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Postpartum health professional contact for improving maternal and infant health outcomes for healthy women and their infants (Protocol)

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Postpartum health professional contact for improving maternal and infant health outcomes for healthy women and their infants.
Cochrane Database of Systematic Reviews 2013, Issue 12. Art. No.: CD010855.
DOI: 10.1002/14651858.CD010855.

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[Intervention Protocol]

Postpartum health professional contact for improving maternal and infant health outcomes for healthy women and their infants

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Editorial group: Cochrane Pregnancy and Childbirth Group.
Publication status and date: New, published in Issue 12, 2013.

Citation: Brodribb WE, Zakarija-Grkovic I, Hawley G, Mitchell B, Mathews A. Postpartum health professional contact for improving maternal and infant health outcomes for healthy women and their infants. *Cochrane Database of Systematic Reviews* 2013, Issue 12. Art. No.: CD010855. DOI: 10.1002/14651858.CD010855.

ABSTRACT

This is the protocol for a review and there is no abstract. The objectives are as follows:

To assess the effect of health professional contact (e.g. home visits, telehealth contact (other than by telephone), or visits to clinics) with postpartum women, not enrolled in specialised programs, within the first four weeks following hospital discharge on maternal and infant health outcomes.

BACKGROUND

Description of the condition

The postpartum period can be a special, though often challenging, time for a mother and her new family as significant physical, psychological and social changes occur (Shaw 2006). Health professional contact in the first month following birth may contribute to a smoother transition and help prevent and manage infant and maternal complications.

While serious postpartum medical problems such as haemorrhage, thromboembolic disease, infection and eclampsia are well described, there are many medical and behavioural issues during the postpartum period that are under recognised, under reported (Schmied 2009) and hence, inappropriately managed (Schmied 2009; Tunçalp 2012).

Postpartum morbidities occur commonly throughout the world (Cheng 2008). For mothers these include tiredness (31% to 59%) (Brown 1998; Glazener 1995; Lagro 2003; Miller 2011; Saurel-Cubizolles 2000; Schytt 2005; Woolhouse 2012), backache (24% to 55%) (Brown 1998; Glazener 1995; Lagro 2003; Miller 2011; Saurel-Cubizolles 2000; Woolhouse 2012), depression (19% to 34%) (Brown 1998; Glazener 1995; Miller 2011; Saurel-Cubizolles 2000), headaches (18% to 47%) (Glazener 1995; Lagro

2003; Saurel-Cubizolles 2000; Schytt 2005; Woolhouse 2012), perineal pain (10.7% to 34.7%) (Brown 1998; Miller 2011; Saurel-Cubizolles 2000; Schytt 2005), urinary incontinence (18% to 30%) (Brown 1998; Glazener 1995; Miller 2011; Thompson 2002; Woolhouse 2012), bowel problems (19% to 45%) (Brown 1998; Thompson 2002; Miller 2011), faecal incontinence (4.5% to 8%) (Brown 2000; Woolhouse 2012) and constipation (10% to 27%) (Glazener 1995; Lagro 2003; Saurel-Cubizolles 2000; Schytt 2005; Woolhouse 2012). Problems such as postpartum anxiety, prolonged bleeding and urinary tract infections are also reported (Keppler 1995; Marchant 2002; Miller 2011).

Ongoing postpartum depression is associated with poorer maternal physical health (Brown 2000). Compromised maternal physical health is associated with a reduction in the mother's capacity to work, look after children or undertake household tasks (Webb 2008). Poorer physical and mental health is also associated with increased infant crying and sleep problems (Bayer 2007), which ultimately has a negative impact on the health, development and well-being of children when aged three (Kahn 2002).

Breastfeeding issues including breast engorgement, sore nipples and mastitis are common, especially in the first few weeks following birth (Hauck 2011). Women are more likely to not breastfeed or stop breastfeeding early if they have ongoing physical and mental health issues (Amir 2010; Dennis 2009; Forster 2006). Similarly, women are more likely to wean if their infant is unsettled, they think they do not have enough milk, they have painful nipples or breast problems (Hauck 2011). Limited or no breastfeeding increases infant morbidity and mortality in the short and long term and increases maternal risk for breast and ovarian cancer, Type-2 diabetes and cardiovascular disease (Horta 2007; Ip 2007; Stuebe 2010).

In terms of infant morbidity, the most common problems following hospital discharge include jaundice (32.6%), feeding difficulty (16.1%), weight loss (13.9%) and nappy rash (10%) (Bennett 1998). In addition, infant crying (Hiscock 2006) and sleep disturbances are common causes of concern for parents (Bayer 2007).

These medium- and long-term consequences of inadequate management of physical and mental health issues in the immediate postpartum period are often not recognised, leading to suboptimal health of the mother/infant dyad and additional expenditure for overburdened health systems (Bartick 2010; Renfrew 2012a).

Postpartum care in the community is becoming increasingly important as post-birth hospital stays have reduced substantially over the past 20 years (Cuncarr 2011; Dana 2003; Goulet 2007; Lancaster 1994; Li 2012) due to fiscal constraints (Dana 2003; Gagnon 2002) and the reduction in the number of postpartum beds within hospitals (McLachlan 2009). Postpartum follow-up provides a suitable opportunity to identify and manage these maternal and infant health issues and provide information so that mothers are better prepared for potential problems that may be encountered after childbirth (Schytt 2005). The Cochrane Systematic Review by Brown et. al. (Brown 2009) found no evidence that early hospital discharge had a detrimental effect on maternal and infant health or breastfeeding rates. However, all studies included in the review provided post-discharge nursing or midwifery support. Nevertheless, it appears that different models of post-discharge care result in differing hospital readmission rates (Goulet 2007), maternal satisfaction (Madden 2004) and changes in the use of primary care services (Mandl 2000). Overall, appropriate care in the weeks following childbirth has the potential to contribute to the health and well-being of the new mother and her family.

Description of the intervention

The main goals of postpartum care in the community are to: provide a safety net to identify important postpartum conditions (e.g. jaundice, puerperal infection, depression); to uncover and manage other physical and/or mental health problems of the mother and/or infant; to build maternal confidence in parenting skills and to support breastfeeding, thereby increase family well-being and satisfaction (Wiegers 2006).

Community postpartum interventions aimed to improve maternal and infant outcomes such as breastfeeding rates, maternal morbidities including postpartum depression and infant morbidity, include telephone or other telehealth contact, home visiting by a nurse or midwife, a visit by the mother to a community or hospital-based clinic, or a combination of these.

At present there is great variation between existing models of postpartum care in different countries and within the same country. In the United States of America, the American Academy of Pediatrics recommends a visit to a paediatrician within 72 hours of hospital discharge if discharged within 48 hours of birth (AAP 2010), but there is limited home visiting (Mandl 2000). In other countries, home visiting is more common. For example, the NICE (National Institute for Health and Clinical Excellence) guidelines, designed for use by those who work in the National Health Service (NHS) in England and Wales, propose a care pathway to optimise maternal and infant health and infant feeding (Demott 2006). They suggest information and care be provided within 24 hours of birth, between days two and seven and between weeks two and eight. Midwifery care is provided up to 28 days post-delivery, followed by health visitor care (Bull 2004). In the Netherlands, women receive up to five or six home visits within the first 10-12 days following early discharge or a home birth (Wiegers 2006) and in Denmark, most women are also offered a home visit within the first 10-14 days (Kronborg 2012). In contrast, in Switzerland postpartum care in the mother's home is provided by self-employed midwives who visit up to 50% of postpartum women (Kurth 2010). Within Australia, there are no consistent recommendations between States, with the provision of almost universal contact by home visiting midwives and then child health nurses occurring in some areas (Biro 2012; Victorian Department of Health 2012), while in others less than 50% of women receive a home visit, some receive

a telephone call only and others have no contact with a health professional at all during the first 10 days postdischarge (Miller 2011). There are also programs that target specific populations (Brodribb 2012; Kemp 2010) without an organised, overarching system or recommendation.

As yet, it is not clear whether health professional contact in the early postpartum period is beneficial, and if it is, what form this contact should take. It may be, that for some women, health professional contact has a detrimental effect due to incorrect information or advice being given or a reduction in the mother's self-efficacy for breastfeeding and other parenting skills. While it would be impossible to compare the effects of 'usual care' across jurisdictions, it is possible to assess the impact of health professional contact interventions in addition to 'usual care'. Although it is recognised that in many places a routine visit is usually scheduled at the end of the postpartum period (i.e. around six weeks), this review is concerned with the impact of earlier contact (e.g. up to and including four weeks).

How the intervention might work

Ideally, timely and appropriate postpartum care should increase breastfeeding continuation rates, identify maternal depression, improve maternal satisfaction with care and confidence in parenting, and decrease utilisation of health services such as general practice, obstetric or paediatric consultations, emergency department visits and readmissions to hospital. Early postpartum health professional contact, including appropriate discussion, may increase a mother's awareness of what is 'normal' and what is not, encourage earlier reporting of maternal and infant problems and facilitate adequate management and treatment (Schytt 2005).

A Cochrane review by Renfrew 2012 found that breastfeeding support interventions had a positive effect on breastfeeding continuation and exclusive breastfeeding. Subgroup analysis found lay support appeared more beneficial than professional support, that postpartum interventions had similar outcomes to interventions that included both an antenatal and postpartum component and that face-to-face interventions were more effective than telephone or mixed interventions. Another recent Cochrane review on the schedules for home visits in the early postpartum period (Yonemoto 2013) found inconsistent results on their effect on maternal and neonatal mortality/morbidity, maternal satisfaction and neonatal immunisation. A recent Cochrane review (Lavender 2013) assessed telephone contact in the antenatal and/or postpartum period and found that there was not enough evidence to support changes in care, although there appeared to be a benefit for some outcomes.

One English study found that an intervention with extended midwifery contact (to three months) improved mothers' mental health status, but not their physical health, compared with usual care by general practitioners (MacArthur 2002).There is also evidence that screening by health professionals at well-child visits increases detection of maternal depression (Sheeder 2009). Treating postpartum depression is also likely to improve other facets such as sleep quality and child development (Dorheim 2009). Additonal visits to medical practitioners have also been assessed. In one study there was no improvement in maternal and child health or breastfeeding rates (Gunn 1998), while in another there was improvement in breastfeeding rates, at least in the short term (Labarere 2005).

Dana and Wambach (Dana 2003) found that women had high satisfaction levels with nurse home visits after early postpartum discharge. The significant factors contributing to this were friendliness and concern, technical skill, infant care teaching and addressing individual needs (Dana 2003). A Western Australian study reported that mothers were particularly happy with practical advice, assistance with baby care and immediate physical recovery that were provided via midwifery care at home (Fenwick 2010). In addition to contributing to maternal satisfaction, quality postpartum care may also improve maternal confidence. In a qualitative study by Forster 2008, women reported feeling more confident in caring for their new infant when health professionals were physically available (both in hospital and at home) to answer concerns.

Why it is important to do this review

Postpartum maternal and infant health issues are common and are a major cause of concern for many new families. Yet there is little consistency in the type, frequency, timing, location and availability of health professional contact women receive in the postpartum period both within and between countries (Schmied 2010; Wiegers 2006). In addition, there are differences in duration and content of the contact and qualifications of the health professional provider (Kemp 2010; Wiegers 2006). Evidence to indicate that one regimen is more effective than others in supporting families and preventing maternal/infant morbidity in the postnatal period is lacking (Bull 2004). However, in many areas governments and health services are spending increasing amounts of money to ensure health professional contact to postpartum women, regardless of need or length of hospital stay. In some circumstances the decision to provide a service is based on political will and health service logistics rather than on maternal and infant need or evidence of improved outcomes. Comparing different interventions for community postpartum care, will provide an evidence-based approach to the most efficacious use of resources for all mothers. This information will be particularly useful for policy makers deciding how healthcare dollars should be spent (Bull 2004; Cooke 1999). In order to avoid overlapping with the recent Lavender 2013 review, our review will not include telephone contact but will look broadly at all other forms of health professional postpartum support and their effect on a wide range of outcomes.

OBJECTIVES

To assess the effect of health professional contact (e.g. home visits, telehealth contact (other than by telephone), or visits to clinics) with postpartum women, not enrolled in specialised programs, within the first four weeks following hospital discharge on maternal and infant health outcomes.

METHODS

Criteria for considering studies for this review

Types of studies

All published or unpublished controlled clinical trials, cluster-randomised and randomised controlled trials in full text that compare different types of health professional contact in the first four weeks post hospital discharge with usual care or one form of contact with another will be included. If an abstract is found that fulfils the selection criteria all efforts will be made to contact the author to obtain sufficient information for inclusion in the review.

Types of participants

Participants will be healthy mothers of full term healthy infants (37 to 42 weeks' gestation), receiving care from a fully qualified health professional within the first four weeks following hospital discharge after a vaginal or caesarean section birth. Studies targeting vulnerable populations (e.g. low-income families, indigenous women, teenage mothers, women at risk of domestic violence) or women or infants who have special needs (e.g. those with substance dependence, significant medical problems, low birthweight or premature infants) will be excluded.

Types of interventions

Interventions will include individual one-on-one contact by:

1. home-visits;
2. telehealth (e.g. email, Skype but excluding telephone or SMS contact);
3. visits to a clinic (e.g. general, obstetric or paediatric practice, hospital or maternal/child health clinic);

that have been proactively organised by the health service rather than self-initiated by the mother and completed within four weeks of birth. A combination of the listed interventions may occur. Interventions will be compared with usual care. Contact to provide a metabolic screen for the infant, but no other care, will also be excluded. Interventions including antenatal or hospital components will be excluded unless the postpartum care segment is able to be analysed separately. We will not include 'telephone contact' in order to avoid overlapping with the Lavender 2013 review on this topic.

Only interventions delivered by a fully qualified health professional who provides maternal and/or infant care will be considered. The following health professionals may be utilised:

1. nurse;
2. midwife;
3. doctor;
4. lactation consultant.

Studies evaluating lay or peer support and lay healthcare assistants will be excluded.

Types of outcome measures

Primary outcomes

Outcomes measured within six months following the birth

1. Stopping breastfeeding:
   i) by four to six weeks;
   ii) by six months.
2. Stopping exclusive breastfeeding:
   i) by four to six weeks;
   ii) by six months.
3. Maternal and infant mortality.
4. Maternal depression measured objectively with the Edinburgh Postnatal Depression Scale or other validated tool.
5. Maternal and infant health service utilisation - regardless of the presenting complaint:
   i) presentation to primary care practitioner;
   ii) presentation to an emergency department;
   iii) hospital readmission.

Secondary outcomes

1. Maternal satisfaction with care measured using a validated tool defined by the study authors.
2. Maternal confidence with parenting measured using a validated tool such as the Breastfeeding Self-efficacy Scale or as defined by the study authors.

Search methods for identification of studies

Electronic searches

We will contact the Trials Search Co-ordinator to search the Cochrane Pregnancy and Childbirth Group's Trials Register.

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.

In addition, we plan to search:

1. CINAHL (1982 to current) using search strategies given in Appendix 1.
2. WHO International Clinical Trials Registry Platform (ICTRP) for planned, ongoing or unpublished trials. The search terms we plan to use are given in Appendix 2.

Searching other resources

1. References from published studies. We will search the reference lists of relevant trials and reviews identified
2. Unpublished literature. If necessary, we will contact the authors for more details about published or ongoing trials.

We will not apply any language restrictions.

Data collection and analysis

Selection of studies

Two review authors will independently assess for inclusion all the potential studies we identify as a result of the search strategy. We will resolve any disagreement through discussion or, if required, we will consult a third person.

Data extraction and management

We will design a form to extract data. For eligible studies, two review authors will extract the data using the agreed form. We will resolve discrepancies through discussion or, if required, we will consult a third person. We will enter data into Review Manager software (RevMan 2012) and check for accuracy.

When information regarding any of the above is unclear, we will attempt to contact authors of the original reports to provide further details.

Assessment of risk of bias in included studies

Two review authors will independently assess risk of bias for each study using the criteria outlined in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011). We will resolve any disagreement by discussion or by involving a third assessor.

(1) Random sequence generation (checking for possible selection bias)

We will describe 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 will assess 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);
- unclear risk of bias.

(2) Allocation concealment (checking for possible selection bias)

We will describe for each included study the method used to conceal allocation to interventions prior to assignment and will assess whether intervention allocation could have been foreseen in advance of, or during recruitment, or changed after assignment.

We will assess 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);
- unclear risk of bias.

(3.1) Blinding of participants and personnel (checking for possible performance bias)

We will describe for each included study the methods used, if any, to blind study participants and personnel from knowledge of which intervention a participant received. We will consider that studies are at low risk of bias if they were blinded, or if we judge that the lack of blinding would be unlikely to affect results. We will assess blinding separately for different outcomes or classes of outcomes.

We will assess the methods as:

- low, high or unclear risk of bias for participants;
- low, high or unclear risk of bias for personnel.

(3.2) Blinding of outcome assessment (checking for possible detection bias)

We will describe for each included study the methods used, if any, to blind outcome assessors from knowledge of which intervention

a participant received. We will assess blinding separately for different outcomes or classes of outcomes.
We will assess methods used to blind outcome assessment as:

- low, high or unclear risk of bias.

(4) Incomplete outcome data (checking for possible attrition bias due to the amount, nature and handling of incomplete outcome data)

We will describe for each included study, and for each outcome or class of outcomes, the completeness of data including attrition and exclusions from the analysis. We will state whether attrition and exclusions were reported and 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 is reported, or can be supplied by the trial authors, we will re-include missing data in the analyses which we undertake.
We will assess methods as:

- low risk of bias (e.g. no missing outcome data; missing outcome data balanced across groups);
- high risk of bias (e.g. numbers or reasons for missing data imbalanced across groups; 'as treated' analysis done with substantial departure of intervention received from that assigned at randomisation);
- unclear risk of bias.

Studies or some outcomes of studies will not be included if there is more than 25% of data missing for the whole study or for a particular outcome.

(5) Selective reporting (checking for reporting bias)

We will describe for each included study how we investigated the possibility of selective outcome reporting bias and what we found. We will assess 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 are reported incompletely and so cannot be used; study fails to include results of a key outcome that would have been expected to have been reported);
- unclear risk of bias.

(6) Other bias (checking for bias due to problems not covered by (1) to (5) above)

We will describe for each included study any important concerns we have about other possible sources of bias. Cluster-randomised trials will also be assessed for recruitment bias, any baseline imbalance between randomised groups and statistical methods used.
We will assess 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;
- unclear whether there is risk of other bias.

(7) Overall risk of bias

We will make explicit judgements about whether studies are at high risk of bias, according to the criteria given in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2011). With reference to (1) to (6) above, we will assess the likely magnitude and direction of the bias and whether we consider it is likely to impact on the findings. We will explore the impact of the level of bias through undertaking sensitivity analyses - *see* Sensitivity analysis.

Measures of treatment effect

Dichotomous data

For dichotomous data, we will present results as summary risk ratio with 95% confidence intervals.

Continuous data

For continuous data, we will use the mean difference if outcomes are measured in the same way between trials. We will use the standardised mean difference to combine trials that measure the same outcome, but use different methods.

Unit of analysis issues

Cluster-randomised trials

We will include cluster-randomised trials in the analyses along with individually-randomised trials. We will adjust their standard errors using the methods described in the *Cochrane Handbook* using an estimate of the intra cluster correlation co-efficient (ICC) derived 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 individually-randomised trials, we plan to synthesise the relevant information. 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.

Multi-group interventions

If studies include multiple intervention arms we will first consider combining groups to produce a single pairwise comparison. If this is not appropriate, we will include pair-wise comparisons separately with the common group divided approximately evenly among the comparisons.

Dealing with missing data

Studies and outcomes will not be included if they have more than 25% missing data or wrong allocation to control or intervention group.

For included studies, we will note levels of attrition. We will explore the impact of including studies with high levels of missing data in the overall assessment of treatment effect by using sensitivity analysis.

For all outcomes, we will carry out analyses, as far as possible, on an intention-to-treat basis, i.e. we will attempt to include all participants randomised to each group in the analyses, and all participants will be analysed 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 will be the number randomised minus any participants whose outcomes are known to be missing.

Assessment of heterogeneity

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

Assessment of reporting biases

If there are 10 or more studies in the meta-analysis, we will investigate reporting biases (such as publication bias) using funnel plots. We will assess funnel plot asymmetry visually. If asymmetry is suggested by a visual assessment, we will perform exploratory analyses to investigate it.

Data synthesis

We will carry out statistical analysis using the Review Manager software (RevMan 2012). We will use fixed-effect meta-analysis for combining data where it is reasonable to assume that studies are estimating the same underlying treatment effect: i.e. where trials are examining the same intervention, and the trials' populations and methods are judged sufficiently similar. If there is clinical heterogeneity sufficient to expect that the underlying treatment effects differ between trials, or if substantial statistical heterogeneity is detected, we will use random-effects meta-analysis to produce an overall summary, if an average treatment effect across trials is considered clinically meaningful. The random-effects summary will be treated as the average range of possible treatment effects and we will discuss the clinical implications of treatment effects differing between trials. If the average treatment effect is not clinically meaningful, we will not combine trials.

If we use random-effects analyses, the results will be presented as the average treatment effect with 95% confidence intervals, and the estimates of T^2 and I^2.

Subgroup analysis and investigation of heterogeneity

If we identify substantial heterogeneity, we will investigate it using subgroup analyses and sensitivity analyses. We will consider whether an overall summary is meaningful, and if it is, use random-effects analysis to produce it.

We plan to carry out the following subgroup analyses:

1. primiparous versus multiparous;
2. vaginal versus caesarean births;
3. younger women versus older women (as defined by the trial authors).

Subgroup analysis will be restricted to the review's primary outcomes.

We will assess subgroup differences by interaction tests available within RevMan (RevMan 2012). 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 will carry out sensitivity analyses to explore the effect of trial quality on the primary outcomes of this review. Trials will be divided into groups according to whether they are at low risk of bias as opposed to unclear or high risk of bias for important outcomes in the review. Where there is a risk of bias associated with a particular aspect of the study (e.g. inadequate allocation concealment or loss to follow-up in the intervention versus control arms), we will cary out a sensitivity analysis. If there is a risk of bias associated with a particular aspect of study quality, we will investigate via sensitivity analyses. For cluster-randomised trials, we will perform sensitivity analysis using a range of values for ICCs.

ACKNOWLEDGEMENTS

As part of the pre-publication editorial process, this protocol has been commented on by three peers (an editor and two referees who are external to the editorial team), members of the Pregnancy

and Childbirth Group's international panel of consumers and the Group's Statistical Adviser.

The National Institute for Health Research (NIHR) is the largest single funder of the Cochrane Pregnancy and Childbirth Group. The views and opinions expressed therein are those of the authors and do not necessarily reflect those of the NIHR, NHS or the Department of Health.

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* *Indicates the major publication for the study*

APPENDICES

Appendix 1. CINAHL search strategy (using EBSCO host)

S1. (MH "Postnatal Period") OR (MH "Postnatal Care") or postpartum or post partum or post-partum or peripartum
S2. (MH "Home Nursing, Professional") OR (MH "Home Visits") OR (MH "Telehealth+") OR "clinic visit" OR (MH "Office Visits")
S3. (MH "Nurses+") OR (MH "Midwifery Service+") OR (MH "Midwives+") OR (MH "Lactation Consultants") OR (MH "Pediatricians") OR (MH "Hospitalists") OR (MH "Physicians, Family")
S4. S1 AND S2
S5. S1 AND S3
S6. (MH "Clinical Trials+")
S7. (MH "Double-Blind Studies") or (MH "Single-Blind Studies") or (MH "Triple-Blind Studies")
S8. (MH "Random Assignment") or (MH "Simple Random Sample") or (MH "Stratified Random Sample") or (MH "Systematic Random Sample")
S9. (MH "Placebos")
S10. TX randomi?ed controlled trial
S11. TX random* N5 trial*
S12. (MH "Systematic Review") or (MH "Cochrane Library")
S13. (MH "Meta Analysis")
S14. S6 OR S7 OR S8 OR S9 OR S10 OR S11 OR S12 OR S13
S15. S4 OR S5
S16. S14 AND S15

Appendix 2. WHO ICTRP search strategy

postpartum or postnatal

CONTRIBUTIONS OF AUTHORS

Wendy Brodribb conceived the review. Ann Mathews and Wendy Brodribb wrote the draft protocol. Irena Zakarija-Grkovic, Glenda Hawley and Ben Mitchell reviewed and commented on the draft, and approved the final protocol.

DECLARATIONS OF INTEREST

None known.

SOURCES OF SUPPORT

Internal sources

- Discipline of General Practice, School of Medicine,The University of Queensland, Australia.
- Department of Family Medicine, University of Split, School of Medicine, Croatia.

External sources

- No sources of support supplied