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Elective repeat caesarean section versus induction of labour for women with a previous caesarean birth (Review)

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[Intervention Review]

Elective repeat caesarean section versus induction of labour for women with a previous caesarean birth

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ABSTRACT

Background

When a woman has had a previous caesarean birth and requires induction of labour in a subsequent pregnancy there are two options for her care, an elective repeat caesarean or planned induction of labour. While there are risks and benefits for both elective repeat caesarean birth and planned induction of labour, current sources of information are limited to non-randomised cohort studies. Studies designed in this way have significant potential for bias and consequently any conclusions based on these results are limited in their reliability and should be interpreted with caution.

Objectives

To assess, using the best available evidence, the benefits and harms of elective repeat caesarean section and planned induction of labour for women with a previous caesarean birth, who require induction of labour in a subsequent pregnancy.

Search methods

We searched the Cochrane Pregnancy and Childbirth Group Trials Register (31 October 2014).

Selection criteria

Randomised controlled trials with reported data that compared outcomes in mothers and babies for women who planned an elective repeat caesarean section with outcomes in women who planned induction of labour, where a previous birth had been by caesarean.

Data collection and analysis

There was no data extraction performed.

Main results

There were no randomised controlled trials identified.

Authors' conclusions

Both planned elective repeat caesarean section and planned induction of labour for women with a prior caesarean birth are associated with benefits and harms. Evidence for these care practices is drawn from non-randomised studies that are associated with potential bias. Any results and conclusions must therefore be interpreted with caution. Randomised controlled trials are required to provide the most reliable evidence regarding the benefits and harms of both planned elective repeat caesarean section and planned induction of labour for women with a previous caesarean birth.

PLAIN LANGUAGE SUMMARY

Elective repeat caesarean versus planned induction of labour for women with a previous caesarean birth

When a woman has had a previous caesarean birth and requires induction of labour in a subsequent pregnancy there are two options for her care, elective repeat caesarean or planned induction of labour. Both forms of care have benefits and risks associated with them. There were no randomised controlled trials to help women, their partners and their caregivers make this choice.

BACKGROUND

Description of the condition

Caesarean section is a common operation performed on women, with reported rates varying across the world. In developed countries, caesarean birth accounts for 25.5% of births in the United Kingdom (HSCIC 2013), 28.8% in Ireland (ESRI 2013), 31.6% in Australia (AIHW 2013), 32.8% in the United States (Martin 2013), and over 50% in some private hospitals in Argentina, Brazil and Paraguay (Villar 2006). The benefits and harms of both elective repeat caesarean birth and vaginal birth after caesarean (VBAC) are discussed more fully in the Cochrane Review 'Planned elective repeat caesarean section versus planned vaginal birth for women with a previous caesarean birth' (Dodd 2013).

For women with a prior caesarean birth who, in a subsequent pregnancy, require birth prior to the onset of spontaneous labour it is unclear whether labour should be induced, or whether a repeat elective caesarean should be performed. In a recent survey of practice, Australian and New Zealand obstetricians were asked about their willingness to offer induction of labour in a subsequent pregnancy to women with a previous caesarean birth (Dodd 2003). Induction of labour was an acceptable option, with 68% of respondents preferring this to caesarean section. However, in the setting of the 'post-term' pregnancy, willingness to proceed with induction of labour fell to 54%.

There are benefits and harms associated with both forms of care. This review will specifically consider the benefits and harms of elective repeat caesarean birth and induction of labour in a subsequent pregnancy for women with a previous caesarean birth. In addition to those concerns associated with elective repeat caesarean birth and VBAC (Dodd 2013), there are more specific concerns related to induction of labour in the presence of a scarred uterus. In particular, the possible increased risk of uterine scar rupture (where the previous caesarean scar breaks down) can be life-threatening for both the woman and her baby.

Description of the intervention

Pregnant women planning birth following a previous caesarean birth may plan an elective repeat caesarean birth or a VBAC.

Repeat elective caesarean birth is associated with an increase in the risk of complications such as bleeding, the need for blood transfusion, infection, damage to the bladder and bowel, and clots in the veins of the legs (called deep venous thrombosis). As the number of caesarean births for each individual woman increases so does the difficulty in performing surgery, due to adhesions and the risk of damage to the bladder or bowel at the time of surgery (Marshall 2011). There may also be difficulties in conceiving a further pregnancy or problems where the placenta develops over the scar in the uterus in a subsequent pregnancy (placenta praevia) (Marshall 2011). Occasionally the placenta may continue to develop into the muscle wall of the uterus (placenta accreta or placenta percreta). This may cause difficulties with the placenta being delivered after birth, and sometimes results in excessive bleeding. Babies born by caesarean may develop some difficulties with breathing (called transient tachypnoea of the newborn) and may need to spend time in a special care nursery. This is usually only for a short duration and most babies recover fully. Occasionally a baby may develop more serious problems with his or her breathing (called respiratory

distress syndrome) and may need extra oxygen, assistance with breathing, and a longer stay in the nursery. The risks of developing neonatal complications relate to the use of general anaesthesia and the age at which the baby is born (Hook 1997; Morrison 1995).

Vaginal birth after previous caesarean birth is associated with decreased maternal morbidity and a decrease in the risk of complications in subsequent pregnancies (ACOG 2010), whilst fulfilling the desire of some women to experience vaginal birth. Women with prior uterine surgery, including caesarean birth, are at increased risk of uterine scar rupture which can occur prior to labour or during VBAC. Whilst uncommon, this potentially serious event can be life-threatening for the woman and her baby. The decision to plan for a vaginal birth is further complicated when labour does not commence spontaneously and labour and birth require medical induction.

How the intervention might work

A large retrospective population-based review has assessed the risk of uterine scar rupture in over 20,000 women with a prior caesarean birth, who gave birth between 1987 and 1996 (Lydon-Rochelle 2001). Uterine scar rupture occurred at a rate of 4.5 per 1000 women (91 of 20,095 women). The risk of scar rupture was compared for women who did not labour and had an elective repeat caesarean section, for women whose labour commenced spontaneously, and for women whose labour was induced. For those women whose labour was induced, there was further consideration of the risks associated with prostaglandin induction agents and with 'non-prostaglandin' methods of induction (for example oxytocin infusion). Women with no labour who had an elective repeat caesarean birth acted as the control group and had an incidence of uterine rupture of 1.6 per 1000 women (11 of 6980 women). The incidence of uterine rupture was 5.2 per 1000 women where the onset of labour occurred spontaneously (56 of 10,789 women), increasing to 7.7 per 1000 women where labour was induced without prostaglandins (15 of 1960 women) and 24.5 per 1000 where labour was induced with prostaglandins (9 of 366 women). When compared with women who did not labour and had an elective repeat caesarean birth, the risk of uterine rupture was increased when labour occurred spontaneously (risk ratio (RR) 3.3; 95% confidence interval (CI) 1.8 to 6.0), when labour was induced without prostaglandins (RR 4.9; 95% CI 2.4 to 9.7), and when labour was induced with prostaglandins (RR 15.6; 95% CI 8.1 to 30.0). The paper did not specifically address the risks associated with different types of prostaglandin agents (for example prostaglandin E2 (PGE2), misoprostol).

In the large National Institute of Child Health and Human Development (NICHD) study, the use of prostaglandin-based medication to induce labour was associated with a non-significant increase in the risk of uterine rupture when compared with mechanical methods of induction of labour (such as the use of a Foley catheter) (Landon 2004). In this study, the risk of uterine rupture was 140 per 10,000 inductions using prostaglandins compared with 89 per 10,000 inductions using a Foley catheter to dilate the cervix (Landon 2004). However, a large retrospective study from Scotland assessing over 36,000 women with a prior caesarean birth, of whom 4600 underwent induction of labour with prostaglandins, indicated an increased risk of uterine rupture leading to perinatal death associated with the use of prostaglandin agents (4.5 per 10,000 non-induced labours versus 11 per 10,000 labours induced with prostaglandins in women with a prior caesarean) (Smith 2004). It is unclear whether the reported risk of uterine rupture related to the use of prostaglandins reflects medication induced changes in the connective tissue of the uterine scar or whether it is a marker of an unfavourable cervix (Bujold 2004; Kayani 2005), which in itself is a predictor of adverse outcomes associated with a trial of labour in women attempting VBAC (Kayani 2005; Landon 2005).

Controversy also exists around the use of oxytocin to induce and augment labour in women with a scarred uterus. The NICHD study suggests an increase in the risk of uterine rupture associated with the use of oxytocin, being 36 per 10,000 women without the use of oxytocin and increasing to 87 per 10,000 women where oxytocin is used (Landon 2005). It is unclear, however, whether this increased risk is confined to women undergoing induction of labour, or whether the risk extends also to those women undergoing augmentation of labour.

The administration of PGE2 gel or the use of intravenous Syntocinon® for women with a previous caesarean section is listed as a contraindication to use in the manufacturers' guidelines of both products. The American College of Obstetricians and Gynecologists released a committee opinion related to induction of labour after caesarean birth and the risk of uterine scar rupture, with the use of prostaglandins in this setting to be "discouraged" (ACOG 2002). Despite this, prostaglandins have been used widely to induce labour in women with an unfavourable cervix who have a scarred uterus. In an Australian survey of practice, almost two thirds of obstetricians indicated a reluctance to use vaginal prostaglandins, whereas 80% indicated a willingness to use Syntocinon® (Dodd 2003). In a Canadian survey of practice, 25% of obstetricians surveyed indicated a willingness to use prostaglandins for induction of labour in women with a previous caesarean birth (Brill 2003).

Why it is important to do this review

For women with a previous caesarean birth, controversy exists as to whether induction of labour and planned VBAC or elective repeat caesarean section constitutes optimal care. This review aimed to assess the benefits and harms of both forms of care.

OBJECTIVES

To assess, using the best available evidence, the benefits and harms of a policy of planned elective repeat caesarean section with a policy of induction of labour for women with a previous caesarean birth, who require induction of labour in a subsequent pregnancy. The primary outcomes related to success of induction of labour, need for caesarean section, maternal and neonatal mortality, and maternal and neonatal morbidity.

METHODS

Criteria for considering studies for this review

Types of studies

All published, unpublished, and ongoing randomised controlled trials with reported data, which compared outcomes for mothers or babies, or both. Women were randomised to a planned elective repeat caesarean birth or induction of labour where a prior birth was by caesarean section. Cluster trials and quasi-randomised trials were also eligible for inclusion. Trials published only as abstracts would be considered if they contained enough information to meet the inclusion criteria.

Types of participants

Women with one or more prior caesarean section (regardless of indication for primary caesarean birth, number of caesarean births, type of uterine scar, or method of closure of uterine incision) who required induction of labour in a subsequent pregnancy.

Types of interventions

Planned elective repeat caesarean birth versus induction of labour.

Types of outcome measures

Primary outcomes

1. Death or serious maternal morbidity (as defined by trial authors)
2. Death or serious infant morbidity (as defined by trial authors)

Secondary outcomes

Outcome measures for the woman:

1. vaginal birth;
2. instrumental vaginal birth;
3. caesarean birth;
4. caesarean birth for fetal distress;
5. uterine rupture (defined as clinically significant rupture involving the full thickness of the uterine wall and requiring surgical repair);
6. uterine scar dehiscence (defined as clinically asymptomatic disruption of the uterus that is discovered incidentally at surgery);
7. haemorrhage (blood loss greater than 500 mL at vaginal birth or greater than 100 mL at caesarean birth, or requiring blood transfusion, or both);
8. evacuation of the uterus after childbirth for postpartum haemorrhage or retained placental tissue;
9. hysterectomy for any complications resulting from birth;
10. vulval or perineal haematoma requiring evacuation;
11. deep vein thrombosis or thrombophlebitis requiring anticoagulant therapy;
12. pulmonary embolus requiring anticoagulant therapy;
13. pneumonia due to infection, aspiration or other causes;
14. adult respiratory distress syndrome;
15. wound infection (requiring prolongation of hospitalisation or readmission);
16. wound dehiscence;
17. puerperal infection;
18. damage to the bladder, bowel or ureter requiring surgical repair;
19. cervical laceration extending to the lower uterine segment or abnormal extension of the uterine incision;
20. occurrence of a fistula involving the genital tract and urinary or gastrointestinal tracts;
21. bowel obstruction;
22. paralytic ileus;
23. pulmonary oedema;
24. stroke (acute neurological deficit greater than 24 hours);
25. cardiac arrest;
26. respiratory arrest;
27. coagulopathy;
28. maternal death;
29. any other serious maternal complication related to birth;
30. level of pain after birth;
31. postnatal depression;
32. breastfeeding.

Outcome measures for the infant:

1. neonatal or perinatal death;
2. meconium-stained liquor;
3. Apgar score less than seven at five minutes;
4. birthweight;
5. admission to the neonatal intensive care unit;
6. birth trauma (subdural or intracerebral haemorrhage, spinal

cord injury, basal skull fracture, other fracture, peripheral nerve injury);
7. seizures at less than 24 hours of age;
8. laceration to baby at time of birth;
9. neonatal encephalopathy;
10. use of anticonvulsant therapy;
11. altered level of consciousness;
12. use of mechanical ventilation;
13. any respiratory disease;
14. severe respiratory distress syndrome requiring oxygen (as defined by trialists);
15. any oxygen requirement;
16. transient tachypnoea of the newborn;
17. use of tube feeding;
18. necrotizing enterocolitis;
19. proven systemic infection treated with antibiotics within 48 hours of life.

Longer-term outcomes for the woman:

1. return to 'normal' activities;
2. health and well-being assessment;
3. sexual health;
4. symptoms related to pelvic floor damage;
5. need for operative pelvic floor repair;
6. relationship with partner and child(ren);
7. future fertility (both voluntary and involuntary);
8. development of placenta praevia or placenta accreta or percreta in subsequent pregnancies;
9. mode of birth in subsequent pregnancy.

Longer-term outcomes for the child:

1. death after discharge from hospital;
2. disability in infancy;
3. disability in childhood.

Measures of satisfaction include:

1. woman satisfied with care;
2. woman preferences for care.

Costs include:

1. costs associated with elective repeat caesarean birth versus induction of labour;
2. maternal postnatal length of stay;
3. neonatal length of stay;
4. costs associated with readmission of mother;
5. costs associated with readmission of baby.

We planned to include outcomes in the analysis if data were available according to the original treatment allocation and reasonable measures were taken to minimise observer bias. Only outcomes with available data would have appeared in the analysis tables.

Search methods for identification of studies

The following methods section of this review is based on a standard template used by the Cochrane Pregnancy and Childbirth Group.

Electronic searches

We searched the Cochrane Pregnancy and Childbirth Group Trial's Register by contacting the Trials Search Co-ordinator (31 October 2014).

The Cochrane Pregnancy and Childbirth Group Trial's 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 e-mail 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.

We did not apply any language or date restrictions.

For the additional author searches that we carried out in an earlier version of this review (Dodd 2006), see Appendix 1.

Data collection and analysis

For methods used in the previous version of this review, please refer to Dodd 2012.

No new studies were identified and included for this update (2014). The following methods will be used in the next update. These are based on a standard template used by the Cochrane Pregnancy and Childbirth Group.

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.
We will create a study flow diagram to map out the number of records identified, included and excluded.

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 2014) and check for accuracy.
When information regarding any of the above is unclear, we will attempt to contact authors of the original reports for them 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); or
- 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 number of 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 that were imbalanced across groups; 'as treated' analysis done with substantial departure of intervention received from that assigned at randomisation);
- unclear risk of bias.

(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.

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 *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.

The quality of the evidence will be assessed using the GRADE approach (Schunemann 2009) in order to assess the quality of the body of evidence relating to the following outcomes for the main comparison:

1. death or serious maternal morbidity (as defined by trial authors);
2. death or serious infant morbidity (as defined by trial authors);
3. uterine rupture (defined as clinically significant rupture involving the full thickness of the uterine wall and requiring surgical repair);
4. haemorrhage (blood loss greater than 500 mL at vaginal birth or greater than 100 mL at caesarean birth, or requiring blood transfusion, or both);
5. hysterectomy for any complications resulting from birth.

GRADE profiler (GRADE 2008) will be used to import data from Review Manager 5.3 (RevMan 2014) in order to create 'Summary of findings' tables. A summary of the intervention effect and a measure of quality for each of the above outcomes will be produced using the GRADE approach. The GRADE approach uses five considerations (study limitations, consistency of effect, imprecision, indirectness and publication bias) to assess the quality of the body of evidence for each outcome. The evidence can be downgraded from 'high quality' by one level for serious (or by two levels for very serious) limitations depending on assessments for risk of bias, indirectness of evidence, serious inconsistency, imprecision of effect estimates or potential publication bias.

Measures of treatment effect

Dichotomous data

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

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 sample sizes using the methods described in the *Handbook for Systematic Reviews of Interventions* using an estimate of the intracluster 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 interaction between the effect of the 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.

Cross-over trials

Cross-over trials are not eligible for inclusion.

Dealing with missing data

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, that is 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 Tau², I² and Chi² statistics. We will regard heterogeneity as substantial if I² is greater than 30% and either Tau² is greater than zero or there is a low P value (less than 0.10) in the Chi² 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 2014). We will use fixed-effect model meta-analysis for combining data where it is reasonable to assume that studies are estimating the same underlying treatment effect; that is 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 model meta-analysis to produce an overall summary when an average treatment effect across trials is considered clinically meaningful. The random-effects model summary will be treated as the average of the 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 model analyses, the results will be presented as the average treatment effect with 95% confidence interval, together with the estimates of Tau² and I².

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. Previous vaginal birth versus no previous vaginal birth.
2. Single prior caesarean birth versus two or more prior caesarean births.

The following outcomes will be used in subgroup analysis:

1. death or serious maternal morbidity (as defined by trial authors);
2. death or serious neonatal morbidity (as defined by trial authors).

We will assess subgroup differences by interaction tests available within RevMan (RevMan 2014). We will report the results of subgroup analyses quoting the Chi² statistic and P value, and the interaction test I² value.

Sensitivity analysis

In future updates, we will carry out sensitivity analyses to explore the effect of trial quality assessed by concealment of allocation, high attrition rates, or both, with poor quality studies being excluded from the analyses in order to assess whether this makes any difference to the overall result.

RESULTS

Description of studies

Using the described search strategy, there were no randomised controlled trials identified which compared planned elective repeat caesarean birth with induction of labour for women with a previous caesarean birth.

Risk of bias in included studies

Not applicable.

Effects of interventions

Not applicable.

DISCUSSION

There were no randomised controlled trials identified assessing the benefits and harms of elective repeat caesarean section with induction of labour for women with a previous caesarean birth.

In the absence of sufficient quality data on which to base clinical decisions, uncertainty persists about the relative benefits and harms of induction of labour in addition to the safety of varying agents used to induce labour in women with a previous caesarean birth. Despite the manufacturers of both vaginal prostaglandin E2 gel and Syntocinon® listing the presence of a uterine scar as a contraindication to use in their product guidelines, they are widely used to induce labour in women with an unfavourable cervix who have had a previous caesarean section. In Australia and New Zealand, almost two-thirds of obstetricians are reluctant to use vaginal prostaglandins, whereas 80% indicate a willingness to use Syntocinon® (Dodd 2003).

The available prospective information regarding the safety of induction of labour is limited, and larger studies that are powered to detect differences in maternal and infant morbidity and mortality are required if this question is to be addressed satisfactorily. However, the question relating to the benefits and harms of induction of labour versus elective repeat caesarean section should be considered in the wider context of the benefits and harms of both elective repeat caesarean section and vaginal birth after caesarean section (VBAC) for women with a previous caesarean birth.

Prospective randomised studies should focus on the benefits and harms of planned induction of labour compared with planned repeat elective caesarean section for women with a scarred uterus who require induction of labour in a subsequent pregnancy. Until these questions have been answered, caution must be exercised in the use of agents for induction of labour in women with a prior caesarean birth.

AUTHORS' CONCLUSIONS

Implications for practice

The practices of elective repeat caesarean section and planned induction of labour for women with a prior caesarean birth are associated with benefits and harms. However, the evidence for the magnitude of these benefits and harms is drawn from non-randomised studies, which are associated with potential bias. The results and conclusions of these studies must therefore be interpreted with caution.

Implications for research

The available non-randomised studies of elective repeat caesarean section and planned induction of labour for women with a previous caesarean birth provide limited insight into the potential benefits and harms associated with both forms of care. Randomised controlled trials are required to provide the most reliable evidence regarding the benefits and harms of both elective repeat caesarean section and planned induction of labour for women with a previous caesarean birth, and should be considered in the wider context of benefits and harms associated with both elective repeat caesarean section and planned vaginal birth after caesarean section.

ACKNOWLEDGEMENTS

As part of the pre-publication editorial process, this review has been commented on by two peers (an editor and referee who is external to the editorial team), one or more members of the Cochrane Pregnancy and Childbirth Group's international panel of consumers, and the Group's Statistical Advisor (in 2006).

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* *Indicates the major publication for the study*

DATA AND ANALYSES

This review has no analyses.

APPENDICES

Appendix 1. Search methods used in previous version of this review

In addition, we searched the Cochrane Central Register of Controlled Trials (*The Cochrane Library* 2006, Issue 1) and PubMed (1966 to January 2006) using the following search terms: vaginal birth after caesarean; vaginal birth after cesarean; trial of labour; trial of labor; elective caesarean; elective cesarean; cesarean section, repeat; induction of labour; induction of labor; prostaglandins; prostaglandin E2; misoprostol; oxytocin; syntocinon.

WHAT'S NEW

Last assessed as up-to-date: 31 October 2014.

Date	Event	Description
31 October 2014	New citation required but conclusions have not changed	No new trials identified from updated search.
31 October 2014	New search has been performed	Search updated. Methods updated.

HISTORY

Protocol first published: Issue 3, 2004

Review first published: Issue 4, 2006

Date	Event	Description
27 January 2012	New search has been performed	Search updated. No new trial reports identified.
27 January 2012	New citation required but conclusions have not changed	Search updated.
2 September 2008	Amended	Converted to new review format.

CONTRIBUTIONS OF AUTHORS

J Dodd drafted the original protocol and review, and J Dodd and C Crowther contributed to all subsequent drafts. For this update, A Deussen drafted the update of the review and all authors contributed to the final draft. J Dodd is the guarantor of the review.

DECLARATIONS OF INTEREST

Jodie Dodd and Caroline Crowther were investigators on The BAC Study: Planned vaginal birth or elective repeat caesarean: Patient preference restricted cohort with nested randomised trial, which was published in 2012.

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Internal sources

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External sources

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- National Health and Medical Research Council (NHMRC), Australia.

DIFFERENCES BETWEEN PROTOCOL AND REVIEW

Methods have been updated to the current Pregnancy and Childbirth Group standard text (2014).

INDEX TERMS

Medical Subject Headings (MeSH)

*Cesarean Section, Repeat [adverse effects]; *Elective Surgical Procedures [adverse effects]; *Labor, Induced [adverse effects]; Vaginal Birth after Cesarean

MeSH check words

Female; Humans; Pregnancy