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Hofmeyr GJ, Kulier R, West HM

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[Intervention Review]

Expedited versus conservative approaches for vaginal delivery in breech presentation

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ABSTRACT

Background

In a vaginal breech birth there may be benefit from rapid delivery of the baby to prevent progressive acidosis. However, this needs to be weighed against the potential trauma of a quick delivery.

Objectives

The objective of this review was to assess the effects of expedited vaginal delivery (breech delivery from umbilicus to delivery of the head within one contraction) on perinatal outcomes.

Search methods

We searched the Cochrane Pregnancy and Childbirth Group's Trials Register (31 May 2015) and reference lists of retrieved studies.

Selection criteria

Randomised trials of expedited vaginal breech delivery compared with delivery not routinely expedited in women undergoing vaginal breech delivery.

Data collection and analysis

Two review authors independently assessed the one identified trial for inclusion.

If studies are included in future updates, two review authors will assess risk of bias, extract data and check data for accuracy.

Main results

No studies were included.

Authors' conclusions

There is not enough evidence to evaluate the effects of expedited vaginal breech delivery.

PLAIN LANGUAGE SUMMARY

Quick versus standard vaginal birth when a baby is bottom-down

Expedited versus conservative approaches for vaginal delivery in breech presentation (Review)

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What is the issue?

The best outcomes in childbirth for both mothers and babies are when babies are born head-first. If the baby is in another position, there is a higher risk of complications including the need for caesarean section. In a 'breech presentation' the unborn baby is bottom-down instead of head-down. This review looked for evidence about whether a quicker birth might be better in such cases.

Doctors call the quicker birth 'expedited delivery'. It means that the baby's head appears just one contraction after its tummy has appeared.

Why is this important?

Quicker births might prevent babies from not getting enough oxygen. This is a risk in breech births because the cord can get squashed. If the cord is squashed, not enough blood gets to the baby. However, the methods used to make the birth quicker might harm the baby or the mother.

What evidence did we find?

No reliable studies were found - (we searched for studies up to May 2015).

What does this mean?

There is no evidence from reliable studies about whether speeding up the birth ('expedited delivery') is helpful if the baby is bottom down.

BACKGROUND

Description of the condition

Breech presentation occurs in 3% to 5% of all pregnancies at term, accounting for the greatest proportion of non-cephalic presentations. Factors which have been associated with breech presentation include: nulliparity; previous breech birth; uterine anomaly; contracted pelvis; use of anticonvulsant drugs; placenta praevia; cornual placenta; decreased or increased amniotic fluid volume; extended fetal legs; multiple pregnancy; prematurity; short umbilical cord; decreased fetal activity; impaired fetal growth; fetal anomaly; and fetal death.

Breech babies tend to be at higher risk of adverse outcomes, with increased neonatal morbidity and mortality (Conde-Agudelo 2000), although it is unclear whether this is due to pre-existing vulnerabilities (perhaps also the factors that caused the initial breech presentation), or the effects of delivery in this position. The interpretation of observational studies that compare outcomes after vaginal breech birth and cephalic birth is confounded by the fact that breech presentation per se appears to be a marker for poor perinatal outcome. For example, the incidence of childhood handicap among singleton breech babies, born at term, has been found to be high (19.4%) and similar for those delivered following trial of labour and those following an elective caesarean section (Danielian 1996). Thus, poor outcomes following vaginal breech birth may be the result of underlying conditions causing breech presentation rather than damage during delivery. However, the care during labour, the delivery methods used, and skill of the birth attendant may also influence outcome.

Some babies will spontaneously turn to a cephalic position before birth, and others can be rotated using external cephalic version (ECV). Other reviews address whether ECV before term or at term can safely prevent breech delivery (Hofmeyr 2012b; Hutton 2006), and which interventions can help turn babies using ECV (Cluver 2012). However, for those babies persisting in the breech position, a decision will need to be taken over the method of delivery. The majority of women with a breech presenting baby would prefer a vaginal birth although most would choose caesarean section if there is a medical indication (Hildingsson 2002). For the singleton fetus in breech presentation, caesarean section has been shown to be safer for the fetus than vaginal birth in many settings (Hofmeyr 2003), although a large prospective study suggested that vaginal breech delivery may be safe under certain conditions with experienced practitioners (Goffinet 2006). In some settings caesarean section may not be possible, vaginal delivery may progress too rapidly to perform a caesarean, or vaginal delivery might be the preferred option.

Description of the intervention

During vaginal breech delivery, fetal oxygenation is considered to be potentially impaired once the umbilicus is delivered, due to umbilical cord compression. A conflict exists between the need to deliver the baby rapidly to avoid progressive acidosis, and the need to avoid trauma due to over-hasty delivery.

Why it is important to do this review

Information from randomised trials is required to determine whether expedited or conservative vaginal delivery is better for infants and their mothers.

OBJECTIVES

To assess the effects on neonatal morbidity and mortality of a policy of breech delivery from umbilicus to delivery of the head within one contraction (expedited delivery).

METHODS

Criteria for considering studies for this review

Types of studies

Randomised controlled clinical trials or cluster-randomised trials comparing the effects of expedited vaginal breech delivery with a control group (delivery not routinely expedited). Quasi-randomised and cross-over trials were not eligible for inclusion.

Types of participants

Women undergoing vaginal breech delivery.

Types of interventions

Expedited delivery protocol compared with conventional delivery.

Types of outcome measures

Primary outcomes

1. Death or serious neonatal morbidity (significant trauma, encephalopathy, intracranial haemorrhage, intensive care unit (ICU) admission > 24 hours, or as defined by trial authors).

Secondary outcomes

1. Duration of second stage of labour.
2. Difficulty with delivery.
3. Five-minute Apgar score less than seven or death.
4. Umbilical artery pH less than 7.2 or death.
5. Neonatal trauma or death.
6. Neonatal encephalopathy or death.
7. Neonatal intracranial haemorrhage or death.
8. ICU admission > 24 hours or death.
9. Childhood disability or death.
10. Perinatal death.
11. Maternal trauma.
12. Maternal satisfaction.

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's Trials Register by contacting the Trials Search Co-ordinator (31 May 2015).

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 (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.

Details of the 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 can be found in the 'Specialized Register' section within the editorial information about the [Cochrane Pregnancy and Childbirth Group](#).

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

Searching other resources

We searched the reference lists of retrieved studies.

We did not apply any language or date restrictions.

Data collection and analysis

For methods used in the previous version of this review, see [Hofmeyr 2012a](#).

No new studies were identified as a result of the updated search.

The following methods section of this review is based on a standard template used by the Cochrane Pregnancy and Childbirth Group. These methods will be used for the next update.

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, at least 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 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.

(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* (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 quality of the evidence

For the next update 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 neonatal morbidity

2. Five-minute Apgar score less than seven
3. Umbilical artery pH less than 7.2
4. Childhood disability or death
5. Maternal trauma
6. Maternal satisfaction

GRADE profiler (GRADEpro 2014) 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 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 sample sizes or standard errors using the methods described in the *Handbook* [Section 16.3.4 or 16.3.6] 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 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 or subgroup analysis to investigate the effects of the randomisation unit.

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, 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 τ^2 , I^2 and χ^2 statistics. We will regard heterogeneity as substantial if an I^2 is greater than 30% and either a τ^2 is greater than zero, or there is a low P value (less than 0.10) in the χ^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 2014). 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 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 analyses, we will present the results as the average treatment effect with 95% confidence intervals, and the estimates of τ^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. Nulliparous, multiparous, parity mixed or not stated.
2. Suspected fetal compromise, no suspected fetal compromise, fetal compromise mixed or not stated.

All outcomes will be used in subgroup analysis.

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

Sensitivity analysis

We plan to 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

Results of the search

One study was identified and excluded (see Figure 1).

Figure 1. Study flow diagram.

Included studies

No studies were included.

Excluded studies

One study was excluded [Arulkumaran 1989](#) because it was a quasi-randomised trial (used alternate method of allocation). (see [Characteristics of excluded studies](#)).

Risk of bias in included studies

The one study considered did not meet the prespecified methodological criteria.

Effects of interventions

No included trials.

DISCUSSION

The one published trial has been excluded because of methodological problems.

AUTHORS' CONCLUSIONS**Implications for practice**

No information is available from randomised trials which should influence current practice.

Implications for research

The hypothesis presented in the excluded study considered ([Arulkumaran 1989](#)) is that progressive acidosis during the period from exposure of the umbilical cord to delivery of the head may be reduced by expediting the cord-to-head delivery time to occur during a single contraction. The concept appears to be worth subjecting to further randomised evaluation.

ACKNOWLEDGEMENTS

Denise Atherton for administrative assistance; Lynn Hampson for the literature search.

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CHARACTERISTICS OF STUDIES

Characteristics of excluded studies [ordered by study ID]

Study	Reason for exclusion
Arulkumaran 1989	Quasi-randomised trial (alternate method of allocation).

DATA AND ANALYSES

Comparison 1. Expedited vaginal delivery in breech presentation

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Death or serious neonatal morbidity	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
2 Duration of 2nd stage of labour	0	0	Mean Difference (IV, Fixed, 95% CI)	0.0 [0.0, 0.0]
3 Difficulty with delivery	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
4 5 minute Apgar score < 7 or death	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
5 Umbilical artery pH < 7.2 or death	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
6 Neonatal trauma or death	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
7 Neonatal encephalopathy or death	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
8 Neonatal intracranial haemorrhage or death	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
9 ICU admission >24 hours or death	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
10 Childhood disability or death	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
11 Perinatal death	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
12 Maternal trauma	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
13 Maternal satisfaction	0	0	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]

WHAT'S NEW

Date	Event	Description
8 May 2019	Amended	Added Declarations of interest statement for Justus Hofmeyr.

HISTORY

Protocol first published: Issue 2, 1996

Review first published: Issue 2, 1996

Date	Event	Description
23 November 2015	Amended	We have added a revised plain language summary
31 May 2015	New citation required but conclusions have not changed	Review updated.
31 May 2015	New search has been performed	Search updated. No new trials identified.
2 February 2012	New search has been performed	Search updated. No new trials identified.
2 February 2012	New citation required but conclusions have not changed	Review updated.
2 July 2010	Amended	Contact details edited.
2 September 2008	Amended	Converted to new review format.
29 June 2007	New search has been performed	Search updated. No new trials identified.
25 October 2004	New search has been performed	Search updated. No new trials identified.
27 January 2003	New search has been performed	Search updated. No new trials identified.

CONTRIBUTIONS OF AUTHORS

GJ Hofmeyr prepared the original version and maintains the review. R Kulier revised and quality-checked the 2012 review. H West updated the 2015 version of the review.

DECLARATIONS OF INTEREST

Helen West is paid to work on Cochrane reviews by a grant to Cochrane Pregnancy and Childbirth. The views and opinions expressed therein are those of the authors and do not necessarily reflect those of the Systematic Reviews Programme, NIHR, NHS or the Department of Health.

Justus Hofmeyr receives royalties from UpToDate for chapters related to [breech pregnancy](#), [delivery of a baby in breech presentation](#) and [external cephalic version](#). UpToDate is an electronic publication by Wolters Kluwer to disseminate evidence-based medicine (such as Cochrane reviews).

SOURCES OF SUPPORT

Internal sources

- University of the Witwatersrand, South Africa.
- Department of Obstetrics and Gynaecology, Geneva University Hospital, Switzerland.
- (HW) Cochrane Pregnancy and Childbirth Group, Department of Women's and Children's Health, The University of Liverpool, Liverpool, UK.

External sources

- South African Medical Research Council, South Africa.
- (HW) National Institute for Health Research (NIHR), UKNIHR Cochrane Programme Grant Project: 13/89/05 – Pregnancy and childbirth systematic reviews to support clinical guidelines, UK.

DIFFERENCES BETWEEN PROTOCOL AND REVIEW

Outcomes have been revised from "Perinatal morbidity and mortality and maternal trauma and satisfaction, as set out in the tables of comparisons" to the current list of specific primary and secondary outcomes.

INDEX TERMS

Medical Subject Headings (MeSH)

*Breech Presentation; Delivery, Obstetric [*methods]

MeSH check words

Female; Humans; Pregnancy