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

Dodd JM, Crowther CA, Grivell RM, Deussen AR

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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 for a subsequent pregnancy, two options are available for her care: an elective repeat caesarean and planned induction of labour. Although risks and benefits are associated with both elective repeat caesarean birth and planned induction of labour, current sources of information are limited to non-randomised cohort studies, and studies designed in this way have significant potential for bias. Consequently, any conclusions based on results of these studies are limited in their reliability and should be interpreted with caution.

### Objectives

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

### Search methods

We searched the Cochrane Pregnancy and Childbirth Trials Register (31 May 2017) and planned to search reference lists of retrieved studies.

### Selection criteria

Randomised controlled trials with reported data on comparison of outcomes in mothers and babies between women who planned an elective repeat caesarean section and women who planned induction of labour when a previous birth was performed by caesarean. Cluster trials and quasi-randomised trials were also eligible for inclusion. We would consider trials published only as abstracts if they provided enough information to meet review inclusion criteria.

## Data collection and analysis

We performed no data extraction. For future updates, if randomised controlled trials are identified, two review authors will independently assess trials for inclusion and risk of bias, and will extract data and check extracted data for accuracy. Review authors will assess the quality of the evidence using the GRADE approach.

## Main results

Review authors identified no randomised controlled trials.

## 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 has been drawn from non-randomised studies, which are associated with potential bias. Therefore, any results and conclusions presented must 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 section versus induction of labour for women with a previous caesarean birth

#### What is the issue?

When clinicians believe that intervention is needed in a pregnancy, which is the better option for a pregnant woman who has had one or more previous caesarean births - a planned caesarean section (a 'repeat elective caesarean section') or planned induction of labour?

#### Why is this important?

Risks and benefits are known to occur with a repeat elective caesarean section and with planned induction of labour. However, we don't know whether evidence indicates that we can expect better outcomes with one form of care over the other. Studies done so far have had strong potential for bias, which means that results may not be reliable.

#### What evidence did we find?

We looked for randomised trials that compared outcomes in mothers and babies when an elective repeat caesarean section was planned and when induction of labour was planned. We found no trials of this type.

#### What does this mean?

Caregivers and women faced with making a decision about labour and birth after a previous caesarean section cannot be informed by randomised trial evidence. A woman should discuss with her caregivers the benefits and risks of both courses of action. She and her caregivers should come to a shared decision for action that is based on the woman's wishes and priorities.

## BACKGROUND

### Description of the condition

Caesarean section is commonly performed, and reported rates of this operation vary across the world. A World Health Organiza-

tion global survey of 150 countries reported that in 2014, 18.6% of births were by caesarean section, and that Latin American and Caribbean regions report a rate of 40.5% (Betran 2016). Data from 121 countries revealed a trend towards increasing rates, with the average worldwide rate of caesarean section increasing almost threefold (from 6.7% to 19.1%) between 1990 and 2014. Some countries have had particularly steep rises, for example, Egypt re-

ported an increase from 4.6% to more than 50% over this period (Betran 2016). National studies have revealed that in developed countries, more than a quarter of births are performed by caesarean section: caesarean section accounts for 25.5% of births in the United Kingdom (HSCIC 2013), 28.8% in Ireland (ESRI 2013), 31.6% in Australia (AIHW 2013), and 32.8% in the United States (Martin 2013). The number of women undergoing caesarean section varies within countries, and overall rates may not reveal the disparity between urban and rural areas, and between different social and economic groups; although rates of caesarean section have not increased in low-resource settings such as sub-Saharan Africa, some private hospitals in Argentina, Brazil, and Paraguay have reported rates over 50% for several years (Villar 2006).

The benefits and harms of both elective repeat caesarean birth and vaginal birth after caesarean (VBAC) for women who have had a previous caesarean birth 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 require birth before the onset of spontaneous labour for a subsequent pregnancy, it is unclear whether labour should be induced, or whether a repeat elective caesarean should be performed. A survey of practice that asked Australian and New Zealand obstetricians about their willingness to offer induction of labour for a subsequent pregnancy in women with a previous caesarean birth (Dodd 2003) revealed that Induction of labour was an acceptable option, with 68% of respondents preferring this option to caesarean section. However, in the circumstance of 'post-term' pregnancy, willingness to proceed with induction of labour fell to 54%.

Benefits and harms are associated with both forms of care. This review will specifically consider the benefits and harms of elective repeat caesarean birth and induction of labour for a subsequent pregnancy in women with a previous caesarean birth. In addition to concerns associated with elective repeat caesarean birth and VBAC (Dodd 2013), more specific worries are related to induction of labour in the presence of a scarred uterus. In particular, the risk of uterine scar rupture (whereby the previous caesarean scar breaks down) may be increased by induction of labour, and this event 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 VBAC.

Repeat elective caesarean birth is associated with increased risk of complications such as bleeding, 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 an individual woman increases, so does the difficulty involved in performing surgery owing to adhesions and risk of damage to bladder or bowel at the time of surgery (Marshall

2011). Subsequent pregnancies may present difficulties in conceiving or problems with the placenta where the placenta develops over the scar on the uterus (Marshall 2011), increasing the risk that the placenta may form partially or completely over the internal opening of the uterus, called placenta praevia. Occasionally, the placenta may continue to develop into the muscle wall of the uterus (placenta accreta or placenta percreta). These complications may cause difficulties at birth and increase the risk of excessive bleeding. Babies born by caesarean may develop breathing difficulties (called 'transient tachypnoea of the newborn') and may need to spend time in a special care nursery - usually for a short duration with full recovery. Occasionally, a baby may develop more serious breathing problems (called 'respiratory distress syndrome') and may need extra oxygen, assistance with breathing, and a longer stay in the nursery. Risks of neonatal complications vary according 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 decreased risk of complications in subsequent pregnancies (ACOG 2010), whilst fulfilling the desire of some women to experience vaginal birth. However, women with prior uterine surgery, including caesarean birth, are at increased risk of uterine scar rupture, which can occur before labour or during VBAC. Whilst uncommon, this potentially serious event can be life-threatening for the woman and for her baby. The decision to plan for a vaginal birth is further complicated when labour does not commence spontaneously and medical induction is required.

## How the intervention might work

A large retrospective population-based review assessed the risk of uterine scar rupture in more than 20,000 women with a prior caesarean that occurred between 1987 and 1996 (Lyndon-Rochelle 2001). Uterine scar rupture occurred at a birth rate of 4.5 per 1000 women (91 of 20,095 women). Review authors compared risk of scar rupture 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 women whose labour was induced, review authors further considered risks associated with prostaglandin induction agents and with 'non-prostaglandin' methods of induction (e.g. 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 for whom onset of labour occurred spontaneously (56 of 10,789 women), and it increased to 7.7 per 1000 women for whom labour was induced without prostaglandins (15 of 1960 women) and 24.5 per 1000 women for whom labour was induced with prostaglandins (9 of 366 women). When compared with women who did not labour and had an elective repeat caesarean birth, 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 2001 review did not specifically address risks associated with different types of prostaglandin agents (e.g. prostaglandin E2 (PGE2), misoprostol).

In a large National Institute of Child Health and Human Development (NICHD) study, use of prostaglandin-based medication to induce labour was associated with a non-significant increase in risk of uterine rupture when compared with mechanical methods of labour induction (such as use of a Foley catheter) (Landon 2004). In this study, risk of uterine rupture was 140 per 10,000 inductions with prostaglandins compared with 89 per 10,000 inductions with use of a Foley catheter to dilate the cervix (Landon 2004). However, a large retrospective study from Scotland that assessed more than 36,000 women with a prior caesarean birth, of whom 4600 underwent induction of labour with prostaglandins, reported increased risk of uterine rupture leading to perinatal death associated with use of prostaglandin agents (4.5 per 10,000 non-induced labours vs 11 per 10,000 labours induced with prostaglandins) (Smith 2004). It remains unclear whether the reported risk of uterine rupture related to 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 surrounds use of oxytocin to induce and augment labour in women with a scarred uterus. The NICHD study suggests increased risk of uterine rupture associated with use of oxytocin (from 36 per 10,000 women without oxytocin to 87 per 10,000 women with oxytocin) (Landon 2005). However, whether this increased risk is confined to women undergoing induction of labour, or whether the risk extends to women undergoing augmentation of labour, remains unclear.

Administration of prostaglandins or intravenous oxytocin is contraindicated in women with a previous caesarean section, according to manufacturers' guidelines for both products. The American College of Obstetricians and Gynecologists released a committee opinion related to induction of labour after caesarean birth and risk of uterine scar rupture, which stated that use of prostaglandins in this setting is 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 revealed reluctance to use vaginal prostaglandins, whereas 80% conveyed willingness to use oxytocin (Dodd 2003). In a Canadian survey of practice, 25% of obstetricians indicated 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 continues 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 versus a policy of induction of labour for women with a previous caesarean birth who require induction of labour for a subsequent pregnancy. Primary outcomes include 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 that compared outcomes for mothers or babies, or both, and reported data. Investigators randomised women to a planned elective repeat caesarean birth or to induction of labour after prior birth by caesarean section. Cluster trials and quasi-randomised trials were also eligible for inclusion. We would consider trials published only as abstracts if they provided enough information to meet review inclusion criteria.

#### Types of participants

Women with one or more prior caesarean sections (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 for a subsequent pregnancy.

#### Types of interventions

Planned elective repeat caesarean birth versus induction of labour.

#### Types of outcome measures

#### Primary outcomes

- Death or serious maternal morbidity (as defined by trial authors)
- Death or serious infant morbidity (as defined by trial authors)

## Secondary outcomes

### Outcome measures for the woman

- Vaginal birth
- Instrumental vaginal birth
- Caesarean birth
- Caesarean birth for fetal distress
- Uterine rupture (defined as clinically significant rupture involving the full thickness of the uterine wall and requiring surgical repair)
  - Uterine scar dehiscence (defined as clinically asymptomatic disruption of the uterus that is discovered incidentally at surgery)
    - Haemorrhage (blood loss > 500 mL at vaginal birth or > 100 mL at caesarean birth, or requiring blood transfusion, or both)
    - Evacuation of the uterus after childbirth for postpartum haemorrhage or retained placental tissue
    - Hysterectomy for any complications resulting from birth
    - Vulval or perineal haematoma requiring evacuation
    - Deep vein thrombosis or thrombophlebitis requiring anticoagulant therapy
    - Pulmonary embolus requiring anticoagulant therapy
    - Pneumonia due to infection, aspiration, or other causes
    - Adult respiratory distress syndrome
    - Wound infection (requiring prolongation of hospitalisation or re-admission)
      - Wound dehiscence
      - Puerperal infection
      - Damage to the bladder, bowel, or ureter requiring surgical repair
        - Cervical laceration extending to the lower uterine segment or abnormal extension of the uterine incision
          - Occurrence of a fistula involving the genital tract and urinary or gastrointestinal tract
            - Bowel obstruction
            - Paralytic ileus
            - Pulmonary oedema
            - Stroke (acute neurological deficit > 24 hours)
            - Cardiac arrest
            - Respiratory arrest
            - Coagulopathy
            - Maternal death
            - Any other serious maternal complication related to birth
            - Level of pain after birth
            - Postnatal depression

- Breastfeeding

### Outcome measures for the infant

- Neonatal or perinatal death
- Meconium-stained liquor
- Apgar score < 7 at 5 minutes
- Birthweight
- Admission to the neonatal intensive care unit
- Birth trauma (subdural or intracerebral haemorrhage, spinal cord injury, basal skull fracture, other fracture, peripheral nerve injury)
  - Seizures at < 24 hours of age
  - Laceration to baby at time of birth
  - Neonatal encephalopathy
  - Use of anticonvulsant therapy
  - Altered level of consciousness
  - Use of mechanical ventilation
  - Any respiratory disease
    - Severe respiratory distress syndrome requiring oxygen (as defined by trialists)
      - Any oxygen requirement
      - Transient tachypnoea of the newborn
      - Use of tube feeding
      - Necrotising enterocolitis
      - Proven systemic infection treated with antibiotics within 48 hours of life

### Longer-term outcomes for the woman

- Return to 'normal' activities
- Health and well-being assessment
- Sexual health
- Symptoms related to pelvic floor damage
- Need for operative pelvic floor repair
- Relationship with partner and child(ren)
- Future fertility (both voluntary and involuntary)
- Development of placenta praevia or placenta accreta or percreta in subsequent pregnancies
  - Mode of birth in subsequent pregnancy

### Longer-term outcomes for the infant

- Death after discharge from hospital
- Disability in infancy
- Disability in childhood

### Measures of satisfaction

- Woman satisfied with care
- Woman preferences for care

## Costs

- Elective repeat caesarean birth versus induction of labour
- Postnatal length of stay
- Neonatal length of stay
- Re-admission of mother
- Re-admission of baby

We planned to include outcomes in the analysis if data were available according to the original treatment allocation, and if 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 Review Group.

### Electronic searches

We searched Cochrane Pregnancy and Childbirth's Trials Register by contacting their Information Specialist (31 May 2017)

The Register is a database containing over 23,000 reports of controlled trials in the field of pregnancy and childbirth. For full search methods used to populate Pregnancy and Childbirth's Trials Register including the detailed search strategies for CENTRAL, MEDLINE, Embase and CINAHL; the list of handsearched journals and conference proceedings, and the list of journals reviewed via the current awareness service, please follow this link to the editorial information about the [Cochrane Pregnancy and Childbirth](#) in the Cochrane Library and select the '*Specialized Register*' section from the options on the left side of the screen.

Briefly, Cochrane Pregnancy and Childbirth's Trials Register is maintained by their Information Specialist and contains trials identified from:

1. monthly searches of the Cochrane Central Register of Controlled Trials (CENTRAL);
2. weekly searches of MEDLINE (Ovid);
3. weekly searches of Embase (Ovid);
4. monthly searches of CINAHL (EBSCO);
5. handsearches of 30 journals and the proceedings of major conferences;
6. weekly current awareness alerts for a further 44 journals plus monthly BioMed Central email alerts.
7. scoping searches of [ClinicalTrials.gov](#) and the WHO International Clinical Trials Registry Platform (ICTRP)

Search results are screened by two people and the full text of all relevant trial reports identified through the searching activities described above is reviewed. Based on the intervention described, each trial report is assigned a number that corresponds to a specific Pregnancy and Childbirth review topic (or topics), and is

then added to the Register. The Information Specialist searches the Register for each review using this topic number rather than keywords. This results in a more specific search set that would have been fully accounted for in the relevant review sections (Included, Excluded, Awaiting Classification or Ongoing).

### Searching other resources

We planned to search the reference lists of retrieved studies. We did not apply language or date restrictions.

## Data collection and analysis

For methods used in the previous version of this review, please refer to [Dodd 2012](#).

We identified and included no new studies for this update (2016). For the next update, we will use the methods described below, which are based on a standard template used by the Cochrane Pregnancy and Childbirth Review Group.

### Selection of studies

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

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

### Data extraction and management

We will design a form on which to record extracted data. For eligible studies, two review authors will extract data using the prepared form. We will resolve discrepancies through discussion or, if required, will consult a third person. We will enter data into Review Manager software ([RevMan 2014](#)) and will check entered data for accuracy.

When information regarding any of the above is unclear, we will attempt to contact authors of the original reports to request additional 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](#).

### Assessment of the quality of the evidence using the GRADE approach

For future updates if trial data become available, the quality of the evidence will be assessed using the GRADE approach as outlined in the [GRADE handbook](#) 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.

[GRADEpro](#) Guideline Development Tool will be used to import data from Review Manager 5.3 ([RevMan 2014](#)) 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 ratios with 95% confidence intervals.

#### Continuous data

For continuous data, we will use mean differences if outcomes are measured the same way by different trials. We will use standardised mean differences when combining trials that measured the same outcome by different methods.

### Unit of analysis issues

#### Cluster-randomised trials

We will include in the analyses cluster-randomised trials along with individually randomised trials. We will adjust sample sizes as recommended in the *Cochrane Handbook for Systematic Reviews of Interventions*, using an estimate of the intracluster correlation coefficient (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 will conduct sensitivity analyses to investigate effects of variation in the ICC. If we identify both cluster-randomised trials and individually randomised trials, we plan to synthesise relevant information. We will consider it reasonable to combine the results of both types of trials if we note little heterogeneity between study designs, and if interaction between effects of the intervention and choice of randomisation unit is considered unlikely.

We will also acknowledge heterogeneity in the randomisation unit and will perform a sensitivity analysis to investigate randomisation unit effects.

#### 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 perform sensitivity analysis to explore the impact of including studies with high levels of missing data in the overall assessment of treatment effect.

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 in the analyses all participants randomised to each group, and will analyse all participants in the group to which they were allocated, regardless of whether they received the allocated intervention. The denominator for each outcome in each trial will be the number randomised minus the number of participants whose outcomes are known to be missing.

#### Assessment of heterogeneity

We will assess statistical heterogeneity in each meta-analysis by using Tau<sup>2</sup>, I<sup>2</sup>, and Chi<sup>2</sup> statistics. We will regard heterogeneity as substantial if I<sup>2</sup> is greater than 30% and either Tau<sup>2</sup> is greater than zero or the P value is low (< 0.10) in the Chi<sup>2</sup> test for heterogeneity.

#### Assessment of reporting biases

If we include 10 or more studies in the meta-analysis, we will use funnel plots to investigate reporting biases (such as publication

bias). We will assess funnel plot asymmetry visually. If asymmetry is suggested by a visual assessment, we will perform exploratory analyses to investigate this.

### Data synthesis

We will carry out statistical analysis using Review Manager software (RevMan 2014). We will use fixed-effect model meta-analysis in combining data when it is reasonable to assume that studies are estimating the same underlying treatment effect, that is, when trials are examining the same intervention, and trial populations and methods are judged sufficiently similar. If clinical heterogeneity is sufficient to suggest that underlying treatment effects differ between trials, or if we detect substantial statistical heterogeneity, we will use random-effects model meta-analysis to produce an overall summary when we consider an average treatment effect across trials to be clinically meaningful. We will treat the random-effects model summary as the average of the range of possible treatment effects, and we will discuss the clinical implications of differing treatment effects between trials. If the average treatment effect is not clinically meaningful, we will not combine trials.

If we use random-effects model analyses, we will present results as the average treatment effect with 95% confidence interval, together with estimates of  $Tau^2$  and  $I^2$ .

### Subgroup analysis and investigation of heterogeneity

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

We plan to carry out the following subgroup analyses.

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

We will use the following outcomes in performing subgroup analysis.

- Death or serious maternal morbidity (as defined by trial authors).
- Death or serious neonatal morbidity (as defined by trial authors).

We will assess subgroup differences by performing interaction tests available within RevMan (RevMan 2014). We will report results of subgroup analyses by quoting the  $Chi^2$  statistic and the P value, along with the interaction test  $I^2$  value.

### Sensitivity analysis

In future updates, we will carry out sensitivity analyses to explore the effect of trial quality as assessed by concealment of allocation, high attrition rates, or both, and will exclude poor quality studies

from the analyses to assess whether this makes any difference in the overall result.

## RESULTS

### Description of studies

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

### Risk of bias in included studies

Not applicable.

### Effects of interventions

Not applicable.

## DISCUSSION

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

In the absence of data of sufficient quality on which to base clinical decisions, uncertainty persists about the relative benefits and harms of induction of labour, as well as the safety of various agents used to induce labour in women with a previous caesarean birth. A recent meta-analysis examining the efficacy and safety of different methods used for induction of labour revealed that many trials examining prostaglandins excluded women who had had a previous caesarean section (Alfirevic 2016). Although the manufacturers of both vaginal prostaglandins and intravenous oxytocin list in their product guidelines the presence of a uterine scar as a contraindication to use, these products 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 willingness to use oxytocin (Dodd 2003).

Available prospective information regarding the safety of induction of labour is limited, and larger studies powered to detect differences in maternal and infant morbidity and mortality are required if this question is to be addressed satisfactorily. However, questions related 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 compare the benefits and harms of planned induction of labour versus planned repeat elective caesarean section for women with a scarred uterus who require induction of labour for a subsequent pregnancy. Until these questions have been answered, clinicians must exercise caution when using agents to induce 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, evidence showing the magnitude of these benefits and harms has been drawn from non-randomised studies, which are associated with potential bias. Therefore, results and conclusions available in the literature must be interpreted with caution.

## Implications for research

Available non-randomised studies of elective repeat caesarean section and planned induction of labour for women with a previous caesarean birth have provided limited insight into the potential benefits and harms associated with both forms of care. Randomised controlled trials are required to provide 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 trial findings 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.

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\* Indicates the major publication for the study

## DATA AND ANALYSES

This review has no analyses.

## WHAT'S NEW

Last assessed as up-to-date: 31 May 2017.

Date	Event	Description
31 May 2017	New citation required but conclusions have not changed	No new trials identified through updated search
31 May 2017	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
31 October 2014	New citation required but conclusions have not changed	No new trials identified through updated search
31 October 2014	New search has been performed	Search updated. Methods updated
27 January 2012	New citation required but conclusions have not changed	Search updated
27 January 2012	New search has been performed	Search updated. No new trial reports identified
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 ([Crowther 2012](#)).

Jodie Dodd: is the recipient of an NHMRC Practitioner Fellowship.

Rosalie Grivell: none known.

Andrea R Deussen: none known.

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- The University of Auckland, The Liggins Institute, New Zealand.

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- National Institute for Health Research, UK.

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

## DIFFERENCES BETWEEN PROTOCOL AND REVIEW

We have updated methods to the current Pregnancy and Childbirth Group standard text (2014).

## INDEX TERMS

### Medical Subject Headings (MeSH)

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

### MeSH check words

Female; Humans; Pregnancy