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[Intervention Review]

Prophylactic versus therapeutic amnioinfusion for oligohydramnios in labour

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ABSTRACT

Background

Amnioinfusion aims to relieve umbilical cord compression during labour by infusing a liquid into the uterine cavity.

Objectives

The objective of this review was to assess the effects of prophylactic amnioinfusion for women in labour with oligohydramnios, but not fetal heart deceleration, compared with therapeutic amnioinfusion only if fetal heart rate decelerations or thick meconium-staining of the liquor occur.

Search methods

We searched the Cochrane Pregnancy and Childbirth Group's Trials Register (28 February 2012).

Selection criteria

Randomised trials comparing prophylactic amnioinfusion in women in labour with oligohydramnios but not fetal heart rate deceleration in labour with therapeutic amnioinfusion.

Data collection and analysis

The authors assessed trial quality and extracted data.

Main results

One randomized trial of 116 women was included. No differences were found in the rate of caesarean section (risk ratio 1.29, 95% confidence interval 0.60 to 2.74). There were no differences in cord arterial pH, oxytocin augmentation, neonatal pneumonia or postpartum endometritis. Prophylactic amnioinfusion was associated with increased intrapartum fever (risk ratio 3.48, 95% confidence interval 1.21 to 10.05).

Authors' conclusions

There appears to be no advantage of prophylactic amnioinfusion over therapeutic amnioinfusion carried out only when fetal heart rate decelerations or thick meconium-staining of the liquor occur.

PLAIN LANGUAGE SUMMARY**Prophylactic versus therapeutic amnioinfusion for oligohydramnios in labour**

Amnioinfusion can help when a baby is surrounded by too little fluid in the womb (oligohydramnios) and is showing distress, but is not needed otherwise.

Oligohydramnios is a condition where too little fluid surrounds the baby in the womb (uterus). Oligohydramnios does not seem to affect some babies, but others may show signs of distress, such as unusual heart rates or the passing of a bowel motion (meconium). Oligohydramnios can be relieved by injections of extra liquid (salt or ringers lactate solution) through the woman's vagina or abdomen into the womb (amnioinfusion). The review of one trial, involving 116 women, found that amnioinfusion for oligohydramnios helps when the baby shows signs of distress. If the baby shows no signs of distress from oligohydramnios, then amnioinfusion is not helpful.

BACKGROUND

Amnioinfusion for oligohydramnios has been used both prophylactically and therapeutically. It has been found to be effective in reducing fetal heart rate deceleration and caesarean section rate ([Abdel-Aleem 2005](#); [Amin 2003](#); [Persson-Kjerstadius 1999](#)). It has been described as a method of preventing or relieving umbilical cord compression during labour ([Miyazaki 1983](#)). Saline or Ringer's lactate is infused transcervically through a catheter into the uterine cavity or transabdominally through a "spinal" needle when membranes are intact.

In a study conducted in monkeys, removal of amniotic fluid resulted in variable decelerations and saline amnioinfusion relieved the decelerations ([Gabbie 1976](#)). The fetal heart rate abnormalities are considered to be due often to umbilical cord compression particularly when there is oligohydramnios.

Readers are referred to reviews of the subject ([Lameier 1993](#); [Hofmeyr 1996](#)). In a randomised trial, a decrease in caesarean section rate in nulliparous women in labour with cord compression heart rate pattern was found in women who had amnioinfusion ([Miyazaki 1985](#)). Nageotte noted that amnioinfusion resulted in significantly reduced frequency and severity of decelerations of the fetal heart in labour ([Nageotte 1991](#)). On the basis of the outcome of earlier studies on amnioinfusion, amnioinfusion either transcervically or transabdominally has been used in the dilution of thick meconium, treatment of abnormal fetal heart rate patterns and prophylactically or therapeutically in cases of oligohydramnios resulting from rupture of membranes.

An important question arising from these findings is whether prophylactic amnioinfusion has any advantage over a policy of using amnioinfusion only therapeutically when fetal heart rate decelerations or meconium-stained liquor occur.

OBJECTIVES

To assess, from the best available evidence, the effect on maternal and perinatal outcome of prophylactic amnioinfusion for oligohydramnios versus therapeutic amnioinfusion only if fetal heart rate decelerations or heavy meconium-staining of the liquor occur.

METHODS

Criteria for considering studies for this review

Types of studies

All published, unpublished and ongoing randomised controlled trials comparing prophylactic amnioinfusion for oligohydramnios versus therapeutic amnioinfusion for fetal heart rate decelerations or meconium-stained liquor. We excluded quasi-randomised trials (for example, those randomised by date of birth or hospital number) from the analysis.

Types of participants

Women in labour with oligohydramnios but not fetal heart rate decelerations.

Types of interventions

Prophylactic amnioinfusion compared with selective (therapeutic) amnioinfusion only for women who develop fetal heart rate decelerations or meconium-stained liquor.

Types of outcome measures

Primary outcomes

1. Caesarean section rate.
2. Five minute Apgar score less than 7.

Secondary outcomes

3. Caesarean section rate for fetal distress.
4. Rate of instrumental deliveries.
4. Intrapartum fever.
5. Oxytocin augmentation.
6. Postpartum endometritis.
7. Cord arterial pH less than 7.2.
8. Meconium aspiration.
9. Neonatal pneumonia.
10. Neonatal sepsis.

Search methods for identification of studies

Electronic searches

We searched the Cochrane Pregnancy and Childbirth Group's Trials Register (28 February 2012).

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

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

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

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

We did not apply any language restrictions.

Data collection and analysis

We evaluated trials under consideration for methodological quality and appropriateness for inclusion according to the prestated selection criteria, without consideration of their results. We included individual outcome data in the analysis if they met the prestated criteria in [Types of outcome measures](#). We processed included trial data as described in [Clarke 2000](#).

We extracted data from the sources and entered them onto the Review Manager computer software (RevMan 2008), checked them for accuracy, and analysed them as above using RevMan 2008. For dichotomous data, we calculated risk ratios and 95% confidence intervals and, in the absence of heterogeneity, results were pooled using a fixed-effects model. We pooled continuous data using mean differences and 95% confidence intervals.

Selection of studies

Two review authors independently assessed for inclusion all the potential studies we identified as a result of the search strategy. We resolved any disagreement through discussion or, if required, consulted a third author.

Data extraction and management

We designed a form to extract data. For eligible studies, two review authors will extract the data using the agreed form. We resolved discrepancies through discussion or, if required, we will consult a third author. Data were entered into Review Manager software (RevMan 2008) and checked for accuracy.

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

Assessment of risk of bias in included studies

Two review authors will independently assess risk of bias for each study using the criteria outlined in the *Cochrane Handbook for Systematic Reviews of Interventions* (Higgins 2008). Any disagreement will be resolved by discussion or by involving a third assessor.

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

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

We assessed the method as:

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

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

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

We assessed the methods as:

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

(3) Blinding (checking for possible performance bias)

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

We assessed the methods as:

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

(4) Incomplete outcome data (checking for possible attrition bias through withdrawals, dropouts, protocol deviations)

We described for each included study, and for each outcome or class of outcomes, the completeness of data including attrition and exclusions from the analysis. We stated whether attrition and exclusions were reported, the numbers included in the analysis at each stage (compared with the total randomised participants), reasons for attrition or exclusion where reported, and whether missing data were balanced across groups or were related to outcomes. Where sufficient information is reported, or can be supplied by the trial authors, we would re-include missing data in the analyses which we undertake. The cut-off point of 20% is used for missing data. We assessed methods as:

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

(5) Selective reporting bias

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

We assessed the methods as:

- low risk of bias (where it is clear that all of the study's pre-specified outcomes and all expected outcomes of interest to the review have been reported);
- high risk of bias (where not all the study's pre-specified outcomes have been reported; one or more reported primary outcomes were not pre-specified; outcomes of interest 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 sources of bias

We described for each included study any important concerns we have about other possible sources of bias (related to the study design, data-dependent process, extreme baseline disbalance).

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

- low risk of bias;
- high risk of bias;
- unclear whether there is risk of bias.

(7) Overall risk of bias

We made explicit judgements about whether studies are at high risk of bias, according to the criteria given in the Handbook (Higgins 2008). With reference to (1) to (6) above, we assessed the likely magnitude and direction of the bias and whether we consider it is likely to impact on the findings. In future updates we will explore the impact of the level of bias through undertaking sensitivity analyses - see [Sensitivity analysis](#).

Measures of treatment effect

Dichotomous data

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

Continuous data

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

Unit of analysis issues

Cluster-randomised trials

If identified, 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 [Gates 2005](#) using an estimate of the intra-cluster correlation co-efficient (ICC) derived from the trial (if possible), or from another source. If ICCs from other sources are used, 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 separate meta-analysis.

Crossover trials

If we identify any crossover trials on this topic, and such trials are deemed eligible for inclusion, they will be included in the analyses with parallel group trials, using methods described by [Elbourne 2002](#).

Multi-armed trials

When analysing multi-armed trials we would combine all relevant experimental intervention groups of the study into a single group and all relevant control intervention groups into a single control group. If one of the arms is considered irrelevant by the authors it will be excluded from the analysis.

Dealing with missing data

For included studies in future updates, 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. 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 would use the I^2 statistic to measure heterogeneity among the trials in each analysis. In subsequent updates, if we identify substantial heterogeneity (more than 50%), we will explore it by prespecified subgroup analysis.

Assessment of reporting biases

Where we suspect reporting bias (see 'Selective reporting bias' above), we would attempt to contact study authors asking them to provide missing outcome data. Where this is not possible, and the missing data are thought to introduce serious bias, we would explore the impact of including such studies in the overall assessment of results by a sensitivity analysis.

Data synthesis

We would carry out statistical analysis using the Review Manager software ([RevMan 2008](#)). We would use fixed-effect inverse variance meta-analysis for combining data where trials are examining the same intervention, and the trials' populations and methods are judged sufficiently similar. Where we suspect clinical or methodological heterogeneity between studies sufficient to suggest that treatment effects may differ between trials, we would use random-effects meta-analysis.

If substantial heterogeneity is identified in a fixed-effect meta-analysis, we would note this and repeat the analysis using a random-effects method.

Subgroup analysis and investigation of heterogeneity

We plan to carry out the following subgroup analyses:

1. Women in labour with oligohydramnios undergoing prophylactic and therapeutic amnioinfusion for fetal heart decelerations.
2. Women in labour with oligohydramnios undergoing prophylactic and therapeutic amnioinfusion for meconium-stained liquor.

The following outcomes will be used in subgroup analysis:

1. Caesarean section rate.
2. Five-minute Apgar score less than 7.

For fixed-effect meta-analyses, we would conduct planned subgroup analyses classifying whole trials by interaction tests as described by [Deeks 2001](#). For random-effects meta-analyses, we would assess differences between subgroups by inspection of the subgroups' confidence intervals; non-overlapping confidence intervals indicate a statistically significant difference in treatment effect between the subgroups.

Sensitivity analysis

In future updates we will perform sensitivity analyses for aspects of the review that might affect the results; for example, where

there is a risk of bias associated with the quality of some of the included trials; or to explore the effects of fixed- or random-effects analyses for outcomes with statistical heterogeneity; and to explore the effects of any assumptions made, such as the value of the ICC used for cluster-randomised trials.

Sensitivity analysis will be performed for the primary outcomes (caesarean section rate and five-minute Apgar score less than 7).

RESULTS

Description of studies

One randomised controlled trial of 116 women was included (Ogundipe 1994). Sealed opaque envelopes ordered by random number table were used to allocate 56 women with amniotic fluid index of 5cm or less to receive prophylactic amnioinfusion (600ml followed by 3ml per minute), and 60 to a therapeutic amnioinfusion group.

Risk of bias in included studies

The included study (Ogundipe 1994) appears methodologically sound, except that the blinding was not done. Analysis was by intention to treat. Of the prophylactic amnioinfusion group, six women progressed too rapidly to receive amnioinfusion. Thirteen women (22%) in the control group developed moderate to severe variable decelerations and received therapeutic amnioinfusion.

Effects of interventions

No significant differences in maternal or neonatal outcomes were found between prophylactic amnioinfusion and therapeutic amnioinfusion for fetal decelerations in women with oligohydramnios in labour based on one small randomized trial (Ogundipe 1994).

The caesarean section rate did not differ between the groups (one study, 116 patients, risk ratio (RR) 1.29, 95% confidence interval (CI) 0.60 to 2.74). The prophylactic amnioinfusion group had a significantly higher incidence of intrapartum fever (one study, 116 patients, RR 3.48, 95% CI 1.21 to 10.05). The incidence of postpartum endometritis did not differ between the groups (one

study, 116 women, RR 3.21, 95% CI 0.34 to 30.00). The rate of neonatal sepsis and neonatal pneumonia did not differ between the groups (one study, 116 patients, RR 0.36, 95% CI 0.01 to 8.58; RR 5.35, 95% CI 0.26 to 109.08), respectively) (Ogundipe 1994).

DISCUSSION

The results show no benefit for prophylactic amnioinfusion over a policy of withholding amnioinfusion unless fetal heart rate decelerations or thick meconium staining of the amniotic fluid occur. The finding of increased intrapartum pyrexia in the included study (Ogundipe 1994) is consistent with the finding of a trend to increased intrapartum pyrexia with amnioinfusion in one study (Strong 1990) of amnioinfusion for cord compression (Hofmeyr 1998). However, the pathogenesis of the pyrexia may be by a mechanism other than infection, as puerperal infection has been reduced with amnioinfusion in studies of amnioinfusion for cord compression or potential amnionitis (Hofmeyr 1998).

AUTHORS' CONCLUSIONS

Implications for practice

The findings of the one randomized trial reviewed do not support the use of amnioinfusion prophylactically for oligohydramnios alone, rather than therapeutically only when fetal heart rate decelerations (or thick meconium staining of the amniotic fluid) occur. Given that amnioinfusion is an invasive and uncomfortable procedure, and the uncertainty concerning the possibility of rare maternal complications (Hofmeyr 1996; Wegnelius 1996), prophylactic amnioinfusion for oligohydramnios alone does not appear to be justified in routine clinical practice.

Implications for research

Future randomized trials of amnioinfusion for intrapartum oligohydramnios should seek to identify specific categories of women who may benefit from amnioinfusion.

ACKNOWLEDGEMENTS

The Cochrane Pregnancy and Childbirth Group team for technical support.

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CHARACTERISTICS OF STUDIES

Characteristics of included studies [ordered by study ID]

Ogundipe 1994

Methods	Allocated by consecutive, sealed opaque envelopes previously randomised from a table of random numbers.
Participants	Inclusion criteria: singleton pregnancy; 36 weeks or more; oligohydramnios (4-quadrant amniotic fluid index 5cm or less). Exclusion criteria: variable or late decelerations before study enrolment; fetal anomalies; non-vertex presentation; vaginal bleeding; maternal fever; previous caesarean delivery; unwillingness to participate.
Interventions	Prophylactic amnioinfusion (with 600ml saline at 20ml per minute followed by 3ml per minute through the first stage of labour) compared with therapeutic amnioinfusion only for recurrent moderate or severe variable decelerations. Intrauterine pressure was checked hourly.
Outcomes	Oxytocin augmentation; intrapartum fever; caesarean delivery; caesarean section for fetal distress; 1 minute Apgar score < 4; 5 minute Apgar score < 5; meconium aspiration; neonatal pneumonia; neonatal sepsis; umbilical artery pH < 7.2.
Notes	Torrance, California, USA. In the prophylactic amnioinfusion group, 6 of the 56 women progressed too rapidly to receive amnioinfusion. In the control group, 13 of 60 women developed moderate or severe FHR decelerations and received therapeutic amnioinfusion. Women were analysed as originally randomized.

Risk of bias

Bias	Authors' judgement	Support for judgement
Random sequence generation (selection bias)	Low risk	Randomization from a table of random numbers.
Allocation concealment (selection bias)	Low risk	Sealed opaque envelopes.
Blinding (performance bias and detection bias) All outcomes	High risk	
Incomplete outcome data (attrition bias)	Low risk	Data were complete.

Prophylactic versus therapeutic amnioinfusion for oligohydramnios in labour (Review)

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Ogundipe 1994 (Continued)

All outcomes

Selective reporting (reporting bias)	Low risk
--------------------------------------	----------

FHR: fetal heart rate

Characteristics of excluded studies [ordered by study ID]

Study	Reason for exclusion
Cook 1993	1. Randomization is unclear. 2. Definition of oligohydramnios is unconventional (AFI < 5 cm) and different to other trials. 3. Published only in abstract form with very limited information of the trial details.

AFI: amniotic fluid index

DATA AND ANALYSES
Comparison 1. Prophylactic vs therapeutic amnioinfusion for intrapartum oligohydramnios

Outcome or subgroup title	No. of studies	No. of participants	Statistical method	Effect size
1 Caesarean for suspected fetal distress	1	116	Risk Ratio (M-H, Fixed, 95% CI)	0.71 [0.21, 2.40]
2 Caesarean, overall	1	116	Risk Ratio (M-H, Fixed, 95% CI)	1.29 [0.60, 2.74]
3 Cord arterial pH <7.2	1	116	Risk Ratio (M-H, Fixed, 95% CI)	0.86 [0.24, 3.03]
4 Intrapartum fever	1	116	Risk Ratio (M-H, Fixed, 95% CI)	3.48 [1.21, 10.05]
5 Oxytocin augmentation	1	116	Risk Ratio (M-H, Fixed, 95% CI)	1.39 [0.88, 2.20]
6 1 minute Apgar score <4	1	116	Risk Ratio (M-H, Fixed, 95% CI)	1.07 [0.07, 16.72]
7 5 minute Apgar score <5	1	116	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
8 Meconium aspiration	1	116	Risk Ratio (M-H, Fixed, 95% CI)	0.0 [0.0, 0.0]
9 Neonatal pneumonia	1	116	Risk Ratio (M-H, Fixed, 95% CI)	5.35 [0.26, 109.08]
10 Neonatal sepsis	1	116	Risk Ratio (M-H, Fixed, 95% CI)	0.36 [0.01, 8.58]
11 Postpartum endometritis	1	116	Risk Ratio (M-H, Fixed, 95% CI)	3.21 [0.34, 30.00]

Analysis 1.1. Comparison 1 Prophylactic vs therapeutic amnioinfusion for intrapartum oligohydramnios, Outcome 1 Caesarean for suspected fetal distress.

Analysis 1.2. Comparison 1 Prophylactic vs therapeutic amnioinfusion for intrapartum oligohydramnios, Outcome 2 Caesarean, overall.

Analysis 1.3. Comparison 1 Prophylactic vs therapeutic amnioinfusion for intrapartum oligohydramnios, Outcome 3 Cord arterial pH <7.2.

Analysis 1.4. Comparison 1 Prophylactic vs therapeutic amnioinfusion for intrapartum oligohydramnios, Outcome 4 Intrapartum fever.

Analysis 1.5. Comparison 1 Prophylactic vs therapeutic amnioinfusion for intrapartum oligohydramnios, Outcome 5 Oxytocin augmentation.

Analysis 1.6. Comparison 1 Prophylactic vs therapeutic amnioinfusion for intrapartum oligohydramnios, Outcome 6 1 minute Apgar score <4.

Analysis 1.7. Comparison 1 Prophylactic vs therapeutic amnioinfusion for intrapartum oligohydramnios, Outcome 7 5 minute Apgar score <5.

Analysis 1.8. Comparison 1 Prophylactic vs therapeutic amnioinfusion for intrapartum oligohydramnios, Outcome 8 Meconium aspiration.

Analysis 1.9. Comparison 1 Prophylactic vs therapeutic amnioinfusion for intrapartum oligohydramnios, Outcome 9 Neonatal pneumonia.

Analysis 1.10. Comparison 1 Prophylactic vs therapeutic amnioinfusion for intrapartum oligohydramnios, Outcome 10 Neonatal sepsis.

Analysis 1.11. Comparison 1 Prophylactic vs therapeutic amnioinfusion for intrapartum oligohydramnios, Outcome 11 Postpartum endometritis.

WHAT'S NEW

Date	Event	Description
23 July 2012	New search has been performed	Search updated in February 2012.
23 July 2012	New citation required but conclusions have not changed	Review updated. No new trials identified.

HISTORY

Protocol first published: Issue 1, 1995

Review first published: Issue 1, 1995

Date	Event	Description
2 July 2010	Amended	Contact details edited.
15 May 2010	New search has been performed	One previously included study has now been excluded (Cook 1993). Search updated. No new studies identified. Studies previously awaiting classification (Abdel-Aleem 2005 ; Amin 2003 ; Persson-Kjerstadius 1999 ; Puertas 2001 ; Rinehart 2000) were not included as they are more suitable for another review 'Amniocentesis for potential or suspected umbilical cord compression in labour' (Hofmeyr 1998). Two new authors joined the review team.
3 August 2009	Amended	Search updated. Five reports added to Studies awaiting classification (Abdel-Aleem 2005 ; Amin 2003 ; Persson-Kjerstadius 1999 ; Puertas 2001 ; Rinehart 2000).
12 November 2008	Amended	Converted to new review format.
30 October 2001	New search has been performed	Search updated. No new studies found.

CONTRIBUTIONS OF AUTHORS

Justus Hofmeyr prepared the original review and participated in the preparation of the updated version. Natalia Novikova and Justus Hofmeyr prepared the updated version of the review. George Essilfie-Appiah commented on the updated version.

DECLARATIONS OF INTEREST

None known.

SOURCES OF SUPPORT

Internal sources

- University of Witwatersrand, South Africa.

External sources

- South African Medical Research Council, South Africa.
- UNDP/UNFPA/WHO/World Bank (HRP), Switzerland.

INDEX TERMS

Medical Subject Headings (MeSH)

*Amnion; Fetal Distress [therapy]; Fetal Heart [*physiopathology]; Heart Rate, Fetal [*physiology]; Injections; Obstetric Labor Complications [prevention & control] [*therapy]; Oligohydramnios [prevention & control] [*therapy]; Randomized Controlled Trials as Topic

MeSH check words

Female; Humans; Pregnancy