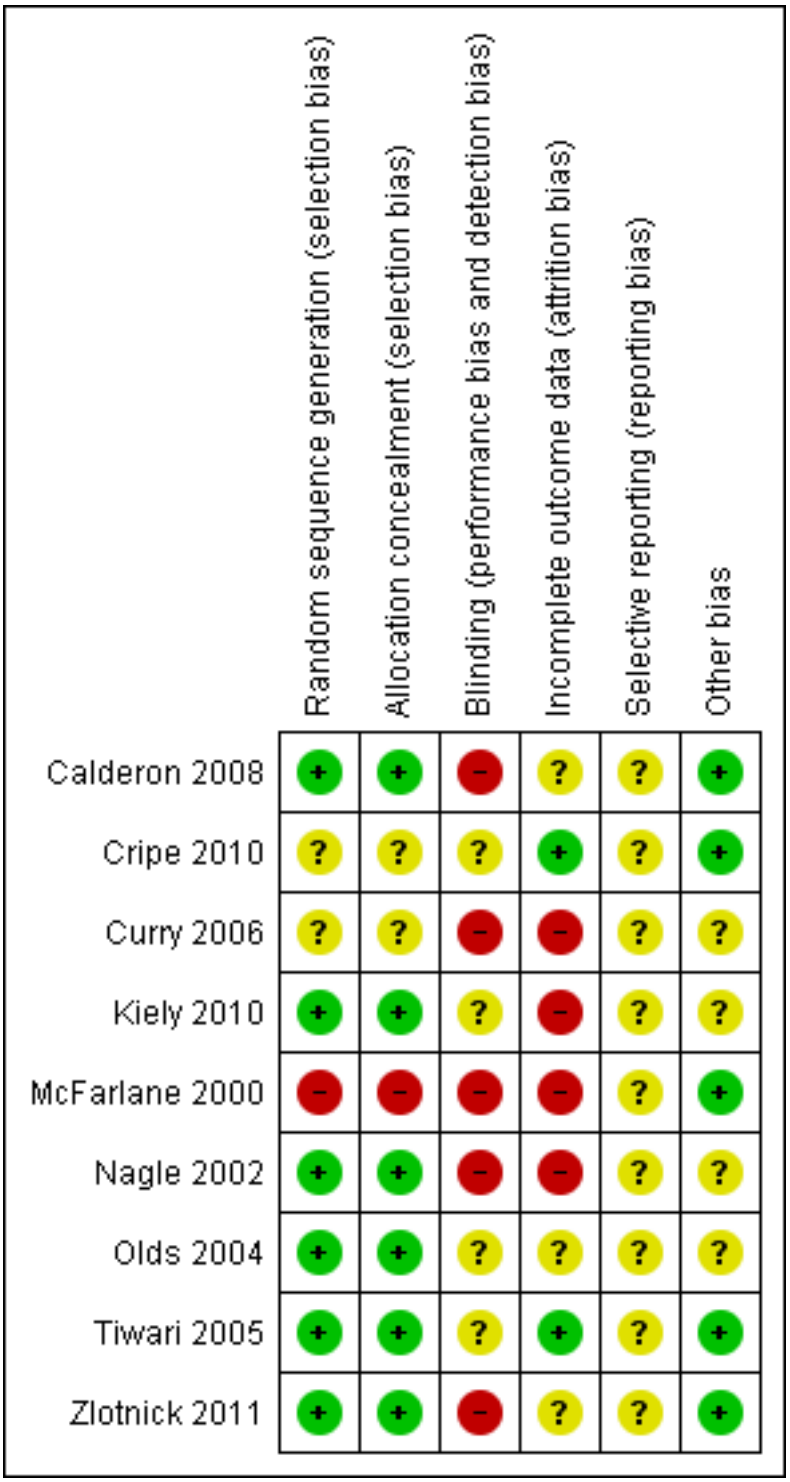
Figure 2. 'Risk of bias' summary: review authors' judgements about each risk of bias item for each included study.



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Effects of interventions

Due to lack of consistency in the outcomes reported in included studies, we were unable to combine results from trials in meta-analysis, and unless otherwise stated results are derived from single studies.

Primary outcomes

Kiely 2010 suggests that women randomised to the group receiving a psychological therapy intervention were less likely to have recurrent episodes of abuse during pregnancy compared with those receiving usual care, although the difference between groups did not reach statistical significance (risk ratio (RR) 0.50, 95% confidence interval (CI) 0.25 to 1.02) (Analysis 1.1). The protective effect of psychological therapy continued during the first three months of follow-up postpartum although again, results were not statistically significant (RR 0.60, 95% CI 0.35 to 1.04) (Analysis 1.2). In this study there was a significant difference between groups in the total number of women reporting DV at any point during pregnancy and/or in the postnatal period, with women in the intervention group being less likely to report abuse (RR 0.62, 95% CI 0.43 to 0.88) (Analysis 1.6).

In other studies examining DV in the postnatal period, findings were inconsistent, and most of the results were not statistically significant. The Zlotnick 2011 study examined a psychotherapy intervention and DV in the first three months after the birth was measured using the Conflict Tactics Scale; the difference between the intervention and control group was not statistically significant (mean difference (MD) 4.20, 95% CI -10.47 to 19.14) (Analysis 1.3). Differences between group scores for DV in the first three months postpartum in the study by Curry 2006 had a MD of -0.12 (95% CI -0.31 to 0.07); the evidence of a difference between groups receiving nurse case management or usual care was not statistically significant (Analysis 1.5).

Tiwari 2005 also used the Conflict Tactics Scale to assess DV in the first three months postpartum following a brief antenatal counselling intervention that focused on improving relationships with partners and strengthening social networks. For this study, mean scores on subscales measuring psychological, physical (minor and severe) and sexual abuse were reported separately. The intervention appeared to be effective in reducing minor physical violence (MD -0.46, 95% CI -0.82 to -0.10) and psychological abuse (MD -0.81, 95% CI -1.45,to -0.17). Severe physical violence and sexual abuse scores were not significantly different between the intervention and control groups (MD 0.08, 95% CI -0.28 to 0.44, and MD -0.09, 95% CI -0.24 to 0.06, respectively) (Analysis 1.4).

Nagle 2002 examined the effects of a nurse home-visiting intervention and found no significant difference between groups for the number of women reporting DV at seven to eight months postpartum (Analysis 1.7).

In a study examining a counselling intervention with or without support from a mentor compared with a brief intervention, authors reported that the severity of abuse decreased over the study period in all groups. However, there were no clear differences between different intervention groups for mean physical violence scores at up to 18 months postpartum (McFarlane 2000). (We have not included data from this study in the data and analysis tables as standard deviations were not reported, and we did not have sufficient information to impute values.)

Secondary outcomes

There was no strong evidence that risk of a major depression episode during pregnancy was lower in the intervention group than in the control group in the study by Zlotnick 2011 (RR 0.42, 95% CI 0.04 to 4.31) (Analysis 1.8) and there was no statistically significant difference in the mean number of depression episodes (Psychiatric Status Rating) (MD -0.46, 95% CI -0.97 to 0.05) (Zlotnick 2011) (Analysis 1.9).

The number of women with depression at up to three months postpartum was reported in two studies examining empowerment training and interpersonal psychotherapy respectively (Tiwari 2005; Zlotnick 2011). In view of differences between the interventions and statistical heterogeneity in findings (I^2 = 66%), we decided not to pool results from these studies and have reported sub-totals only. Tiwari 2005 reported a positive treatment effect following a brief prenatal intervention (RR 0.39, 95% CI 0.20 to 0.75), whereas, an interpersonal psychotherapy intervention involving multiple sessions (Zlotnick 2011) did not appear reduce the number of women with depression (RR 1.40, 95% CI 0.38 to 5.18) (Analysis 1.11). Zlotnick 2011 also reported mean scores on the Edinburgh Postnatal Depression Scale at three months postpartum and identified no clear difference between groups (MD -1.88, 95% CI -5.24 to 1.48) (Analysis 1.12).

Nagle 2002 reported the number of women with depression (scoring greater than 16 on the Beck Depression Inventory) and mean depression scores at seven to eight months postpartum; there was no strong evidence of differences between groups for either outcome (RR 0.85, 95% CI 0.37 to 1.98, and MD -0.65, 95% CI -2.41 to 1.11, respectively) (Analysis 1.13; Analysis 1.14).

The study by Kiely 2010 and colleagues examining the effectiveness of a psycho-behavioural intervention was the only one that reported neonatal outcomes. Mean birthweight was similar for babies whose mothers were in the intervention group who received individually-tailored therapy sessions compared with women receiving usual care (3139 g ± 593 versus 3098 g ± 717) (MD 41.00, 95% CI -106.19 to 188.19) (Analysis 1.23). The number of low birthweight babies (less than 2500 g) was also similar in the two

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groups (RR 0.74; 95% CI 0.41 to 1.32) (Analysis 1.24). The intervention was not associated with any significant reduction in the overall number of preterm births in this study (RR 0.69, 95% CI 0.40 to 1.20) (Analysis 1.25), although there was a significant increase in mean gestational age at delivery for women in the intervention group (MD 1.40 weeks, 95% CI 0.33 to 2.47) (non-prespecified outcome, Analysis 1.26).

Other secondary outcomes

None of the studies reported results for several of our secondary outcomes: Apgar score less than seven at one minute, and five minutes; stillbirth, neonatal death, miscarriage, maternal mortality, antepartum haemorrhage, and placental abruption.

Non-prespecified outcomes

Several studies reported data on outcomes that we had not pre-specified. Cripe 2010 examined an empowerment intervention in pregnancy compared with usual care. Results showed that women in the intervention group were more likely to make plans to avoid abuse by adopting safety behaviours (RR 2.60, 95% CI 1.41 to 4.79) (Analysis 1.27).

Curry 2006 investigated active case management by nurses aiming to reduce stress among pregnant women, and reported some reduction in stress scores for women receiving the intervention (MD -2.06, 95% CI -3.34 to 0.78) (Analysis 1.28).

Calderon 2008 examined an intervention that aimed to increase identification of women suffering violence in pregnancy and reported that the intervention led to more women discussing abuse with their healthcare providers (Analysis 1.29).

Women were followed up over several years following home visits by public health nurses during pregnancy and the postnatal period in the study by Olds 2004. It was not clear in this study whether or not prevention or reduction of DV was a pre-specified outcome, nor whether the intervention was tailored for women at risk of abuse. The number of women reporting violence two to four years after the birth was not significantly different in the intervention and control groups (Analysis 1.30).

DISCUSSION

Summary of main results

Our review set out to examine the effectiveness of interventions in preventing or reducing domestic violence (DV) against pregnant women. In this review, there were nine identified studies of prevention of DV in pregnancy (Calderon 2008; Cripe 2010; Curry 2006; Kiely 2010; McFarlane 2000; Nagle 2002; Olds 2004; Tiwari 2005; Zlotnick 2011), seven of which studied pregnant women who were at high risk of partner violence (all except Nagle 2002 and Olds 2004). Six of these studies reported on at least one of our pre-specified outcomes (Cripe 2010; Curry 2006; Kiely 2010; Nagle 2002; Tiwari 2005; Zlotnick 2011). The interventions examined in the studies varied significantly and included a single brief individualised consultation, case management and referral to social care workers, and multiple therapy sessions during pregnancy and postpartum.

Results for many outcomes were not consistent and most differences between groups were not statistically significant. We were not able to combine results from different studies, in the hope of identifying patterns among study results, source of disagreement (if any) among results, or other interesting relationships that may appear in the context of a meta-analysis. Due to the lack of data and the disparate way outcomes were reported, we were unable to single out one intervention that works better than the others.

In one study, compared with women receiving usual care, women receiving a psychological therapy intervention were less likely to report DV at any point during pregnancy and/or in the postnatal period (Kiely 2010). An intervention which aimed to improve women's relationships with their partners and strengthen social networks slightly reduced psychological abuse and minor physical violence scores, but had no significant effect on severe physical violence scores (Tiwari 2005). There was no strong evidence that an educational video focusing on abusive relationships along with tailored case management was effective in reducing intimate partner abuse in the first three months postpartum.

Several trials examined the effects of interventions on postpartum depression but results were inconsistent.

There was very little information on outcomes relating to pregnancy complications and neonatal outcomes. The non-significant effect of psycho-behavioural intervention on birthweight or preterm birth could be due to the reported effect measures being unadjusted for the confounding effect of gestational age (Kiely 2010).

We did not find any evidence that interventions had a negative, harmful effect.

Overall completeness and applicability of evidence

There was a limited range and lack of consistency in the outcomes reported in the studies included in the review, and in cases where studies measured and reported similar outcomes (such as frequency of violence or depression), they were measured with various tools and at different time points. This meant we were unable to carry out meta-analysis and this limited our ability to draw conclusions about the overall effect of interventions.

We also noted the paucity of data on various important outcomes that should be the centre of attention for healthcare providers during pregnancy. Only one study investigated outcomes related

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to newborn babies (Kiely 2010) and none investigated maternal pregnancy outcomes such as miscarriage, maternal mortality, antepartum haemorrhage, and placental abruption.

Quality of the evidence

The risk of bias of the included trials was mixed.
Only six of the included studies used adequate randomisation techniques or allocation concealment. The nature of interventions was such that they did not allow for blinding of women and the staff providing care, thus compromising the validity of study results (i.e. lack of blinding may have led to response bias for outcomes such depression or frequency of DV episodes). Loss of follow-up was not a major problem in trials for pregnant women as the clinicians had enough time to assess the risk of DV, implement interventions and collect outcome data during routine prenatal and postpartum visits, although sample attrition was a source of concern in some of the included studies (Curry 2006; Kiely 2010; McFarlane 2000; Nagle 2002).

Potential biases in the review process

We are aware that there was a risk of introducing bias at all stages of the review process and we took various steps to minimise this. Two review authors assessed eligibility, risk of bias and carried out data extraction and a third author checked assessments. Data were entered by one review author and were checked by a second review author. However, assessing risk of bias, for example, requires individual judgement about the impact of bias on outcomes, so it is possible that a different review team may not have agreed with all of our assessments.

Agreements and disagreements with other studies or reviews

Our conclusions are compatible with the only review (O'Reilly 2010) that appraised both the effect of screening and preventive interventions for DV. The latter part of the review looked at four studies (El-Mohandes 2008; McFarlane 2000; Parker 1999; Tiwari 2005) focusing on the effectiveness of interventions on prevention of DV. We did not include one of these trials in this review for methodological reasons (Parker 1999).

AUTHORS' CONCLUSIONS

Implications for practice

There was so much variation in the study outcomes examined that we were unable to combine study findings to develop a summary measure of the effectiveness and safety of interventions to prevent or reduce violence against pregnant women. None of the studies reported neonatal mortality and important morbidity outcomes.

Implications for research

The studies included in our analysis examined different interventions, reported on a limited range of outcomes, and varied in the way in which outcomes were measured, and we were unable to conduct a meta-analysis. With so few good quality research studies on the topic, it is evident that more high-quality studies are required.

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As part of the pre-publication editorial process, this review has been commented on by three peers (an editor and two referees who are external to the editorial team), a member of the Pregnancy and Childbirth Group's international panel of consumers and the Group's Statistical Adviser.

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* *Indicates the major publication for the study*

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CHARACTERISTICS OF STUDIES

Characteristics of included studies *[ordered by study ID]*

Calderon 2008

Methods	Stratified RCT. Women were stratified by risk factor combination (1 of 15 possible combinations of 4 risk behaviours (IPV, alcohol, drugs, smoking))
Participants	Women attending 5 prenatal clinics in the San Francisco bay area (USA). 37 women experiencing DV were randomised Inclusion criteria: women less than 26 weeks' gestation, English speaking, aged 18 years or older, not attending for first prenatal visit who screened positive for one (or more than one) of 4 risk factors (smoking, alcohol, drug use or DV) Exclusion criteria: women who had no risk factors were not randomised
Interventions	Experimental intervention: video doctor assessment with appropriate messages for risk factor (e.g. encouraging women to discuss problem) and healthcare staff were alerted and given a cueing sheet to discuss risk factor (DV). (20 women randomised to the intervention group.) Control/Comparison intervention: all women had video doctor assessment but healthcare staff did not receive cueing sheet. Women randomised to the control group received usual care and any discussion of risk was at the discretion of healthcare staff. (17 women randomised to control condition.)
Outcomes	This study examined whether discussions re DV occurred and women's views of the discussion (acceptability and helpfulness). No other outcome data were reported
Notes	

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Sequence generation was by a computer programme, however, it was not clear how stratification affected the randomisation process as some women may have had several risk factors (it was not clear whether these women would then receive several intervention messages and several staff cueing forms)
Allocation concealment (selection bias)	Low risk	By computer programme.
Blinding (performance bias and detection bias) All outcomes	High risk	Blinding was not mentioned.

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Calderon 2008 (*Continued*)

Incomplete outcome data (attrition bias) All outcomes	Unclear risk	37 women experiencing DV were randomised and data appear available for all women at the immediate post-intervention assessment. There were some missing data at follow-up (32 were followed up at 2 months) but all women were included in a sensitivity analysis (those lost to follow-up were assumed to have had no discussion of DV).
Selective reporting (reporting bias)	Unclear risk	Results for women assessed with a single risk factor were reported but it seems that women may have had multiple risk factors and multiple interventions and there may have been some interaction effect (it is possible that if a woman had multiple risk factors then she was more likely than those with a single risk factor to have discussions with healthcare staff).
Other bias	Low risk	Other bias not apparent.

Cripe 2010

Methods	RCT. 2-arm trial with individual randomisation.
Participants	Setting: public hospital in Lima, Peru, providing services to low-income women living in Lima. Recruitment in 2007. Inclusion criteria: 220 pregnant women (gestational age between 12 and 26 weeks' gestation) attending for care in the study hospital who screened positive for DV on the modified Abuse Assessment screen and able to speak and understand Spanish. (Women screened positive if they said yes to any of the following in the past year - been pushed, shoved, slapped, hit, kicked or otherwise physically hurt or been forced into sexual activity by a former or current partner.)
Interventions	Experimental intervention: (110 randomised) empowerment intervention during pregnancy which included standard care (a card with information about agencies providing IPV support). Women in the empowerment intervention received supportive counselling and education, and advice in the areas of safety by a trained social worker lasting about 30 minutes. Interviewers listened empathetically to the women and acknowledged their perceptions and feelings. Interviewers also helped women understand the cycle of violence and reviewed components of the safety plan including behaviours indicated in the Safety Behavior Checklist. For example, women were asked how they might secure and hide money and important documents such as birth certificates. Interviewers helped women develop a code to use with family and trusted friends to signal the need for assistance and/or to mentally plan their escape when needed. Women were given a brochure with a 13-item safety plan to reinforce safety behaviours. To make the safety plan brochure less conspicuous, other prenatal brochures on topics such as breastfeeding or nutrition were

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	also offered to the women. Interviewers also provided a list of community resources, such as emergency shelter, legal aid, law enforcement, and counselling, and strategies for seeking help from these resources. As part of the intervention, interviewers also offered to assist women with telephone calls to social service agencies or women's groups who could act as advocates for abused women. At the conclusion of the empowerment intervention session, interviewers helped women determine if it was safe for her to keep the safety plan brochure and the referral card. Women were free to discuss the pros and cons of leaving the abuser, reporting the abuser to law enforcement, or applying for a protection order Control/Comparison intervention: (110 randomised) women randomised to receive standard care received a wallet-size referral card listing agencies that provide DV services to abused women (e.g., legal, social services,and law enforcement). No counselling, advocacy, education, or other services were offered to women in this group during pregnancy. However, they were provided the empowerment intervention, specifically supportive counselling and education, and advice in the areas of safety by a trained social worker at the conclusion of the study during the 6th week postpartum visit
Outcomes	Physical, functional and emotional functioning at follow-up as compared with baseline. Use of community resources and safety behaviours. Episodes of violence in the past year were reported at baseline but data on this outcome were not reported at follow-up
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Unclear risk	Assignment was by a "randomised block design" but how the sequence was generated and block size were not stated
Allocation concealment (selection bias)	Unclear risk	Methods used to allocate women to groups at the point of randomisation were not described
Blinding (performance bias and detection bias) All outcomes	Unclear risk	Women and staff would be aware of the purpose of the study and which group they were assigned to. Post-intervention interviews were carried out by a different interviewer than those who carried out the pre-intervention ones to reduce bias. It was not clear what impact lack of blinding would have on the outcomes reported
Incomplete outcome data (attrition bias) All outcomes	Low risk	Sixteen women (7.3%) were lost to follow-up. 220 women were randomised and follow-up data were available for 204

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Cripe 2010 (*Continued*)

Selective reporting (reporting bias)	Unclear risk	Episodes of violence at follow-up were not reported in this paper (it may be that these outcomes will be addressed in future papers)
Other bias	Low risk	Groups were reported to be similar at baseline and there was no other bias apparent

Curry 2006

Methods	RCT, 2 arms with individual randomisation (2 sites).
Participants	1000 women who spoke English and were 13 to 23 weeks pregnant at the time of recruitment. At the completion of the first assessment, women were randomised to 1 of the 2 groups, 501 to the control group and 499 to the treatment group. Prenatal Psychosocial Profile test was used to estimate women's stress level. After women at risk of abuse with high level of stress were identified, 106 were found in the intervention group and 101 in the control group Study carried out in two prenatal clinics of a Health Maintenance Organisation in the USA (clinics both served what was described as a geographically, culturally and economically diverse group). Each clinic enrolled 500 women over the period 2001-2003 Exclusion criteria: adolescents for whom consent was not available
Interventions	Experimental intervention: standard care plus video about abuse (watched by < 30%) , 24/7 access to Nurse Case Management. Women were contacted by phone by nurse who actively managed their care. The intervention was intended to provide support and was individually tailored to women's needs Control/Comparison intervention: standard care which involved written information on community and health services resources for abused women. (Women assessed as being in danger were provided with safety planning and the offer of referral to the clinic social worker.)
Outcomes	Process outcomes (number of contacts, etc). The outcomes reported in this paper related to stress scores on Prenatal Psychosocial Profile measure (it was not clear what other outcome information was collected)
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Unclear risk	Not described.
Allocation concealment (selection bias)	Unclear risk	Not described.

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Curry 2006 (*Continued*)

Blinding (performance bias and detection bias) All outcomes	High risk	No information about blinding was provided - it is likely that women, care providers and outcome assessors would all be aware of the intervention
Incomplete outcome data (attrition bias) All outcomes	High risk	1000 women were enrolled on the study. 106 women in the intervention group and 101 women in the control group were assessed as being at high risk at the first study assessment and results were only provided for the high-risk group (i.e. approximately 20% of those randomised). Of the high-risk groups 99 of the high-risk intervention group and 92 of the high-risk control group provided follow-up data
Selective reporting (reporting bias)	Unclear risk	It was not clear what outcome data were collected.
Other bias	Unclear risk	There was very little information on methods and there may be further outcome data not published in this paper

Kiely 2010

Methods	A complex RCT with several arms and 4 different interventions targeting women with risk factors (depression, smoking, passive smoking and IPV). Women may have had more than 1 risk factor and may have been randomised to receive more than 1 intervention. Women at risk of IPV were randomised into intervention and control arms**.**
Participants	Setting: 6 community prenatal clinics serving mainly African-American women in Washington DC. July 2001-2003 Inclusion criteria: women from minority groups (mainly African-American) aged at least 18 years, 28 weeks pregnant or less, English speaking and resident in the study area Exclusion criteria: women who were identified as suicidal at baseline or follow-up were excluded
Interventions	Experimental intervention: cognitive behavioural intervention focusing on 4 risk factors (smoking, passive smoking, depression and DV). Women received an intervention specifically focusing on their individual risk factors (most women had more than one risk factor and would receive more than 1 intervention component. 336 women reported DV and 169 were randomised to the DV intervention group. The intervention was delivered as part of routine prenatal visits by psychologists or social workers. The intervention was based on empowerment theory and emphasised safety planning and behaviours and a list of phone numbers for community resources was provided. The intervention took place over several sessions lasting about 30 minutes and women received a small incentive for attending sessions. There were 2 postpartum booster sessions to reinforce messages. 51% of women in the intervention group received 4 or more sessions and a quarter attended

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	none Comparison group: 167 of the women reporting DV received standard care according to protocols at each clinic
Outcomes	DV was identified by Abuse Assessment Screen at baseline. Follow-up sessions used Conflict Tactics Scale to identify women at risk Episodes of DV during pregnancy and in the early postpartum period (minor and severe and sexual violence). Low and very low birthweight, gestational age at delivery, preterm and very preterm birth
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Computer-generated randomisation scheme with site and risk specific permuted block randomisation with investigators and staff blinded to block size
Allocation concealment (selection bias)	Low risk	Allocation by external data co-ordinating centre by telephone
Blinding (performance bias and detection bias) All outcomes	Unclear risk	Women and staff delivering intervention would be aware of group assignment. Telephone interviewers were reported to be blind to allocation but it was not clear whether this was successful (women may have revealed their allocation during interviews)
Incomplete outcome data (attrition bias) All outcomes	High risk	More than 1000 women were recruited to this trial and randomised. Randomisation was stratified according to baseline risk and only those women with DV risk were randomised for the DV intervention. (In this review we have reported findings for those women identified at risk of DV and randomised to receive or not receive the DV intervention; If we carried out analysis using all women randomised any intervention effect would be diluted considerably) . 336 with DV risk had baseline data but there were considerable amounts of missing data at follow-up, although there were some outcome data for at least 1 of the follow-up interviews for 306 women and out-

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		come data for babies were available for 306 women. It was reported that women were analysed by randomisation group whether or not they received the planned intervention
Selective reporting (reporting bias)	Unclear risk	Most women had more than 1 risk factor and were likely to receive different interventions - the interventions may have had some synergistic or interactive effect. But results are reported only by single risk factors - i.e. this paper only focuses on women reporting DV at baseline.
Other bias	Unclear risk	Intervention and control group characteristics appeared similar at baseline. It was no clear how many woman received multiple interventions or whether women in the control group received other interventions to address risk factors other than DV

McFarlane 2000

Methods	3-arm trial. Quasi-randomisation (clinics rotated through different interventions; "monthly sequential assignment
Participants	Setting: 2 prenatal clinics in SW USA. Each clinic served 2000-3000 pregnant women each year and 97% were Hispanic Inclusion criteria: women who reported abuse in the year prior to or during current pregnancy by current or former male partner (mean gestational age at recruitment 18 weeks) Exclusion criteria: not described.
Interventions	335 women agreed to participate. 96% women Hispanic - and only results for these 329 women are reported in this paper 3 interventions: 1. Brief intervention (control). Women were provided with a card with phone numbers for community resources to help with DV and information about personal safety planning. (No other counselling or education routinely offered) (n = 113). 2. Counselling intervention group: unlimited access to counsellor with expertise in DV. Women could drop-in to the maternity clinic or arrange appointments with the counsellor or reach the counsellor by phone or pager. The counsellor gave advice and support and assisted women in accessing other services. Counselling from recruitment in pregnancy up until delivery (n = 98). 3. Outreach intervention: same counselling intervention as group 2 plus trained lay mentor who offered support and assistance in accessing services. The mentor was available to visit or by phone. The intervention was from recruitment in pregnancy up until delivery (n = 118).

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Outcomes	Follow-up at 2, 6, 12 and 18 months post-intervention (i.e. after delivery). Outcomes were reported abuse and use of resources. Abuse on Severity of Violence against Women Scale (SVAWS) a 46 item scale; 19 items on threats of violence and 21 items on physical violence and 6 items on sexual violence with 4 point response re how often the behaviour occurred - never (1) to many times (4). Possible scores 19-76 on threats and 27-108 on violence
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	High risk	Women were allocated by clinic.
Allocation concealment (selection bias)	High risk	Women were allocated by clinic; it is possible that women attending the different clinics were different and the order in which staff delivered the interventions may have had an effect.
Blinding (performance bias and detection bias) All outcomes	High risk	No mention of blinding; lack of blinding may have affected responses and other aspects of care
Incomplete outcome data (attrition bias) All outcomes	High risk	Results only reported for Hispanic women in the sample. Results by intervention group were available for 259/329 women (79%) (> 20% missing data)
Selective reporting (reporting bias)	Unclear risk	Results in this paper for Hispanic women only - while they were 96% of the population it is not clear why other women were excluded or whether they were balanced across groups. Results are reported by gestational age at recruitment and over time - so there were a large number of possible correlations.
Other bias	Low risk	Groups were described as similar at baseline. Other bias not apparent

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Nagle 2002

Methods	3-arm RCT. Individual women randomised.
Participants	Setting: pregnant women attending state public health clinics in 3 parishes in Louisiana USA 1999-2000 Inclusion criteria: pregnant women less than 28 weeks' gestation with no previous live births and Medicaid eligible
Interventions	Experimental intervention: 2 intervention arms that were combined in the results 1. Nurse home visits with visits during pregnancy and up to the child being 2 years old. Details of the content of visits were not described in detail. 2. As 1 but the nurse home-visiting team included a mental health specialist. Control/Comparison intervention: usual care (not clearly described)
Outcomes	This thesis mainly reports on participation and adherence. Long-term outcomes included child development, abuse and neglect, injury, subsequent pregnancy, mother-child interaction and maternal employment At follow-up in the third trimester (28-34 weeks) and at 6-8 months postpartum results were described for depression (Beck Depression Inventory) and for partner violence (current and previous) (partner violence interview) with 13 items on physical violence (0 - never experienced, 1 - has experienced) (the time frame for reporting violence was not described)
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Randomisation was carried out by an external agency.
Allocation concealment (selection bias)	Low risk	It was stated that the nurses carrying out recruitment had no influence over randomisation which was carried out by an external agency (allocations were obtained by telephone)
Blinding (performance bias and detection bias) All outcomes	High risk	Participants and staff were aware of group assignment.
Incomplete outcome data (attrition bias) All outcomes	High risk	357 women consented to participation and were randomised (241 in intervention groups and 116 controls). There was considerable loss to follow-up and deviations from protocol. 19.5% of intervention women received no intervention. 32.8% did not complete the first follow-up interview at 28-34 weeks, and an additional 9%

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Nagle 2002 (*Continued*)

		of the women did not have this interview until after the birth of the baby and data were therefore not included in the analysis (206 followed-up at interview one and 181 available at the postpartum follow-up) . Loss was balanced across groups but this loss to follow-up and the large number of women not receiving the allocated intervention means that results are difficult to interpret
Selective reporting (reporting bias)	Unclear risk	This thesis concentrated on only a limited part of the overall research project. The large number of analyses means that some significant results may have occurred by chance
Other bias	Unclear risk	There was little information on some aspects of the trial design. We are not aware that other results from this trial have been published Groups appeared similar at baseline.

Olds 2004

Methods	RCT, 3-arm trial, individual randomisation.
Participants	Low-income, pregnant women with no previous live births referred to antenatal clinic in Denver (n = 735) were included in this study. These women were either qualified for Medicaid or had no private insurance. Setting: 21 public and private healthcare settings in Denver, Colorado USA 1994-5 Inclusion criteria: low-income women with no previous live births and qualified for Medicaid or had no private medical insurance
Interventions	Home visits were provided from pregnancy through to child age 2 years. The home-visiting program had 3 broad goals, (1) to improve maternal and fetal health during pregnancy by helping women improve their health-related behaviours; (2) to improve children's health and development by helping parents provide more competent care; and (3) to enhance mothers' personal development by promoting planning of future pregnancies and helping women continue their education and find work. The visitors helped women accomplish these goals by promoting the adaptive behaviours specified above, by helping them improve their relationships with key family members and friends (especially their mothers and boyfriends), and by promoting women's use of health and human services. Nurse home visitors were required to have a degree and experience in community or maternal and child health nursing, whereas para-professionals were expected to have a high school education, no college preparation in the helping professions, and strong people skills Women were randomised into 3 groups: Women in treatment 1 (n = 255) were

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	provided with free developmental screening and referral for their children at 6, 12, 15, 21, and 24 months of age. Women in treatment 2 (n = 245) were provided with the screening offered in treatment 1 plus para-professional home visiting during pregnancy and the child's first 2 years of life. Women in treatment 3 (n = 235) were provided with the screening offered in treatment 1 plus nurse home visiting during pregnancy and the child's first 2 years
Outcomes	Outcomes consisted of maternal reports of subsequent pregnancies, participation in education and work, use of welfare, marriage, cohabitation, experience of domestic violence, mental health, substance use, and sense of mastery; observations of mother-child interaction and the home environment; tests of children's language and executive functioning; and mothers' reports of children's externalising behaviour problems A large number of outcomes were reported in this study where follow-up continued into late childhood. The paper relating to the follow-up when the child was four years old collected data relating to child development, subsequent pregnancies, drug and alcohol use and episodes of DV during previous 6 months and since the child was 2 years (i.e. 3.5 years and 2-4 years postpartum)
Notes	The data reported in this review were from a paper reporting a follow-up study of mothers and children in their homes near the child's fourth birthday, 2 years after the end of the program

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**
Random sequence generation (selection bias)	Low risk	Stratified randomisation by computer by external operations office
Allocation concealment (selection bias)	Low risk	External randomisation service.
Blinding (performance bias and detection bias) All outcomes	Unclear risk	Women and staff would be aware of treatment group but it was reported that follow-up data were collected by investigators who were blind to group assignments
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	735 women randomised and 695 followed up at 4 years. There were missing data for some variables
Selective reporting (reporting bias)	Unclear risk	It was not clear that all outcomes had been prespecified before the onset of the study. The study resulted in multiple publications
Other bias	Unclear risk	Groups appeared comparable at baseline and follow-up rates were relatively high

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Tiwari 2005

Methods	RCT. 2-arm trial with individual randomisation.
Participants	Setting: public hospital in Hong Kong, May 2002-June 2003. Inclusion criteria: 110 women identified as abused by intimate partner. Pregnant women over 18 and less than 30 weeks' gestation attending for first antenatal visit. Women had "answered 'yes' to being physically or emotionally abused by someone or forced to have sexual activities within the last year" (Abuse Assessment Screen) Exclusion criteria: no reported abuse or abused by someone other than their partner
Interventions	Experimental intervention: 55 women (51 followed up) Intervention based on empowerment and empathic understanding. The aim of the intervention was to enhance independence and control. In a 30-minute interview with a midwife researcher women were advised on safety and problem solving (the intervention was tailored for use with Chinese women). Women were also given a brochure (it was not clear whether this intervention was in addition to standard care Control/Comparison intervention: 55 women (all followed up). Standard care. Women were given written information about community resources to support abused women
Outcomes	Conflict Tactics scale (CTS) mean scores (with scores for minor and severe physical violence and psychological and sexual abuse); General Health scores; EPDS; and satisfaction with intervention. Women were followed up by telephone interview at 6 weeks postpartum
Notes	

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	The allocation schedule was generated by a computer.
Allocation concealment (selection bias)	Low risk	Allocation was "concealed in consecutively numbered sealed envelopes" by a researcher not involved in the study.
Blinding (performance bias and detection bias) All outcomes	Unclear risk	Women and care providers would not be blind to randomisation group. It was stated that outcome assessment was carried out by researchers unaware of allocation and that women did not reveal their group until the end of the follow-up interview
Incomplete outcome data (attrition bias) All outcomes	Low risk	110 women were randomised (55 in each group). 4 women were lost to follow-up and it was stated that an intention-to-treat analysis was carried out although it was not clear what this meant

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Tiwari 2005 (*Continued*)

Selective reporting (reporting bias)	Unclear risk	Assessment from published study report.
Other bias	Low risk	No other sources of bias identified. There did appear to be differences between groups at baseline but it is not clear whether or to what extent baseline differences influenced results.

Zlotnick 2011

Methods	RCT. 2 arms with individual randomisation.
Participants	Women attending for prenatal care were recruited from 2 primary care clinics and a private clinic in Rhode Island (USA) Inclusion criteria: 54 pregnant women aged between 18 and 40 years who screened positive for recent (past year) DV on the Revised Conflict Tactics Scale. (Gestational age at recruitment not clear.) Exclusion criteria: women with a current affective disorder, post-traumatic stress disorder or current substance abuse were excluded and referred for appropriate treatment
Interventions	Experimental intervention: (28 women randomised.) an intervention based on principles of interpersonal psychotherapy which aimed to enhance social support as a means of reducing depression, encouraging service use and reducing partner violence. The intervention also included empowerment and stabilisation components. The intervention involved four 60 minute individual, scripted sessions during pregnancy and a booster session within 2 weeks of the birth; delivered by 2 trained staff Control/Comparison intervention: (26 women randomised.) usual medical care which included educational material and list of resources for DV Women in both groups received financial compensation for completing assessments
Outcomes	Assessed at baseline, 5-6 weeks after recruitment, 2 weeks after the birth and 3 months postpartum Physical, sexual and psychological attacks measured on Revised Conflict Tactic scale (at baseline and then since last assessment) Major depressive disorder (assessed on Longitudinal Interval Follow-up Examination (LIFE)) Postnatal depression score on EPDS. Post traumatic stress (on Davidson Trauma scale). History of trauma.
Notes	

Risk of bias

Bias	**Authors' judgement**	**Support for judgement**

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Zlotnick 2011 (*Continued*)

Random sequence generation (selection bias)	Low risk	Computer-generated schedule.
Allocation concealment (selection bias)	Low risk	Allocations concealed in consecutively numbered, sealed envelopes; allocation was by an investigator blind to baseline assessments.
Blinding (performance bias and detection bias) All outcomes	High risk	Blinding women, staff providing care and staff delivering the intervention to this type of intervention is not feasible. It is not clear how outcome data were collected.
Incomplete outcome data (attrition bias) All outcomes	Unclear risk	54 women were randomised and there were outcome data for 46 (85%). Information on women lost to follow-up was not provided. It was stated that analyses were by intention-to-treat.
Selective reporting (reporting bias)	Unclear risk	Assessment from published study reports.
Other bias	Low risk	No other bias is apparent.

DV: domestic violence
EPDS: Edinburgh Postnatal Depression Scale
IPV: intimate partner violence
RCT: randomised controlled trial

Characteristics of excluded studies *[ordered by study ID]*

Study	Reason for exclusion
Armstrong 1999	The population in this study were not pregnant women.This was a trial examining an intervention in the postnatal period. There was no intervention in pregnancy and women were not recruited until after the birth. The intervention was delivered by child health nurses who offered weekly visits to support mothers and enhance parenting confidence with advice on child development and behaviour and facilitate access to other services. Individually tailored to family circumstances
Bair-Merritt 2010	Women recruited to this study were not pregnant. The trial examined early childhood home visits to improve family functioning and reduce child maltreatment. Para-professionals gave advice on child development and parenting and offered support. The first visit was scheduled within a week of the birth and visits were thereafter scheduled weekly at first then tapering off for up to 3 years (mean of 13 visits in first year)

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(*Continued*)

Blackmore 2006	This study examined whether an antenatal health assessment form identified women with risk factors for postnatal depression (including DV). There was no intervention to prevent or reduce IPV
Bullock 2009	This study had an DV component but the objective did not meet this review criteria as it was looking into the effect of intervention on smoking cessation and DV reported as a part of secondary outcome
Eddy 2008	Target populations were not pregnant women. They were facilitators and trainers from various countries (n = 21)
Janssen 2003	This is a descriptive population based study with important findings on the effect of DV on pregnancy outcome. However, it is not a RCT
Janssen 2011	This was a proposal for a study reported in a trial registration; the planned trial did not take place
Kataoka 2010	This study did not examine interventions to prevent IPV rather it compared 2 different methods of increasing disclosure about DV
Koziol-McLain 2010	Women in this study were not pregnant. Participants included 399 English-speaking women aged 16 years and older who referred to emergency department,
Lipsky 2003	This cohort study investigates the effect of police report on pregnancy outcome of women suffering DV
Macy 2007	This was a longitudinal study with no randomisation and blinding
Marcenko 1994	Intervention involved consultation pertaining to DV prevention but none of the our priori outcome of interest was reported
McFarlane 1996	This study was not a RCT.
Miller 2011	The participants in this study were women attending family planning clinics; the women were not pregnant. The study was looking at reproductive coercion
Parker 1999	This was not a RCT.
Quinlivan 2003	Women were recruited to this study during pregnancy but were not randomised until after delivery and the intervention did not start until the postnatal period. The intervention was a postnatal home-visiting service by nurse-midwives aimed at "reducing adverse neonatal outcomes and in improving knowledge about contraception, vaccination schedules, and breastfeeding in teenage mothers younger than age 18 years" Structured home visits at 1, and 2 weeks and 1, 2, 4 and 6 months after the birth. Each visit lasted 1-4 hours. Content of visits included infant feeding advice and support, information on vaccinations and facilitate attendance for vaccinations, discussion of mood disorders and information on parenting. Follow-up of any issues (which could include violence) raised at 2 months postpartum, discussion of issues re drugs and alcohol and advice on contraception
Taft 2009a	The women recruited to this study were not all pregnant. The sample included women who were pregnant, who had a child under 5 or who otherwise were at high risk of IPV Women in the intervention arm received up to 12 months support from trained and supported non-professional mentor mothers. Women in the intervention arm received up to 12 months support from a trained

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(*Continued*)

	mentor. (No separate breakdown for pregnant women experiencing DV)

DV: domestic violence
IPV: intimate partner violence
RCT: randomised controlled trial

Characteristics of studies awaiting assessment *[ordered by study ID]*

Loree 2008

Methods	RCT.
Participants	Participants were adolescent couples; women were pregnant at recruitment
Interventions	A counselling intervention was compared with routine care.
Outcomes	Outcomes were episodes of violence within couples. It was not clear whether women or their partners were the victims of the violence; authors report that much of the violence was reciprocal
Notes	We have contacted the authors to see if we can obtain data for women only. paulf@ewm.edu (author contacted 20th June 2012)

RCT: randomised controlled trial

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DATA AND ANALYSES

Comparison 1. Any intervention to prevent violence (all interventions) versus standard care

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Partner abuse: episodes during pregnancy	1	220	Risk Ratio (M-H, Fixed, 95% CI)	0.5 [0.25, 1.02]
2 Partner abuse: episodes during the first 3 months postpartum	1	271	Risk Ratio (M-H, Fixed, 95% CI)	0.60 [0.35, 1.04]
3 Partner abuse: abuse score in the first 3 months postpartum	1	46	Mean Difference (IV, Fixed, 95% CI)	4.20 [-10.74, 19.14]
4 Partner abuse: abuse score in first 3 months postpartum (Conflict Tactics Scale)	1		Mean Difference (IV, Fixed, 95% CI)	Subtotals only
4.1 Psychological abuse	1	106	Mean Difference (IV, Fixed, 95% CI)	-0.81 [-1.45, -0.17]
4.2 Minor physical violence score	1	106	Mean Difference (IV, Fixed, 95% CI)	-0.46 [-0.82, -0.10]
4.3 Severe physical violence score	1	106	Mean Difference (IV, Fixed, 95% CI)	0.08 [-0.28, 0.44]
4.4 Sexual abuse score	1	106	Mean Difference (IV, Fixed, 95% CI)	-0.09 [-0.24, 0.06]
5 Partner abuse in the first 3 months postpartum (Current abuse score)	1	191	Mean Difference (IV, Fixed, 95% CI)	-0.12 [-0.31, 0.07]
6 Partner abuse: total episodes at final study assessment (pregnancy and up to 10 weeks postpartum)	1	306	Risk Ratio (M-H, Fixed, 95% CI)	0.62 [0.43, 0.88]
7 Partner violence at 7-8 months postpartum	1	181	Risk Ratio (M-H, Fixed, 95% CI)	0.53 [0.23, 1.21]
8 Women with depression (after the intervention) during pregnancy	1	46	Risk Ratio (M-H, Fixed, 95% CI)	0.42 [0.04, 4.31]
9 Mean Depression Episodes, Psychiatric Status Rating)	1	46	Mean Difference (IV, Fixed, 95% CI)	-0.46 [-0.97, 0.05]
10 Depression scores (after the intervention) during pregnancy	0	0	Mean Difference (IV, Fixed, 95% CI)	0.0 [0.0, 0.0]
11 Women with depression up to 3 months postpartum	2		Risk Ratio (M-H, Fixed, 95% CI)	Subtotals only
11.1 Empowerment training	1	106	Risk Ratio (M-H, Fixed, 95% CI)	0.39 [0.20, 0.75]
11.2 Psychological therapy intervention	1	46	Risk Ratio (M-H, Fixed, 95% CI)	1.40 [0.38, 5.18]
12 Depression scores up to 3 months postpartum	1	46	Mean Difference (IV, Fixed, 95% CI)	-1.88 [-5.24, 1.48]
13 Women with depression up to 1 year postpartum	1	182	Risk Ratio (M-H, Fixed, 95% CI)	0.85 [0.37, 1.98]
14 Depression scores up to 1 year postpartum	1	182	Mean Difference (IV, Fixed, 95% CI)	-0.65 [-2.41, 1.11]

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15 Apgar score less than 7 at 1 minute	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
16 Apgar score less than 7 at 5 minutes	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
17 Stillbirth	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
18 Neonatal death	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
19 Miscarriage (up to 20 weeks)	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
20 Maternal mortality	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
21 Antepartum haemorrhage	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
22 Placental abruption	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
23 Mean infant birthweight (g)	1	306	Mean Difference (IV, Fixed, 95% CI)	41.0 [-106.19, 188.19]
24 Number of low-birthweight (< 2500 g) babies	1	306	Risk Ratio (M-H, Fixed, 95% CI)	0.74 [0.41, 1.32]
25 Preterm labour (before 37 weeks' gestation)	1	306	Risk Ratio (M-H, Fixed, 95% CI)	0.69 [0.40, 1.20]
26 (Non-prespecified outcome) Mean gestational age at birth (weeks)	1	306	Mean Difference (IV, Fixed, 95% CI)	1.40 [0.33, 2.47]
27 (Non-prespecified outcome) Women adopting safety behaviours	1	204	Risk Ratio (M-H, Fixed, 95% CI)	2.6 [1.41, 4.79]
28 (Non-prespecified outcome) stress score in late pregnancy	1	191	Mean Difference (IV, Fixed, 95% CI)	-2.06 [-3.34, -0.78]
29 (Non-prespecified outcome) Did not discuss abuse with care providers	1	46	Risk Ratio (M-H, Fixed, 95% CI)	0.17 [0.04, 0.66]
30 (Non-prespecified outcome) Women reporting any domestic violence 2-4 years postpartum	1	735	Risk Ratio (M-H, Fixed, 95% CI)	0.81 [0.60, 1.08]

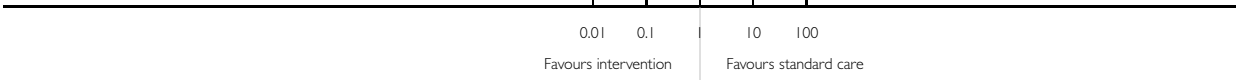
Analysis 1.1. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 1 Partner abuse: episodes during pregnancy.

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 1 Partner abuse: episodes during pregnancy

Study or subgroup	Intervention n/N	Standard care n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
Kiely 2010 (1)	10/110	20/110		100.0 %	0.50 [0.25, 1.02]
Total (95% CI)	**110**	**110**		**100.0 %**	**0.50 [0.25, 1.02]**
Total events: 10 (Intervention), 20 (Standard care)					
Heterogeneity: not applicable					
Test for overall effect: Z = 1.91 (P = 0.056)					
Test for subgroup differences: Not applicable					



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(1) Episodes since last follow-up at 34-38 weeks

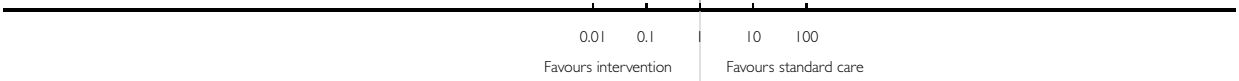
Analysis 1.2. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 2 Partner abuse: episodes during the first 3 months postpartum.

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 2 Partner abuse: episodes during the first 3 months postpartum

Study or subgroup	Intervention n/N	Standard care n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
Kiely 2010	17/134	29/137		100.0 %	0.60 [0.35, 1.04]
Total (95% CI)	**134**	**137**		**100.0 %**	**0.60 [0.35, 1.04]**
Total events: 17 (Intervention), 29 (Standard care)					
Heterogeneity: not applicable					
Test for overall effect: Z = 1.83 (P = 0.068)					
Test for subgroup differences: Not applicable					



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Analysis 1.3. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 3 Partner abuse: abuse score in the first 3 months postpartum.

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

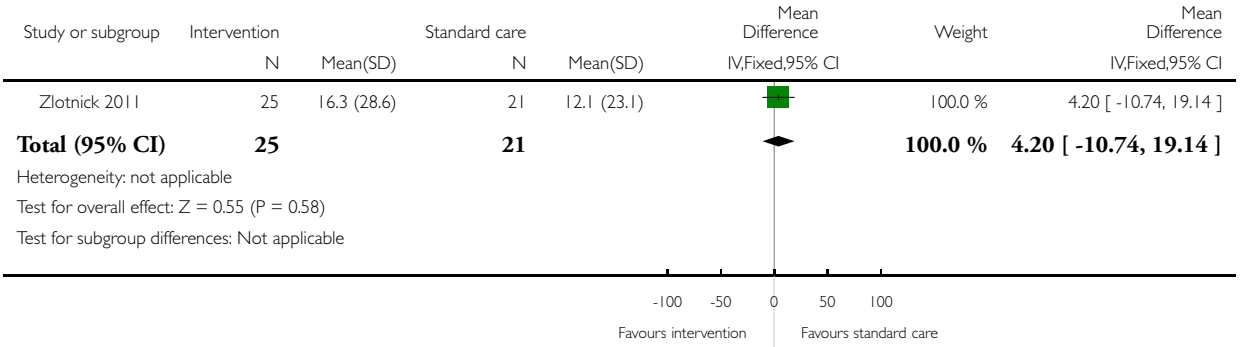
Outcome: 3 Partner abuse: abuse score in the first 3 months postpartum

Study or subgroup	Intervention N	Mean(SD)	Standard care N	Mean(SD)	Mean Difference IV,Fixed,95% CI	Weight	Mean Difference IV,Fixed,95% CI
Zlotnick 2011	25	16.3 (28.6)	21	12.1 (23.1)		100.0 %	4.20 [-10.74, 19.14]
Total (95% CI)	**25**		**21**			**100.0 %**	**4.20 [-10.74, 19.14]**

Heterogeneity: not applicable

Test for overall effect: Z = 0.55 (P = 0.58)

Test for subgroup differences: Not applicable



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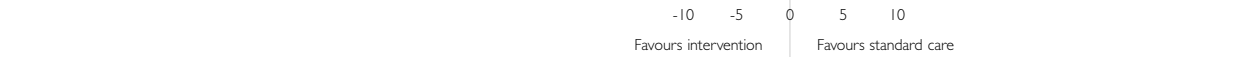
Analysis 1.4. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 4 Partner abuse: abuse score in first 3 months postpartum (Conflict Tactics Scale).

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 4 Partner abuse: abuse score in first 3 months postpartum (Conflict Tactics Scale)

Study or subgroup	Intervention N	Mean(SD)	Standard care N	Mean(SD)	Mean Difference IV,Fixed,95% CI	Weight	Mean Difference IV,Fixed,95% CI
1 Psychological abuse							
Tiwari 2005	51	0.79 (1)	55	1.6 (2.2)		100.0 %	-0.81 [-1.45, -0.17]
Subtotal (95% CI)	**51**		**55**			**100.0 %**	**-0.81 [-1.45, -0.17]**
Heterogeneity: not applicable							
Test for overall effect: Z = 2.47 (P = 0.014)							
2 Minor physical violence score							
Tiwari 2005	51	0.05 (0.4)	55	0.51 (1.3)		100.0 %	-0.46 [-0.82, -0.10]
Subtotal (95% CI)	**51**		**55**			**100.0 %**	**-0.46 [-0.82, -0.10]**
Heterogeneity: not applicable							
Test for overall effect: Z = 2.50 (P = 0.012)							
3 Severe physical violence score							
Tiwari 2005	51	0.25 (1.2)	55	0.17 (0.54)		100.0 %	0.08 [-0.28, 0.44]
Subtotal (95% CI)	**51**		**55**			**100.0 %**	**0.08 [-0.28, 0.44]**
Heterogeneity: not applicable							
Test for overall effect: Z = 0.44 (P = 0.66)							
4 Sexual abuse score							
Tiwari 2005	51	0.03 (0.11)	55	0.12 (0.55)		100.0 %	-0.09 [-0.24, 0.06]
Subtotal (95% CI)	**51**		**55**			**100.0 %**	**-0.09 [-0.24, 0.06]**
Heterogeneity: not applicable							
Test for overall effect: Z = 1.19 (P = 0.23)							
Test for subgroup differences: Chi^2 = 9.07, df = 3 (P = 0.03), I^2 =67%							



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Analysis 1.5. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 5 Partner abuse in the first 3 months postpartum (Current abuse score).

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 5 Partner abuse in the first 3 months postpartum (Current abuse score)

Study or subgroup	Intervention N	Mean(SD)	Standard care N	Mean(SD)	Mean Difference IV,Fixed,95% CI	Weight	Mean Difference IV,Fixed,95% CI
Curry 2006	99	1.18 (0.52)	92	1.3 (0.76)		100.0 %	-0.12 [-0.31, 0.07]
Total (95% CI)	**99**		**92**			**100.0 %**	**-0.12 [-0.31, 0.07]**

Heterogeneity: not applicable

Test for overall effect: Z = 1.26 (P = 0.21)

Test for subgroup differences: Not applicable

Analysis 1.6. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 6 Partner abuse: total episodes at final study assessment (pregnancy and up to 10 weeks postpartum).

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 6 Partner abuse: total episodes at final study assessment (pregnancy and up to 10 weeks postpartum)

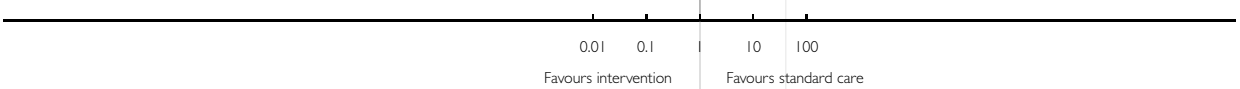
Study or subgroup	Intervention n/N	Standard care n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
Kiely 2010 (1)	35/150	59/156		100.0 %	0.62 [0.43, 0.88]
Total (95% CI)	**150**	**156**		**100.0 %**	**0.62 [0.43, 0.88]**

Total events: 35 (Intervention), 59 (Standard care)

Heterogeneity: not applicable

Test for overall effect: Z = 2.68 (P = 0.0073)

Test for subgroup differences: Not applicable



(1) Up to 8-10 weeks postpartum

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Analysis 1.7. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 7 Partner violence at 7-8 months postpartum.

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 7 Partner violence at 7-8 months postpartum

Study or subgroup	Intervention n/N	Standard care n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
Nagle 2002	10/118	10/63		100.0 %	0.53 [0.23, 1.21]
Total (95% CI)	**118**	**63**		**100.0 %**	**0.53 [0.23, 1.21]**

Total events: 10 (Intervention), 10 (Standard care)
Heterogeneity: not applicable
Test for overall effect: Z = 1.50 (P = 0.13)
Test for subgroup differences: Not applicable

Analysis 1.8. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 8 Women with depression (after the intervention) during pregnancy.

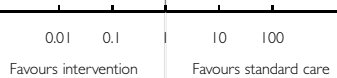
Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 8 Women with depression (after the intervention) during pregnancy

Study or subgroup	Intervention n/N	Standard care n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
Zlotnick 2011 (1)	1/25	2/21		100.0 %	0.42 [0.04, 4.31]
Total (95% CI)	**25**	**21**		**100.0 %**	**0.42 [0.04, 4.31]**

Total events: 1 (Intervention), 2 (Standard care)
Heterogeneity: not applicable
Test for overall effect: Z = 0.73 (P = 0.47)
Test for subgroup differences: Not applicable



(1) Major depressive episode

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Analysis 1.9. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 9 Mean Depression Episodes, Psychiatric Status Rating).

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 9 Mean Depression Episodes, Psychiatric Status Rating)

Study or subgroup	Intervention N	Mean(SD)	Standard care N	Mean(SD)	Mean Difference IV,Fixed,95% CI	Weight	Mean Difference IV,Fixed,95% CI
Zlotnick 2011	25	1.65 (0.81)	21	2.11 (0.94)		100.0 %	-0.46 [-0.97, 0.05]
Total (95% CI)	**25**		**21**			**100.0 %**	**-0.46 [-0.97, 0.05]**

Heterogeneity: not applicable

Test for overall effect: Z = 1.76 (P = 0.078)

Test for subgroup differences: Not applicable

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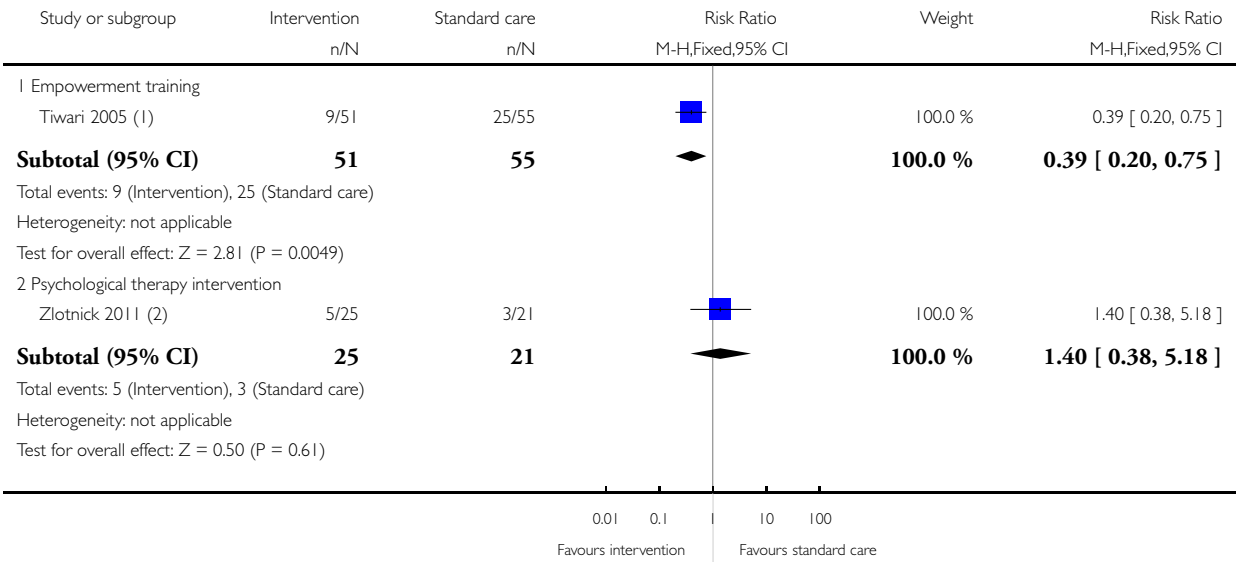
Analysis 1.11. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 11 Women with depression up to 3 months postpartum.

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 11 Women with depression up to 3 months postpartum



(1) EPDS score 10 or more

(2) Major depressive episode

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Analysis 1.12. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 12 Depression scores up to 3 months postpartum.

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 12 Depression scores up to 3 months postpartum

Study or subgroup	Intervention N	Mean(SD)	Standard care N	Mean(SD)	Mean Difference IV,Fixed,95% CI	Weight	Mean Difference IV,Fixed,95% CI
Zlotnick 2011 (1)	25	6.12 (5.86)	21	8 (5.74)		100.0 %	-1.88 [-5.24, 1.48]
Total (95% CI)	**25**		**21**			**100.0 %**	**-1.88 [-5.24, 1.48]**

Heterogeneity: not applicable

Test for overall effect: Z = 1.10 (P = 0.27)

Test for subgroup differences: Not applicable

(1) EPDS scores

Analysis 1.13. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 13 Women with depression up to 1 year postpartum.

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 13 Women with depression up to 1 year postpartum

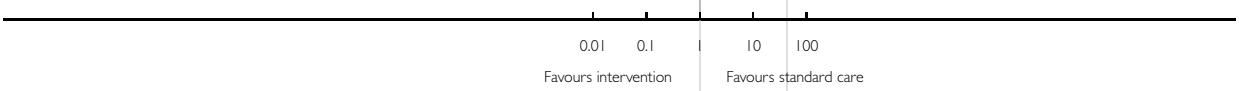
Study or subgroup	Intervention n/N	Standard care n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
Nagle 2002 (1)	12/116	8/66		100.0 %	0.85 [0.37, 1.98]
Total (95% CI)	**116**	**66**		**100.0 %**	**0.85 [0.37, 1.98]**

Total events: 12 (Intervention), 8 (Standard care)

Heterogeneity: not applicable

Test for overall effect: Z = 0.37 (P = 0.71)

Test for subgroup differences: Not applicable



(1) Score >16 on Beck depression scale at 7-8 months postpartum

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Analysis 1.14. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 14 Depression scores up to 1 year postpartum.

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 14 Depression scores up to 1 year postpartum

Study or subgroup	Intervention N	Mean(SD)	Standard care N	Mean(SD)	Mean Difference IV,Fixed,95% CI	Weight	Mean Difference IV,Fixed,95% CI
Nagle 2002 (1)	116	5.12 (5.11)	66	5.77 (6.21)		100.0 %	-0.65 [-2.41, 1.11]
Total (95% CI)	**116**		**66**			**100.0 %**	**-0.65 [-2.41, 1.11]**

Heterogeneity: not applicable

Test for overall effect: Z = 0.72 (P = 0.47)

Test for subgroup differences: Not applicable

(1) Mean score on Beck depression scale at 7-8 months postpartum

Analysis 1.23. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 23 Mean infant birthweight (g).

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 23 Mean infant birthweight (g)

Study or subgroup	Intervention N	Mean(SD)	Standard care N	Mean(SD)	Mean Difference IV,Fixed,95% CI	Weight	Mean Difference IV,Fixed,95% CI
Kiely 2010	150	3139 (593)	156	3098 (717)		100.0 %	41.00 [-106.19, 188.19]
Total (95% CI)	**150**		**156**			**100.0 %**	**41.00 [-106.19, 188.19]**

Heterogeneity: not applicable

Test for overall effect: Z = 0.55 (P = 0.59)

Test for subgroup differences: Not applicable

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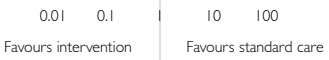
Analysis 1.24. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 24 Number of low-birthweight (< 2500 g) babies.

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 24 Number of low-birthweight (< 2500 g) babies

Study or subgroup	Intervention n/N	Standard care n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
Kiely 2010	17/150	24/156		100.0 %	0.74 [0.41, 1.32]
Total (95% CI)	**150**	**156**		**100.0 %**	**0.74 [0.41, 1.32]**
Total events: 17 (Intervention), 24 (Standard care)					
Heterogeneity: not applicable					
Test for overall effect: Z = 1.03 (P = 0.30)					
Test for subgroup differences: Not applicable					



Analysis 1.25. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 25 Preterm labour (before 37 weeks' gestation).

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 25 Preterm labour (before 37 weeks' gestation)

Study or subgroup	Intervention n/N	Standard care n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
Kiely 2010	18/150	27/156		100.0 %	0.69 [0.40, 1.20]
Total (95% CI)	**150**	**156**		**100.0 %**	**0.69 [0.40, 1.20]**
Total events: 18 (Intervention), 27 (Standard care)					
Heterogeneity: not applicable					
Test for overall effect: Z = 1.30 (P = 0.19)					
Test for subgroup differences: Not applicable					

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Analysis 1.26. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 26 (Non-prespecified outcome) Mean gestational age at birth (weeks).

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 26 (Non-prespecified outcome) Mean gestational age at birth (weeks)

Study or subgroup	Intervention N	Mean(SD)	Standard care N	Mean(SD)	Mean Difference IV,Fixed,95% CI	Weight	Mean Difference IV,Fixed,95% CI
Kiely 2010	150	38.3 (3.3)	156	36.9 (5.9)		100.0 %	1.40 [0.33, 2.47]
Total (95% CI)	**150**		**156**			**100.0 %**	**1.40 [0.33, 2.47]**

Heterogeneity: not applicable

Test for overall effect: Z = 2.57 (P = 0.010)

Test for subgroup differences: Not applicable

Analysis 1.27. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 27 (Non-prespecified outcome) Women adopting safety behaviours.

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 27 (Non-prespecified outcome) Women adopting safety behaviours

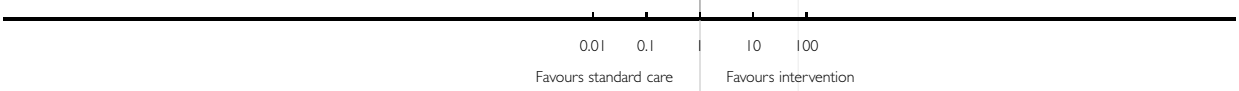
Study or subgroup	Intervention n/N	Standard care n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
Cripe 2010	30/100	12/104		100.0 %	2.60 [1.41, 4.79]
Total (95% CI)	**100**	**104**		**100.0 %**	**2.60 [1.41, 4.79]**

Total events: 30 (Intervention), 12 (Standard care)

Heterogeneity: not applicable

Test for overall effect: Z = 3.07 (P = 0.0022)

Test for subgroup differences: Not applicable



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Analysis 1.28. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 28 (Non-prespecified outcome) stress score in late pregnancy.

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 28 (Non-prespecified outcome) stress score in late pregnancy

Study or subgroup	Intervention N	Mean(SD)	Standard care N	Mean(SD)	Mean Difference IV,Fixed,95% CI	Weight	Mean Difference IV,Fixed,95% CI
Curry 2006	99	19.67 (4.13)	92	21.73 (4.81)		100.0 %	-2.06 [-3.34, -0.78]
Total (95% CI)	**99**		**92**			**100.0 %**	**-2.06 [-3.34, -0.78]**

Heterogeneity: not applicable

Test for overall effect: Z = 3.16 (P = 0.0016)

Test for subgroup differences: Not applicable

Analysis 1.29. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 29 (Non-prespecified outcome) Did not discuss abuse with care providers.

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 29 (Non-prespecified outcome) Did not discuss abuse with care providers

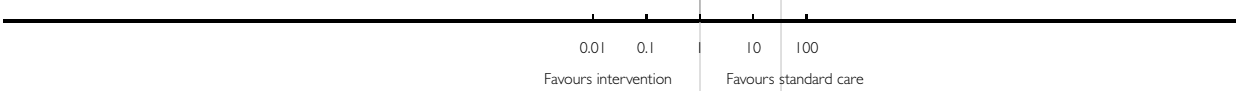
Study or subgroup	Intervention n/N	Standard care n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
Calderon 2008	2/22	13/24		100.0 %	0.17 [0.04, 0.66]
Total (95% CI)	**22**	**24**		**100.0 %**	**0.17 [0.04, 0.66]**

Total events: 2 (Intervention), 13 (Standard care)

Heterogeneity: not applicable

Test for overall effect: Z = 2.55 (P = 0.011)

Test for subgroup differences: Not applicable



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Analysis 1.30. Comparison 1 Any intervention to prevent violence (all interventions) versus standard care, Outcome 30 (Non-prespecified outcome) Women reporting any domestic violence 2-4 years postpartum.

Review: Interventions for preventing or reducing domestic violence against pregnant women

Comparison: 1 Any intervention to prevent violence (all interventions) versus standard care

Outcome: 30 (Non-prespecified outcome) Women reporting any domestic violence 2-4 years postpartum

Study or subgroup	Intervention n/N	Standard care n/N	Risk Ratio M-H,Fixed,95% CI	Weight	Risk Ratio M-H,Fixed,95% CI
Olds 2004	91/480	60/255		100.0 %	0.81 [0.60, 1.08]
Total (95% CI)	**480**	**255**		**100.0 %**	**0.81 [0.60, 1.08]**
Total events: 91 (Intervention), 60 (Standard care)					
Heterogeneity: not applicable					
Test for overall effect: Z = 1.47 (P = 0.14)					
Test for subgroup differences: Not applicable					

CONTRIBUTIONS OF AUTHORS

Shayesteh Jahanfar wrote the protocol, conducted an additional search, extracted the data, assessed risk of bias, analysed data, wrote the discussion, abstract and edited the result section.

Patricia Janssen commented on the protocol and edited the review.

Louise Howard commented on the protocol, extracted data, assessed risk of bias and read and edited the review.

Therese Dowswell was involved in data extraction, assessing risk of bias, data analysis and drafting text of the review.

DECLARATIONS OF INTEREST

Louise Howard is funded by a National Institute for Health Research Programme Grant for Applied Research on Improving the Healthcare Response to Domestic Violence and is a member of the WHO Guideline Development Group and the NICE/SCIE Guideline Development Group developing guidelines on the preventing and reducing domestic violence.

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