Reviewer's report

Title: Anal incontinence, urinary incontinence and sexual problems in primiparous women - what is the role of obstetric anal sphincter injuries vs. episiotomy?

Version: 2  Date: 7 August 2014

Reviewer: Amanda Ampt

Reviewer's report:

Major Compulsory Revisions

1. Title - The title is misleading as it implies a comparison of episiotomies with OASIs - this study compares women with episiotomies only and those who have both episiotomies and OASIs.

2. Is the question posed original, important and well defined? - The aims of this study as written in last paragraph of the Background:
Primary aim - assess if there was a difference in prevalence of AI, UI and sexual problems in women with episiotomy and OASIS compared to women with episiotomy only.
Second aim - assess if episiotomy characteristics were associated with AI, UI and sexual problems.
These are clear, however confusion arises on p5 lines 116-118 where another aim is mentioned regarding assessing associations between episiotomy characteristics and OASI - authors then report this has been published. This section needs to be moved to the background. This paper does not investigate this aspect, and it is confusing to have it in the methods section. Ensuing sections of the paper refer to such association, and it would be clearer if the authors briefly reported these findings at the outset in the background.

Are the data sound and well controlled?
Are the methods appropriate and well described, and are sufficient details provided to allow others to evaluate and/or replicate the work?

3. The authors refer to this study as a retrospective cohort, however it is actually a matched cohort.

4. Although the authors report the definition and collection of certain variables in quite a bit of detail, more detail is required for selection of women into the study. The authors do not report the number of women with OASI and episiotomy who were identified through birth logs, nor those with episiotomy alone. The decision to match on instrumental delivery has obviously influenced selection of participants, however there is no detail regarding why such matching was considered necessary and needs to be justified. Once women had been identified as potential participants, there needs to be reporting of how women
were then recruited into the study, with reporting of participation rates. Without more detail, there is major concern regarding selection bias. A major confounder which has not been discussed in the methods or results sections is parity, although it is mentioned in the paper title, title of Table 5 and in the conclusion implying that the cohort did consist of primiparous women.

Results

5. Table 1 – Demographics. The authors presented conditional logistic regression results, but I am unsure why they then did not actually report the ORs and confidence intervals rather than just the p values. I assume these results are univariate associations although this is not stated. Was regression undertaken because the variables were continuous – otherwise McNemar’s test may have been more appropriate. Was the assumption of log linearity tested?

Table 2:

6. There needs to be detail of whether any adjustment was undertaken in these models.

7. Has the assumption of log linearity been assessed? It is not reported, and from the distribution of these variables when categorised, it looks questionable. I would consider whether reporting of per unit increase is actually a clinically relevant score, as categorisation results are more easily interpreted.

Although the odds ratios are presented in the table, it is of note that they are not discussed in the text. It may be less confusing for the reader to undertake this analysis using McNemar test (assuming no adjustment was undertaken).

8. The sexual responses are generated from likert scales - and thus the data is ordinal, and analysis should be restricted to categorical approaches (not means). I am also concerned about the validity of creating an overall sexual problem score and scoring system for frequency of complaints by adding up likert scales. Has this approach been previously validated for this questionnaire?

Table 3 & Table 4

9. Were the distributions of the episiotomy characteristics normally distributed? From the apparent distributions in Table 3, it would appear they were skewed – as such medians (not means) should be presented in Table 3. I would however debate the necessity of Table 3 as length and depth can be reported in the text, and information regarding incision point and angle is also presented in table 4 – the median values for these values could be added to this table.

10. Table 5 The methods section states that Spearman coefficient was used to estimate correlation between episiotomy characteristics and AI, UI and sexual problems in the two groups separately, however Table 5 does not report the separate results, nor are they mentioned in the text.

Is the interpretation (discussion and conclusion) well balanced and supported by the data?

11. The authors refer to episiotomies without associated OASIs as ‘protective’. I
feel they would be better described just as episiotomies without OASIs - the use of the word protective becomes confused with characteristics of the episiotomy that the authors have stated as protective features of an episiotomy such as length and depth (p10 line 216). The results state that there was a moderate correlation for sexual problems with episiotomy length and depth - however this is contradicted in the discussion with the statement "there were no associations...." (p11 lines 242-244). I find this aspect poorly described and confusing, and distracting from the main message as I was often unsure if the authors were referring to protective features of an episiotomy or not.

12. Statements such as "thus it is important to prevent OASIS" (p12 line 279) are unnecessary as this is self evident. If the authors are truly arguing that an appropriately performed episiotomy may help to prevent OASI and that this study highlights that the sequelae of episiotomy are not as bad as for OASI then they should state it as such.

Minor Essential Revisions
1. Table 5 - Needs notation that this table is reporting Spearmans coefficient.
2. I find the discussion around "association between episiotomy characteristics with dysfunctions" (p9 line 201) confusing, as Table 4 which is mentioned immediately after this statement does not reflect this but just reports the range.
3. p4 line 79 Refs 1-3 do not actually report anal incontinence of 30%-50% as stated in the first paragraph of the paper (eg Roos reports 20%, de Leeuw reports 31% with fecal incontinence and 10% with fecal soiling – not necessarily mutually exclusive, and Marsh reports 4% fecal incontinence with 24% poor-variable control of flatus)

Discretionary revisions
1. It would be informative to have more information regarding ‘birth logs’ – for example, are they the complete clinical record or an administrative dataset?
2. p4 line 90 "and continue" should be "and continues"
3. p4 lines 96 when highlighting that mediolateral episiotomies are rarely performed properly, it would add more meaning if you stated what the research has found that clinicians do that is improper
4. p5 lines 121-122 "women with OASIS that had episiotomy" should be written as "women ...who had"
5. p6 line 144 suggest "assessed" instead of "registered"
6. p8 line 181 add the word OASIS "women with comapred..." should be "women with OASIS compared..."
7. p8 line 182 Table 2 reports 22% not 21%, and 16% not 14%

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

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:
I declare that I have no competing interests