Author's response to reviews

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

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Version: 2 Date: 13 October 2013

Author's response to reviews:

October 13, 2013
Editor in Chief
BMC pregnancy and childbirth

Dear Editor in Chief,

Re: MS: 1492894998100703.

Please find a revised edition of our manuscript with the changed title “Finger joint laxity, number of previous pregnancies and pregnancy induced back pain. A cohort study”. In one manuscript version the changes are made clear by showing changed and added text highlighted in yellow. A second manuscript is our final version without highlights.

Please find our replies to the comments of the reviewers in red text together with the respective comment.

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:

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 Do the authors clearly acknowledge any work upon which they are building, both published and unpublished?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

   The definition of back pain and no back pain groups are now stated in the second paragraph under the subheading “Medical history” on page 6.

   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.

   In the calculation of statistical difference of the changes of the finger angle the paired t-test was used. In that test only participants with actual values at the different time points was included in the test. This explains the difference to the mean values of the whole cohort at each time point.
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 wide dispersion of post partum weeks is added as a limitation in the discussion, in paragraph 3, page 14 with the wording: “Also, the wide dispersion of the time since delivery might have distorted the data, since recovery might be expected 29 weeks after delivery but not necessarily after 4 weeks.” However, we believe that it is more adequate to present the fact of this wide range rather than partly conceal it with mean and standard deviations.

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.

In the present study, all variables measured in early pregnancy were included in the multiple regression analysis and therefore competing about the association to the dependent variable pregnancy-induced back pain. Since both AA and number of previous pregnancies remained statistically significantly associated to back pain, as compared to the simple regression, the conclusion is that they independently, irrespective of each other, were related to back pain. This also means that either association do not go through any other measured variable.

Though not absolutely necessary, it would be nice to see a picture of the set-up. A drawing of the angle measurement device is shown as Figure 1. Referred to under “Clinical examination” page 7.

The small number of subjects in the “back pain group” is also a limitation of the study.

In discussion on page 4, paragraph 3 this is clarified and discussed as: “In addition, the number of women with persistent back pain after delivery was small, which probably reduced the sensitivity of the study to show only the strongest associations, and suggests that the chance of a false positive in detecting these associations is small.”

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?

On page 6, paragraph 3 the text is changed to: “the lowest back pain location”.

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?

I hope this is clarified by the changed wording in paragraph 2, on page 8 as:
“Spearman correlation was used to test association of inter-individual abduction angles at different time intervals. Differences between the dependent intra-individual abduction angles at different time intervals were tested with paired t-test.”

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 paragraph 1, on page 11, we hope the information is made clearer by changing the sentence to “Similar differences between women with and without back pain with onset during the present pregnancy were shown at the appointment in late pregnancy”.

In Discussion, line 2: “was” should be “were”.
“Was” is changed to “were”.

There is a typographical error in the title of Reference 12. The reference (now number 18) is changed accordingly.

Units are missing in Table 2. The table head is changed to: “Abduction angle of left fourth finger (°), throughout pregnancy and post-partum of all women included in the study.”

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: 4 July 2013
Reviewer: Nina Vollestad

Reviewer's report:
This study aims at examining the association between general hypermobility and postpartum lumbopelvic pain by exploring data obtained from a previous cohort study. The present study has in principle a strong design for identifying predictors, yet there are several short-comings that reduce its value.

Major Compulsory Revisions
1. Apart from a few relatively recent Scandinavian studies, the authors seem to lean on literature and knowledge available at the time when the data were collected (first half of the 1990-ies). Over the last decade quite an extensive number of studies have provided knowledge of relevance for the questions addressed in the present paper. The authors need to include these in their discussion, even though the present study have not included some of the more recently identified predictors or risk factors for lumbopelvic pain.

References are added to the Introduction:
regarding mobility of the symphysis: Garras et al.,

2. The main focus of this paper is on hypermobility (or laxity) which is measured by the change in angle of the forth finger with a given force applied to it. No reference for the method is given, nor any measurements properties. The force applied is given, but the authors also need to describe how far the instrument was moved. Otherwise, it is hard to understand to what extent this method actually picks up differences in laxity.

It is well known form other joints that this kind of measures usually has large measurement errors. Usually changes or differences within 10 degrees are assumed to be undetectable by these measures. Unless the authors provide data showing better results, we must assume the same uncertainty applies for their method. The authors also need to describe their rationale for the chosen method to assess laxity. How can you be sure that this method is a valid measure?

In the Methods section on page 7, paragraph 3, two references are added. In the first reference the same method was used and in the second reference a significant correlation between the abduction angle and Beighton score was shown.

A picture showing the device used for the abduction angle measurement is added to the manuscript as Figure 1, on page 7 paragraph 3. The figure shows the immobilized index finger, the abduction force on the fourth finger and that the instrument was not moved during the measurement of the angle.

Regarding the rationale for the chosen method we refer to the discussion on page 14, first paragraph 1.

In discussion on page 14, paragraph 3, we have added to the limitations section: “There was no information about the size of error in the reading of the finger angle….”.

Furthermore, the uncertainty of the individual measurement to predict back pain post partum is shown by the low sensitivity of 0.42 and low specificity of 0.63, presented on page 12, paragraph 2.

3. The authors claim that their measurement of laxity reflects a general hypermobility, i.e. that the inter-individual differences capture general and non-pregnant properties. Yet, their first measure is obtained after 6-19 weeks of pregnancy. Knowing that for instance relaxin increases in early phases of pregnancy, and recent studies have provided support to the impact of relaxin on laxity, it is difficult to use a measure of mobility in early pregnancy as an indicator of a general hypermobility. It seems hard to distinguish between changes induced in early pregnancy from the general state. The authors need to discuss
these aspects and not ignore them.
In the discussion on page 13, paragraph 1 the “effect” is changed to “association” to increase the uncertainty in this measurement.

To clarify that a possible reflexion of the finger angle to general joint laxity is restricted to pregnancy and post partum state we have added “in pregnancy and post partum” to discussion on page 13, paragraph 1 and paragraph 3.

4. The outcome measure is the presence of back pain vs no back pain. Altogether only 16 women reported back pain 13 weeks postpartum, resulting in some uncertainty of the calculated estimates. The authors need to consider this in their interpretation. Other possible outcomes were also included (e.g. Disability rating index (DRI) and pain intensity) and it is not obvious that the chosen one is the best. The DRI score is quite low for the back pain group and the SD values quite large, indicating that quite a few of those without back pain have a high DRI, and vice versa. A similar pattern is seen for pain intensity. The authors should therefore also include analyses using DRI and pain intensity as outcomes to further substantiate their results.

The limitation because of the low number of women with back pain post partum is further discussed on page 14 and paragraph 3. This is taken into account in the interpretation of the results that now is generally changed to “association” rather than effect.

We hypothesized that peripheral joint laxity measured in early pregnancy was associated with pregnancy induced back pain. This implies that the outcome measure is localization of pain and not function or pain intensity. However, the latter could be seen as possible confounders. When DRI or pain intensity in early pregnancy was included as dependent variables in each logistic regression analysis, the finger angle and number of pregnancies were statistically significant but not DRI or pain intensity.

5. It is hard to understand the study has sufficient power to allow the analysis behind Fig. 1. It probably needs inclusion of interaction effects in the model, and the number is probably too low for this.

Because of the limited number of participants in the study the logistic regression model was used to compute expected mean incidence estimates of back pain post partum, based on the left fourth finger in early pregnancy and the number of previous pregnancies.

Minor Essential Revisions

6. The authors shift in their opinion of what kind of factor they examine hypermobility to be. In the title they use the term "marker", in the abstract they use "the effect of" or "factors important for", in the introduction they aim at identifying "a predictor" and in the conclusion they use "factors that favour the development". The authors need to decide whether they simply explore an association or whether they aim at determining the impact of a set value in predicting back pain in relation to pregnancy.
The title is changed to “Finger joint laxity, number of previous pregnancies and pregnancy induced back pain. A cohort study.”

In the Abstract:
“...the effect of...” is changed to “... the possible association of peripheral joint mobility...”
“...are suggested to be factors important for” is changed to “...were associated with...”.
In the Introduction section “...could be used as a predictor...” is changed to “...was associated with...”.
In Conclusion in the discussion section at page 15, paragraph 1 “are suggested to be factors important for” is changed to “...were associated with...”.

7. The reference list is insufficient. Newer and internationally published papers should be used.
Eight new references are added to the manuscript.

Level of interest: An article of limited interest
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.

Yours sincerely,
Kristiansson, Per
Lindgren, Anne