Intravenous fluids for preventing dehydration and prolonged labour in nulliparous women (Protocol)

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Intravenous fluids for preventing dehydration and prolonged labour in nulliparous women

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ABSTRACT

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

- To assess the impact of early or routine intravenous fluid administration on the duration and course of labour.
- To determine the risks and benefits of early or routine intravenous fluid administration during labour.
- To determine the risks and benefits of different fluid regimens (ie. different volumes or different types of fluids and or both) used in labour.

A subgroup analysis will also be performed on the following studies:

- Compare the course of labour when intravenous fluids are compared to oral intake (food or water or both);
- Compare the course of labour when oral intake is supplemented by routine intravenous fluids versus oral intake only.
BACKGROUND

Description of the condition

The aim of this review is to evaluate the impact of routine administration of intravenous fluids to nulliparous labouring women, in particular, to assess whether there is any reduction in the duration of labour and the prevention of dehydration.

Many factors affect the course of labour and over the years several studies have been conducted to improve our understanding of the progression of normal labour. There has also been an increased effort to prevent rather than treat abnormal labour and to identify factors that may lead to a reduction in operative deliveries (Handa 1993).

Human labour may be considered as a period of prolonged exercise. The labouring, exercising uterus and ultimately prevent prolonged labours (Salin 1998).

During exercise, fluid loss is poorly regulated and moreover, the rate of fluid loss by sweating and respiration is not reduced in a state of dehydration (Garite 2000). More specifically, uterine blood flow is not autoregulated and in the presence of decreased intravascular volume (which may occur secondary to dehydration), fluid is redistributed away from the uterus aggravating the problem (Eslamian 2006). Optimal uterine perfusion is not only required for adequate fetal oxygenation but also for the delivery of nutrients and the elimination of waste products from the contracting myometrium. Furthermore, the physical stress of labour may also lead to accelerated requirements of glucose metabolism. When glycogen stores are depleted there is a risk of ketosis (accumulation of ketone bodies as a result of starvation or prolonged exercise) (Foulkes 1985). An association between ketosis and longer labours has been described (Foulkes 1985). Dehydrated labouring women also have alterations in the acid-base balance of the fluid surrounding the myometrial fibres resulting in a decrease in the pH. Changes in the pH have been shown to affect calcium signalling and force of myometrial contractility prolonging the course of labour (Pierce 2003).

Prevention of dehydration in labour may therefore not only alleviate symptoms of thirst, but may also provide better hydration for the labouring, exercising uterus and ultimately prevent prolonged labour and avoid more intrusive interventions.

Description of the intervention

Although oral fluids are the simplest and easiest way of replenishing lost fluids, it is thought that amounts of more than 500 mls/hour are required in prolonged exertion (Noakes 1993) such as labour. Thus there may be some justification for the routine use of intravenous fluids for labouring women, however the volumes and types of fluids needs further clarification. The main aim of this review is to determine the impact (if any) on the use of routine intravenous fluid administration on prevention of dehydration and/or the duration of labour. It has also been suggested that adequate intravenous fluid replacement regimes may even obviate the need for caesarean sections that are performed solely for “failure to progress” in labour or uterine dystocia (Eslamian 2006) as a result of dehydration. That the administration of routine intravenous fluids may even prevent operative deliveries by preventing dehydration is largely theoretical as operative deliveries may result from various factors. However, even if there is a partial benefit of intravenous fluids in the reduction of unnecessary caesarean section then it is important to evaluate this as there are wider cost implications as well as the impact on future pregnancies.

While intravenous fluids may seem a simple and benign intervention to health care professionals, it may adversely affect the perception of labour for women and negatively impact on the normality of labour. We will therefore also evaluate the disadvantages of routine intravenous fluids which include women being less mobile in labour, maternal discomfort, fear and pain and overall negative feelings.

This review will not examine situations where intravenous fluids are medically indicated for example as preload prior to epidural analgesia, or for any other medical interventions such as cardio-respiratory conditions, diabetes, haematological or renal disease. Neither will this review consider the use of intravenous fluids for women with a febrile illness or pyrexia during labour.

How the intervention might work

Many institutions have adopted the policy of restricting women from food and drink in labour as a protective measure against aspiration of gastric contents in the event of general anaesthesia. This policy has led to the adoption of routine intravenous fluids to women in labour in some institutions. However, whilst some hospitals and institutions have adopted this policy, it is certainly not universal. Many midwifery-led units in the UK at least, do not routinely administer intravenous fluids for low-risk women and neither is it a common practice in home births and some birth centres (Michael 1991). A survey that was conducted in around 350 units in England and Wales revealed that a third allowed some food or drink and well over ninety percent allowed some oral intake (Michael 1991). Whether or not restricting oral intake is justifiable is the subject of another review (Singata 2002). However, we aim to include trials that compare oral intake versus intravenous fluids.
**Why it is important to do this review**

Different types of intravenous fluid regimens may produce different results for women in labour. The type and volume of intravenous fluid that is administered during labour needs to be elucidated. Moreover, this review is important as it may assist in providing evidence that routine intravenous fluid administration is not superior to oral intake in low-risk women. The routine use of intravenous fluids in labour has not been properly evaluated. As well as beneficial effects of fluid administration, there may be adverse effects on both maternal and fetal health. Excessive amounts of fluids may lead to fluid overload (Montain 2008). While high-dose glucose solutions may correct maternal ketosis, the administration thereof may be associated with an increased incidence of neonatal hyponatraemia (Tarnow-Mordi 1981). Dextrose only solutions also cause hyponatraemia and affect serum osmolality (Keppler 1988).

**OBJECTIVES**

- To assess the impact of early or routine intravenous fluid administration on the duration and course of labour.
- To determine the risks and benefits of early or routine intravenous fluid administration during labour.
- To determine the risks and benefits of different fluid regimens (ie. different volumes or different types of fluids and or both) used in labour.

A subgroup analysis will also be performed on the following studies:

- Compare the course of labour when intravenous fluids are compared to oral intake (food or water or both);
- Compare the course of labour when oral intake is supplemented by routine intravenous fluids versus oral intake only.

**METHODS**

**Criteria for considering studies for this review**

**Types of studies**

Randomised and quasi-randomised studies comparing the administration of routine intravenous fluids compared to non-administration of intravenous fluids.

Randomised and quasi-randomised studies comparing the administration of different volumes of the same intravenous fluids.

Randomised and quasi-randomised studies comparing the administration of different intravenous fluids.

Since this review aims to determine whether the routine use of intravenous fluids shortens the duration of labour, minimises maternal dehydration or minimises the need for augmentation of labour, women who subsequently require oxytocin augmentation will not be excluded. However, women who require oxytocin primarily for induction of labour will be excluded. Therefore we will include caesarean sections, other operative deliveries and the need for augmentation and the need to treat dehydration as outcome measures.

**Types of participants**

- Nulliparous women
- Spontaneous active labour
- Singleton presentations
- Cephalic presentation
- Term pregnancies (> 36 weeks)
- Low-risk pregnancies ie. no medical conditions such as diabetes, pre-eclampsia etc and no obstetric problems such as antepartum haemorrhage or chorioamnionitis

**Types of interventions**

- Early or routine administration of intravenous fluids at the onset of active labour (at least 3 centimetres dilated)
- Type of intravenous fluid administered
- Volume of intravenous fluid

**Types of outcome measures**

The outcomes will be divided into maternal and fetal outcomes.

**Primary outcomes**

**Maternal**

1. Duration of labour
2. Dehydration
3. Fluid overload
4. Maternal comfort
5. Maternal satisfaction
6. Subjective feelings of thirst and exhaustion
7. Objective measurements of dehydration (urine specific gravity, serum or urine osmolality)
8. Objective measurements of ketosis

**Fetal**

1. Fetal distress requiring caesarean section only as a result of prolonged labour not responsive to augmentation with syntocinon/oxytocin and no other identifiable obstetric reason for caesarean section
2. Cord pH < 7.20

**Secondary outcomes**

**Maternal**

1. Augmentation of labour
2. Caesarean section
3. Assisted delivery
4. Ketoacidosis

Fetal and neonatal
1. Hyponatraemia
2. Admission to NICU
3. Hyperbilirubinaemia
4. Cord gases
5. Early newborn weight loss

Search methods for identification of studies

Electronic searches
We will contact the Trials Search Co-ordinator to search the Cochrane Pregnancy and Childbirth Group’s Trials Register. The Cochrane Pregnancy and Childbirth Group’s Trials Register is maintained by the Trials Search Co-ordinator and contains trials identified from:
1. quarterly searches of the Cochrane Central Register of Controlled Trials (CENTRAL);
2. weekly searches of MEDLINE;
3. handsearches of 30 journals and the proceedings of major conferences;
4. weekly current awareness alerts for a further 44 journals plus monthly BioMed Central email alerts.
Details of the search strategies for CENTRAL and MEDLINE, 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 will not apply any language restrictions.

Data collection and analysis

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

Data extraction and management
We will design a form to extract data. For eligible studies, both 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. Data will be entered into Review Manager software (RevMan 2008) and checked 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
Both 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 person.

(1) Sequence generation (checking for possible selection bias)
We will describe for each included study the methods used to generate the allocation sequence in sufficient detail to allow an assessment of whether it should produce comparable groups. We will assess the methods as:
• adequate (any truly random process e.g. random number table; computer random number generator);
• inadequate (any non random process e.g. odd or even date of birth; hospital or clinic record number);
• unclear.

(2) Allocation concealment (checking for possible selection bias)
We will describe 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 will assess the methods as:
• adequate (e.g. telephone or central randomisation; consecutively numbered sealed opaque envelopes);
• inadequate (open random allocation; unsealed or non-opaque envelopes, alternation; date of birth);
• unclear.

(3) Blinding (checking for possible performance bias)
We will describe for each included study all the methods 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 will also provide information on whether the intended blinding was effective. Where blinding is not possible, we will assess whether the lack of blinding was likely to have introduced bias. Blinding will be assessed separately for different outcomes or classes of outcomes. We will assess the methods as:
• adequate, inadequate or unclear for participants;
• adequate, inadequate or unclear for personnel;
• adequate, inadequate or unclear for outcome assessors.

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

(5) Selective reporting bias
We will describe for each included study how the possibility of selective outcome reporting bias was examined by us and what we found.
We will assess the methods as:

- adequate (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);
- inadequate (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.

(6) Other sources of bias
We will describe for each included study any important concerns we have about other possible sources of bias eg. was there a potential source of bias related to the specific study design? Was the trial stopped early due to some data-dependent process? Was there extreme baseline imbalance? Has the study been claimed to be fraudulent?
We will assess whether each study was free of other problems that could put it at risk of bias:

- yes;
- no;
- unclear.

(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 2008). With reference to (1) to (6) above, we will assess the likely magnitude and direction of the bias and whether we consider it is likely to impact on the findings. We will explore the impact of the level of bias through undertaking sensitivity analyses - see Sensitivity analysis.

**Measures of treatment effect**

**Dichotomous data**
For dichotomous data, we will present results as summary risk ratios 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. Their sample sizes will be adjusted using the methods described in Gates 2005 using an estimate of the intracluster correlation co-efficient (ICC) derived from the trial (if possible), or from another source. If ICCs from other sources are used, this will be reported and sensitivity analyses conducted 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.

**Dealing with missing data**
For included studies, levels of attrition will be noted. The impact of including studies with high levels of missing data in the overall assessment of treatment effect will be explored by using sensitivity analysis.

For all outcomes analyses will be carried out, 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 will use the I² statistic to measure heterogeneity among the trials in each analysis. If we identify substantial heterogeneity (>50%) we will explore it by prespecified subgroup analysis.

**Assessment of reporting biases**
Where we suspect reporting bias (see selective reporting bias above), we will 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, the impact of including such studies in the overall assessment of results will be explored by a sensitivity analysis.

**Data synthesis**
We will carry out statistical analysis using the Review Manager software (RevMan 2008). We will 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 there is clinical or methodological heterogeneity between studies sufficient to suggest that treat-
ment effects may differ between trials we will use random-effects meta-analysis. If substantial heterogeneity is identified in a fixed-effect meta-analysis this will be noted and the analysis repeated using a random-effects method, as a sensitivity analysis.

Subgroup analysis and investigation of heterogeneity
We plan to carry out the following subgroup analyses:

- intravenous fluids versus oral intake (food or water or both) on the course of labour;
- compare routine intravenous fluids supplemented by oral intake versus oral intake only on the course of labour.

The following outcomes will be used in subgroup analysis.

Maternal
1. Duration of labour in hours
2. Dehydration

Fetal
1. Fetal distress as a result of prolonged labour
2. Cord pH < 7.20

For fixed-effect meta-analyses we will conduct planned subgroup analyses classifying whole trials by interaction tests as described by Deeks 2001. For random-effects meta-analyses we will 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
A sensitivity analysis will be performed if substantial heterogeneity is noted between the studies in terms of the volumes of intravenous fluids that are administered in different studies as the volumes are not predetermined.

ACKNOWLEDGEMENTS
As part of the pre-publication editorial process, this protocol has been commented on by two peers (an editor and referee who is external to the editorial team), a member of the Pregnancy and Childbirth Group’s international panel of consumers and the Group’s Statistical Advisor.

REFERENCES

Additional references

Barr 1991

Deeks 2001

Eslamian 2006

Foulkes 1985

Garite 2000

Gates 2005

Handa 1993

Higgins 2008

Keppler 1988

Maughan 1996

Michael 1991

Montain 1992
Montain 2008

Noakes 1993

Pierce 2003

RevMan 2008

Saltin 1998

Singata 2002

Tarnow-Mordi 1981

* Indicates the major publication for the study

HISTORY
Protocol first published: Issue 2, 2009

CONTRIBUTIONS OF AUTHORS
Feroza Dawood created the first draft of the protocol and Siobhan Quenby commented on the draft and suggested changes.

DECLARATIONS OF INTEREST
None known.