Reviewer's report

Title: Finger joint laxity and number of previous pregnancies as antenatal markers of pregnancy induced back pain. A cohort study.

Version: 1 Date: 5 July 2013

Reviewer: Genevieve Dumas

Reviewer's report:

Review of paper entitled: Finger joint laxity and number of previous pregnancies as antenatal markers of pregnancy induced back pain. A cohort study.

Answer to specific questions

1. Is the question posed by the authors well defined?
   Yes, the research question is well defined

2. Are the methods appropriate and well described?
   The methods are generally appropriate and well defined, though a few points need to be clarified (see comments)

3. Are the data sound?
   The data look sound though it looks like the data have been collected a long time ago (1991) but probably not analysed until recently.

4. Does the manuscript adhere to the relevant standards for reporting and data deposition?
   I am not sure what the specific standards referred to are.

5. Are the discussion and conclusions well balanced and adequately supported by the data?
   Yes, generally, but the discussion could be a little more in depth and complete (see comments). The authors also seem to contradict themselves in the Conclusion compared to the Results about the independence of laxity and number or previous pregnancies (see below).

6. Are limitations of the work clearly stated?
   A few more limitations could be presented.

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?
   Yes they do.

8. Do the title and abstract accurately convey what has been found?
   Yes, reasonably well
9. Is the writing acceptable?
Yes, the manuscript is nicely written

Comments
This is a solid study with a good size sample and. A few points need to be clarified and there are very few typographic errors.

The definition of the two groups should be stated early in methods. It is not clearly stated until Table 3 and it is confusing for the reader that the intensity of pain is compared between a group of women with back pain and a group without pain (pain intensity assumed to be zero).

In Table 2, it may be preferable to present data only from women with a complete data set. Otherwise, the data may be misleading because of the wide range of weeks in each period. With a good size sample it may not make a difference, but it should be verified. A second potential misleading factor is the widespread post partum period (recovery might be expected to be accomplished in week 29, but not necessarily in week 4). Mean(SD) may be an alternate or additional way to represent the periods. These points should be mentioned in the limitations of the study too.

The authors state that AA and number of previous pregnancies are significantly and independently associated with the incidence of back pain. Please clarify how independence between these two factors was established as Ostgaard suggested they were related. The first sentence of the Conclusion contradicts this statement about the two factors been independent.

Though not absolutely necessary, it would be nice to see a picture of the set-up. The small number of subjects in the “back pain group” is also a limitation of the study.

Typographical and other minor errors
Under : Medical History, paragraph 2, last line: “lowest back pain” is ambiguous. Is it the lowest pain and pain in the low back?

Under Statistical analyses, lines 2 and 3: please be more explicit about what was tested with t-tests and Spearman correlation. What are the “inter-individual” tests? Between the 2 groups? and were they Spearman correlations as the order in the sentence suggests?

In Results, second page: the end of the top paragraph is unclear. Please clarify in what the pattern was similar in what way it was not similar. The sentence is confusing.

In Discussion, line 2: “was” should be “were”.

There is a typographical error in the title of Reference 12.

Units are missing in Table 2.