Reviewer’s report

Title: Anti-fibrinolytic agents in bleeding during pregnancy, delivery and puerperium: A systematic review

Version: 2 Date: 11 May 2009

Reviewer: Lelia Duley

Reviewer’s report:

The authors have dealt with some of my comments. However, this paper still largely reads as a review of an intervention for prevention or treatment of postpartum haemorrhage. My view is that it makes more sense from a clinical perspective to deal with these questions separately, and this review should therefore be restricted to PPH.

If the authors want to keep the topic of the review so broad, they should deal with this throughout the review. Currently antepartum haemorrhage is mentioned in the background, but is not taken account of in the methods of the review, nor in discussion. If use for antepartum haemorrhage is included there should be a much wider range of outcomes – including mode of delivery and outcome for the baby. The abstract conclusion ‘Tranexamic acid may reduce blood loss in obstetric haemorrhage’ is not supported by the data – as there are only data on PPH.

To say the review covers antepartum haemorrhage, and then make no reference to this use in discussion and implications for practice and research is potentially misleading. It might imply to some that this is an accepted indication – and I don’t think that is the case.

Whilst I accept that the use of uterotonics is in Table 3, my earlier comment was that this should be much clearer throughout the text – from objectives thro to conclusions. It is hugely important clinically, and should not be confined solely to a column in the tables. This is another example of why it is simpler to restrict the review to PPH – as this comment only applies to prevention of PPH.

The issue of cost and availability has not been dealt with. The three trials included used IV drug for prevention of PPH. Before recommending further trials of prophylaxis, it would seem sensible to discuss the cost and acceptability (to women as well as health services) of this intervention – particularly in low and middle income countries.

Outcomes not reported by the trials is not a reason to exclude them from the methods. Methods should list what outcomes were sought, and results what data were found.

I understand that the reviewers have to use the published data – but they should discuss the limitations, which is that the methods used in meta analysis of
skewed data may be unreliable.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

'I declare that I have no competing interests'