Author's response to reviews

Title: Birth setting, transfer and maternal sense of control: results from the DELIVER study

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Author's response to reviews: see over
Dear Dr. Andrew Symon,

Please find enclosed our revised manuscript entitled “Birth setting, referral and maternal sense of control: results from the DELIVER study” which we submit for publication in BMC Pregnancy and Childbirth.

We thank the reviewers for their useful comments, and we have responded to them point by point. You can find this at the bottom of this letter. After the point by point comments of the referees we have stated RESPONSE after which our response is written down. If applicable, the responses refer to changes that we have made in the manuscript accordingly.

Main changes include that we have replaced figure 2 by table 3A and table 3B, because the figure was difficult to read. From the study population we excluded 10 more women, because their gestational age was ≥ 42 weeks, and they are not considered low-risk: this did not change the associations. Furthermore we have clarified the aim of the study more clearly in the introduction section and throughout the manuscript.

All authors have read the manuscript and approved submission; the manuscript has not been published and is not being considered for publication elsewhere, in whole or in part, in any language, except as an abstract.

We hope that you will find the revised manuscript of interest to the readers of BMC Pregnancy and Childbirth.

Yours sincerely,

Caroline Geerts

Response to the referee 1

Minor Essential Revisions

1. The two last sentences before the sensitivity analysis could be mentioned earlier, more specifically in the method section: groups of planned and actual place of birth or transfer.
RESPONSE: We have relocated the sentences to the method section “The group who…and 30 parous women).” (page 7, 5th paragraph of data analysis section).

2. Results section: last sentence before ‘planned place of birth and LAS score’: women in the home birth group were less anxious during pregnancy for giving birth.

RESPONSE: This indeed is a better sequence of words, I have changed it in the text: “Women in the….for giving birth”, page 8, 3rd paragraph of result section.

3. Check spelling of Chronhbach’s alpha --> Cronbach’s alpha?

RESPONSE: We have corrected the spelling, see result section page 8, 2nd paragraph, 1st line “The Cronbach’s alpha…for parous women.”

4. I miss a table with regression coefficients of the models.

RESPONSE: With regard to whether findings are clinically relevant, we prefer showing the differences.

Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

1. The background section is thin. There is little theory behind it, and no clear hypotheses are formulated.

RESPONSE: We have formulated a hypothesis as suggested by the reviewer. To clarify to the reader we added “The current thought is…during labour (RQ2).” in the final paragraph of the background section, page 3, 4.

2. In the background and/or discussion section I miss the comparison with LAS scores in other studies/countries/comparable settings.

RESPONSE: We have included the study of M. Nieuwenhuijze in the discussion section and we compared our LAS scores with their study (based on LAS-11), which was performed among Dutch women in midwife-led care as well: “The sense of control scores… not clinically relevant (< 5.5 points).”, discussion section, page 10, 2nd paragraph. We have included a qualitative study that reports experiences of women who were transferred (reference 39): discussion section, page 10, 3rd paragraph “Women hope for…this is disappointing.”

3. The general research question contains two sub questions. The manuscript would benefit from a distinction between the two (throughout the whole text): 1) Is the sense of control associated with place of birth? 2) is the sense of control associated with being transferred?

RESPONSE: The two research questions (RQ) for this manuscript that we aim to gain insight in are:
RQ 1. What is the association between planned place of birth, home or hospital in midwife-led care, with feelings of control during labour experienced by low risk women?
RQ 2. What is the difference in feelings of control for women who planned a home birth, but who were transferred during labour, with women who planned a hospital birth and who were transferred?
We have made the distinction between these two research questions more clear throughout the text. To clarify this throughout the manuscript, we have more clearly formulated the research questions in the introduction section: “We formulated two research questions….transferred during labour (RQ2).” on page 4. Furthermore we have separated 2 groups of women: for RQ1 of the study we were interested in the overall group of women who started labour in primary care and we compared LAS scores for these women according to planned place of birth (home or hospital in midwife-led care). The results are shown in table 2 ‘all women’ on page 17. For RQ2 we have replaced figure 2 and added table 3A, page 18: It shows the mean LAS score of women who were transferred during labour to obstetrician-led care, and who planned a home versus a hospital birth in midwife-led care.

4. In a next step hypotheses could be formulated for these two questions, with the necessary reference to previous empirical and theoretical literature.

RESPONSE: In accordance with this suggestion, we have formulated a hypotheses and we defined the two research questions at the end of the background section. “The current thought is that…to an obstetric-led unit during labour (RQ2).” in the last paragraph of the introduction section, page 3 and 4.

5. The manuscript is difficult to read because of the multiple comparisons made. I would suggest to make comparisons always very explicit ‘what is compared to what’ to make it easier for the reader to follow.

RESPONSE: According to your previous suggestion (3), we have made a more clear distinction between the two research questions throughout the manuscript:

- In the introduction, page 4: “We formulated two research questions…obstetrician-led unit during labour”. In the methods section, data analyses 2nd paragraph, page 7: “For the primary aim of this study (RQ1),” and 4th paragraph: “From information on place of birth… who were transferred (hosp-transfer)”.
- In the results section results are described for RQ1, page 8, 4th paragraph: “All women”, and for RQ2, page 8 and 9, 1st paragraph: “Women who were transferred…a home or hospital birth”. In the table 2 and 3A the comparisons made are clearly defined in the title with a reference to RQ1 or RQ2.
- We have replaced the figures and separated the two comparisons that we were interested in in table 2 (RQ1) and table 3A (RQ2). Additionally, we have added another comparison, table 3B ‘no transfer’. In this table, three groups of women who were not transferred, were compared according to their planned and actual place of birth. This gives us additional insight in the effect of birth setting on sense of control, when transfer of care was not necessary. This is explained in the data analyses section, page 7 5th paragraph “Furthermore, women who….and 30 parous women)” and result section, page 9, 2nd paragraph “Women who were not transferred…hospital under midwife-led care”.

6. The choice of the reference groups is not well explained. I think other choices would also have been meaningful. Why did the authors choose these reference groups in their analysis?

RESPONSE: Since the option for home birth is being questioned with regard to patient satisfaction (Chervenak et al), we used (planned) hospital birth as the reference group in all comparisons.

- To clarify this to the reader we added the following in the data analysis section on page 7, final paragraph: “Since the option for home birth…..as the reference group.”
7. What is the main message? Is it about the comparison between hospital and home births? Or is it about ‘with or without transfer’? Or both? Now these two ‘histories’ are mixed up. For example, in the abstract, and in the last sentences of the discussion section (p. 10-11) the comparison is limited to home and hospital births, while being transferred or not is not mentioned, although being transferred has a meaningful impact on feelings of control. In order to have a more balanced manuscript, both ‘histories’ should be presented and discussed in the same way (starting with the research questions and hypotheses). Now I get the impression that the impact of being transferred is downplayed a bit. In addition in the introduction the main argument is about transfers, while in results and discussion section it is turned into the home versus hospital comparison. More consistence in the narrative throughout the manuscript is needed.

RESPONSE: The main message of the study is that feelings of control are similar for women who plan a home versus women who plan a hospital birth (RQ1). This is the most important message because prior to labour women will make a choice about their planned place of birth but they do not yet know whether they will be transferred or not. Therefore, it is most important for them to know whether sense of control is different among the group of women who plan birth at home versus in hospital.

Among women who were transferred feelings of control were similar for women planning a home or hospital birth (RQ2). Thus, planned place of birth had no influence on feelings of control, even not in case of transfer. Nevertheless, feelings of control were lower among women who were transferred, but birth settings had nothing to do with that. Therefore, we recommend that a qualitative research should provide more insight into the negative experience of transfer. To clarify this and to bring more balance in the manuscript, we added the following:

- For more balance, we discussed findings of both the first and second research question throughout the discussion section. For this purpose, the following sentences were added to the discussion section, page 10, 4th paragraph “Among the women who were transferred….were found as well”. Page 11, 1st paragraph “Moreover we showed…hospital birth.” and the 3rd paragraph“…and that, when transfer is necessary…to this decline.”

- We analysed and discussed the fact that sense of control was lower among women who were transferred during labour compared to those who were not transferred. With regard to that, the following changes were made in the manuscript: in the results of the abstract, page 2: “Overall, women who were…than women who were not transferred” and in the conclusion: “Transfer of care during labour….who planned a home or hospital birth”. In the results section of the manuscript, page 9, 1st paragraph: “Overall, feelings of control…of labour 4.3; 3.1 - 5.6).” In the discussion section on page 11 the 2nd paragraph “In our study, feelings of control….no influence on this decline.” And the 4th paragraph: “in addition with regard…of these women”.

8. The idea of transfer should be well defined. Transfer from home to hospital is one thing, transfer from primary to secondary care is another. Being transferred to hospital, but still with a midwife attending the birth is different from being transferred to hospital and give birth under the supervision of an obstetrician. Is a transfer to a hospital always a transfer to secondary care? This is not clear.

RESPONSE: When the word ‘transfer’ is used we always mean transfer of care from midwife-led to obstetrician-led care. For physically moving to the hospital we used the word ‘transportation’. When a
women who planned a homebirth is transferred from midwife-led to obstetrician-led care, she needs to be transported to the hospital, while women who planned a hospital birth who need to have care transferred, usually remain in the same hospital room, or they need be transported by wheelchair or bed, to another room or another floor within the hospital.

To clarify this issue, we have added “Transfer of care for women….only the caregiver changes.” Page 5 of the method section in the paragraph concerning ‘Planned place of birth and transfer of care during labour’.

9. The group hospital-home has a LAS score comparable to the home-home group. This is remarkable, but the authors do not pay attention to it, nor try to explain this. Lack of control and dissatisfaction are associated with violated expectations. In the hospital-home group one would expect that expectations were not met, while feelings of control are high. This is interesting… and could perhaps mean that expectations were surpassed, resulting in feeling in control. It would be interesting to discuss this finding in the discussion section.

RESPONSE: Indeed this is an interesting finding, and we have addressed this issue in the discussion section, as suggested by the reviewer. Discussion section page 10, 3rd paragraph: “Women who planned a hospital birth….resulting in feeling in control.”

Response to referee 2

1. I know the word ‘delivery’ is still commonly used, when people mean ‘childbirth’, but I would implore you not to use it. Especially when speaking about women who (plan to) have their baby at home, they do not deliver or are not delivered, but they give birth. Pizzas or postal packages are delivered at home, not babies.
In addition to this, there are some small but annoying writing errors or omissions throughout the text. Please check your text carefully.

RESPONSE: Thank you for noticing, we changed the word ‘delivery’ and replaced it by giving birth, throughout the manuscript.

2. Abstract: The results section in the abstract is not clear to me. I have no way of judging whether or not the differences you found are large or small. Now the conclusion that you found no clinically significant differences comes as a surprise. That would be different if you had mentioned the possible range of the LAS-score in the result section.

RESPONSE: We agree that the conclusion with regard to clinical relevant difference comes as a surprise and we have included the value that we will consider as a minimal clinical relevant difference in the method section of the abstract (page 2), last sentence.

3. And if you write that a score was minus 4.2 points lower, does that then actually mean it was 4.2 points higher?

RESPONSE: Yes, we mean it was 4.2 higher in a planned home birth. We have reversed this in the text (see abstract, page 2 result section).

4. I would appreciate it if you would present the results each time in the same order: planned home birth compared with planned hospital birth. Now you have turned that around when presenting the score in the 2nd stage of labour.
RESPONSE: We have changed the order of presenting the results and now consistently each time planned home is compared with planned hospital (see line 4, results section of abstract, page 2).

5. Introduction: You refer to transfer rates in 2008, but I would suggest you refer to the most recent data, about 2010. They are available on the PRN-website.

RESPONSE: Thank you for this suggestion, we have updated the transfer rates. We compared the transfer rates of 2010 with 2008, and it shows a serious increase again. See line 15 and 16, introduction section “Transfer rates during labour…for parous women.”

6. I do not quite understand why a higher home birth rate in the Netherlands might explain that Dutch women, planning a home birth, are more disappointed when giving birth in hospital than Canadian women. Canadian women, planning an home birth, might be expected to be much more focussed on achieving that because it is so special.

RESPONSE: It is true, we can not know whether Canadian or Dutch women will be most disappointed if home birth is not achieved. Therefore, this sentence in the introduction section (that Dutch women might be more disappointed when giving birth in hospital than Canadian women) was removed.

7. Methods: Confounding factors: please explain more clearly how you included social status. Did you use the definition developed by the SCP and calculated the variable based on data about education, income and employment that the respondents provided? Or did you use the data of the SCP (linked to postal code?) and linked them to the client data file?

RESPONSE: We used the data of the SCP linked to postal code, and linked it to the client data file. We have added this information in the method section page 6, confounding factors, line 3-5“For social status….linked it to the client data file.”

8. In the definition of ethnic background, are people classified as western or non-western when one of both parents is born outside the Netherlands, or when one or both parents are born abroad?

RESPONSE: With regard to ethnic background, indeed, people are classified as western or non-western when one or both parents are born outside the Netherlands.

- We have corrected this in the text, in the method section, the paragraph about confounding factors on page 6, line 7,8“...(at least one parent…Asia or Turkey)”.

9. Overall, I would suggest you order the methods paragraph differently, and use only three headings. First the study population, including the Deliver study and the selection for these analyses; second, all the data you are using in these analyses, divided in dependent, independent, confounding and explanatory variables; third, the data-analyses, including hypotheses about expected associations and categorization of the respondents in five groups.

RESPONSE: In accordance with the suggestion of the reviewer, we have ordered the method paragraph differently, using three headings: study population and study design, data collection, which includes subheadings for the independent (planned place of birth) and dependent variable (LAS score), confounding factors, and potential explanatory variables; and data analysis.

- To clarify this to the reader, the method of categorization of the five groups was added to the data-analysis section, and we separately describe the data analyses for all women according
to planned place of birth, and for women who were transferred, and women who were not transferred, see data analysis section on page 7: “From information on place of birth…30 parous women.”

10. Please explain in which of the five subgroups the women were included for whom the start of labour could not be defined with confidence.

RESPONSE: For women for whom start of labour in primary care seemed likely, despite some discrepancies in the data, too little information was available to categorize them among women who were transferred or who were not transferred. Therefore, the sensitivity analysis was only performed for the primary research question of this study, thus for the comparison of women according to their planned place of birth.

- To clarify this to the reader, we added in the result section page 9, sensitivity analysis paragraph: “Sensitivity analysis for…yielded similar results.”

11. Results: The word ‘data’ is plural, so, data were available.

RESPONSE: We have changed the words ‘data was available’ in ‘data were available’ in the first line of the results-section, page 8.

12. What does it mean that for 83 women their start of labour in primary care was not certain? If you are not sure they started labour with a midwife, shouldn’t they have been excluded in the previous step?

RESPONSE: For some women start of labour in primary care seems likely, but information of the LVR1 data shows discrepancies for the onset of labour. We conducted sensitivity analyses for women with and without discrepancies in the definition for start of labour in primary care, while the main analysis was conducted only for women without discrepancies in the definition for start of labour in primary care.

- To clarify this in the data-analysis on page 7, in the last paragraph we added “For the main analysis….labour in primary care.”

13. You describe differences between the planned home and planned hospital birth groups, referring to table 1. But have these differences been tested? No significance levels or p-values are provided.

- We have included p-values in table 1, to see which differences are statistically significant.

14. Why would you want to adjust the analysis of sense of control during the 1st stage of labour with events from the 2nd or postpartum stage?

RESPONSE: We indeed adjusted for factors that happened after sense of control was experienced. However, sense of control during labour was determined on average 6 weeks postpartum. Although it has been shown that LAS remains stable until a couple of weeks postpartum, for example regarding the baby’s health, may have a negative influence on the recall of the birth experience, including sense of control.

- This explanation is stated in the final paragraph of the potential explanatory factors in the method section page 6 last sentence “Finally, the impact…including sense of control.”
15. In the text you only refer to Panel 2 in Figure 2. Please first explain what we see in Panel 1. Also simplify the first sentence of that paragraph, break it up in several sentences. First, explain which group is the reference group. Then mention the non-significant comparisons. Then mention the significant comparisons. Maybe you should provide a table with these data, so your text may become clearer. Now I have the feeling that I am missing the point: what is the most relevant information here, the comparison between planned place of birth or the comparison of transfer status? You are comparing women who planned a hospital birth but gave birth at home with women who planned a home birth but were transferred. What do we learn from that? I find this confusing.

RESPONSE: We have described results more clearly in relation to our research questions. Table 2 refers to the first research question (planned place of birth and sense of control during labour among all women who started labour in primary care). We replaced figure 2 by table 3A which shows the results of the difference in LAS scores between planned home and hospital birth in midwife-led care for women who were transferred during labour. Finally, in table 3B LAS scores are presented according to planned and actual place of birth, among women who did not experience transferal during labour. This table was added to give a full picture of the impact of transfer or not, on sense of control during labour.

- In the text we refer to the figures and tables, see ‘All women’, ‘Women who were transferred’ and ‘Women who were not transferred’ in the result section page 8 and 9. The table 3A and 3B can be found on page 18 and 19.

16. Discussion: The first sentence of the discussion has not even been mentioned in the results section. That should have been the explanation of Panel 1 of Figure 2. The next few sentences are an adequate résumé of the results.

RESPONSE: The first sentence of the discussion section refers to table 2.

17. I do not agree with you that a randomised controlled trial would have been the optimal design for this kind of study. Especially in studies concerning choice, trust, anxiety, control etc. randomization introduces an interference with the emotions you want to capture in your study. The randomization itself will be a confounding factor you cannot eliminate or account for. A prospective design, such as used in this study, is the optimal design to study these kind of questions.

RESPONSE: Thank you for this suggestion. The advantage of an RCT is that it would enable to show causality for planned place of birth, whereas in a prospective cohort study we find associations, rather than causal relations, between planned place of birth and the outcome. In agreement with the reviewer, we think that a prospective cohort study is the best design, because an RCT was shown not feasible.

- To clarify this, we have changed a few sentences in the discussion, second paragraph, line 2-4 “We used data from…unequally distributed characteristics.”

18. Since the PRN data for 2009 and 2010 are now available, I would suggest you refer to the most recent data.

RESPONSE: Thank you for the suggestion to use PRN results from 2009 and 2010. See line 13, 14 in the second paragraph of the discussion section, page 10 “The transfer rate of…2010 (26.2%).”
19. The consideration about clinical relevance (I would keep the word ‘significance’ for statistical issues) is important. This means that none of your results may be clinically relevant.

**RESPONSE**
- With regard to clinical relevance, we stated what we would consider as a clinically relevant difference in the method section of the manuscript, page 5 (and the beginning of page 6), final paragraph (sense of control): “In our study, a…..half a point on a 7 point scale.”, so this does not come as a surprise in the discussion section anymore.
- We have changed significant to relevant in the manuscript with regard to the clinical important difference.

20. In the last sentence on page 8 you propose that medicinal pain relief may be associated with lower sense of control, because you found it explained part of the difference between planned home and planned hospital birth for nulliparous women. But medicinal pain relief is only available in hospital in obstetrician-led care so it would be strongly correlated with transfer. Which in turn may mean that not the pain relief in itself, but the combination of pain relief with transfer is associated with lower sense of control.

**RESPONSE:** This is an interesting comment about the correlation between transfer and pain relief. Indeed, it could be that the higher rate of both a combination of pain relief and transfer in the hospital lowers feelings of control in women who plan hospital birth. However, when adjusting for transfer separately, this had no effect on the association and feelings of control were still lower for women who planned a hospital birth. When the analysis was adjusted for medicinal pain relief, the difference was no longer statistically significant. Therefore, it seems more likely that pain relief (which might reflect the severity of the experience of labour pain) might cause lower feelings of control during first stage of labour in nulliparous women who plan a hospital birth.
- We have clarified this in the method section, page 6, 2nd paragraph on potential explanatory factors “Furthermore, we evaluated…place of birth and sense of control.” And in the discussion section, page 10, 4th paragraph “This could suggest that…to a lower sense of control”.

21. I would appreciate some words on strengths and limitations of this study and would add a subheading ‘Conclusion’.

- Strengths and limitations are included in the 2nd paragraph of the discussion section, page 9 “Our studies had some strengths and limitations which need to be addressed.”
- Finally, we added a subheading “Conclusion”, discussion section, page 11, 12.

22. Reference list: Authors of reference 10 should be: Wiegers TA, Zee J van der, Keirse MJNC.

**RESPONSE:** Thank you, we have corrected this in the reference list, page 11.

23. Tables and Figures: A legend for figure 1 is missing, the one for figure 2 is included twice.

**RESPONSE:** Accidentally we included it twice. Figure 1 has no legend.

References used for the response on referee 2:
Response to referee 3:

Major compulsory
1. The authors need to include details of how the DELIVER questionnaires were given to clients and how clients were followed up if a questionnaire wasn’t returned, given that the return rate was 62%.

RESPONSE: Clients were informed about the DELIVER study by their midwife, during a routine visit. The clients received a brochure with a link to a website. This website contained the questionnaires for this study.
To improve the overall response, a reminder was sent to all non-responders. In addition, five research assistants were enrolled (student midwives) to call all clients who did not complete the questionnaire within one week and invite them once more to participate.
- To clarify this, this information is included in the method section in the ‘study design and study population’-paragraph, 2nd paragraph “Clients who received antenatal care….invited once more to participate”.

2. Social status is included in table one – how is this determined?

RESPONSE: For social status we used a score linked to postal code, developed by the Netherlands Institute for Social Research (SCP), based on education, income and employment rates, and we linked it to the client data file. A low score equals low social status. This explanation is included in the method section, page 6, paragraph about confounding factors: “For social status…low social status.”

3. The only major concern I have with this paper is figure 2. It is extremely difficult to read. In the legend it is stated that 2nd stage is denoted by “I” but this is not the case in the diagram – there are clear circles. This information would be much better presented in a table or at least a table to accompany the diagram. First and second stage should be presented separately in different tables or in different sections of a table. The categories (home- home) should be more explicitly defined. The findings from the primary hypothesis should be clearly presented i.e. planned home and transferred vs. planned hospital and transferred.

RESPONSE: We would like to respond to the useful comments concerning figure 2. To find out what the impact is of transfer in a planned home birth, on sense of control during labour, this study aimed at two main research questions (RQ): 1 = difference in LAS score between women who planned a home or hospital birth in midwife-led care and who were in primary care at the onset of labour and 2 = difference in LAS score among women who were transferred during labour.
- To clarify this further, we added a table on page 18, table 3A which includes the results of RQ2. Table 2 refers to RQ 1. Additionally, in table 3B LAS scores are presented according to planned and actual place of birth, among women who did not experience transferal during labour. This gives a complete overview of how sense of control varies among women who planned hospital and home birth. We refer to tables, see ‘All women’, ‘Women who were transferred’ and ‘Women who were not transferred’ in the result section, page 8 and 9. Below table 3A and B, page 19, and in the data analysis section, page 7, “From information on place of birth…. (22 nulliparous women and 30 parous women)” it is described how the different categories are defined. The figure has been removed and it was replaced by table 3A and 3B, because the figure was difficult to read, and it was suggested to present these results in a table.

Minor essential
1. The abstract would be more clear if the last sentence read “…we studied sense of control among women who were transferred to physician care during labour according to planned place of birth: home vs. hospital. Other than this the abstract is clear and concise.

RESPONSE: Thank you for this suggestion, we have included it in the abstract, page 2, last sentence of background “In particular, we studied sense of control…: home versus hospital.”

2. In the methods what was the sampling framework for choosing the 20 practices? – more details needed.

RESPONSE: In the Netherlands there are 519 primary care practices and twenty were approached and invited to participate in this study. Purposive sampling was used to select practices, using three stratification criteria: region (north, centre, south), level of urbanisation (urban or rural area), and practice type (dual or group practice). Four practices were selected in the south, 10 in the middle and another 6 in the North of the Netherlands. Six practices were rural, five were urban and 9 practices were both urban and rural. Furthermore 2 duo practices were selected, the other were group practices.

3. Given that linkage to the Netherlands Perinatal Register was 86% is there anything you can tell the reader about how non-linked subjects differed from linked subjects?

RESPONSE: Thank you for this suggestion. We compared maternal age, socioeconomic status and ethnic background for women who had and had no data of the Netherlands Perinatal Register (LVR1) available. Women were comparable for maternal age and ethnic background, however, women with LVR1 had higher socioeconomic status than women without LVR1 data.

- This information was added to the method section, 3rd paragraph of study design and population page 4 "Women with and without….LVR1 data available."

4. Under data analysis it is stated that analyses for explanatory factors was done using an adjusted multilevel model. The nature of the model was not explained, nor how it was decided which variables should be retained in the model.

RESPONSE: The explanatory factors were added to the multivariable multilevel model in addition to the confounding factors, i.e. ethnic background, maternal age and socioeconomic status, one at the time, thus, for example, we added medicinal pain relief as an explanatory factor, in addition to the confounders (mentioned above), in the multilevel model with planned place of birth as independent variable and LAS as the dependent variable. Next, we added another explanatory factor, but at the same time we removed the first, in this case medicinal pain relief.

- In the data analysis section, 3rd paragraph page 7, it has been clarified to the reader: “Next, in an additional analysis, possible explanatory factors……, one at a time”.

5. The authors should give a brief description of the Predicted Mean Matching method. To say that the number of imputations was based on the percentage of missing values is not enough information. Was there a cut-off – i.e. if a certain number of responses were not answered was that questionnaire deleted or were all missing values imputed no matter how many?

RESPONSE: With the Predicted Mean Matching method each missing value is imputed randomly from a set of nearest observed values in the dataset.

- To clarify it in the text, this information is added to the data-analysis section, page 7 “With this method….observed values in the dataset”.
All missing values were imputed, no matter how many items were missing, including if all ten items were missing.

- In the data-analysis section, page 7, we added “All items for anxiety were imputed when missing”.

6. Under planned place of birth and LAS score, it is stated that the association between planned place of birth and sense of control during first stage of labour was partly explained by pain relief – the authors should be more specific – what was it before adjustment -were there more pain relief measures given in the planned hospital birth group? – this needs more clarity. One might also question whether this should be adjusted for because pain management or lack thereof would be on the causal pathway to sense of control and adjusting for it would decrease the ability to see valid differences between groups. The authors make this point in the discussion.

RESPONSE: Among women who planned hospital birth the rate of pain relief was much higher (22 versus 10%, see table 1). We agree that it is very likely that pain relief is in the causal pathway in the association between planned place of birth and feelings of control. The results (table 2, adjusted for ethnic background, maternal age and socioeconomic status) show a difference in sense of control between planned home and hospital births. We were interested, whether this difference was explained by the higher rate of medicinal pain relief in the hospital group. So we added medicinal pain relief as an explanatory factor to the multilevel model. In first stage of labour for nulliparous women it appeared that the difference was no longer statistically significant. Therefore we reasoned that place of birth itself has no independent effect on feelings of control, but that medicinal pain relief, which is used more by women who plan a hospital birth, is causing lower feelings of control. Indeed, this suggests that medicinal pain relief is in the causal pathway.

- We have clarified this in the method section, page 6, 2nd paragraph of potential explanatory factors “Furthermore, we evaluated…place of birth and sense of control.” And in the discussion section, page 10, 4th paragraph “This could suggest…to a lower sense of control”.

7. In table 1, symbols for footnotes should be superscripts – they are difficult to read in the table.

RESPONSE: We have changed the symbols for footnotes into superscripts, see table 1 on page 16 “Pregnancy related anxiety” and “Pharmacological pain relief”.

Discretionary

1. 4th paragraph in introduction, last sentence is not clear. It could read “the positive experiences associated with planning and beginning labour at home may be mitigated by the experience of moving to hospital if transfer of care to a physician is required.”

RESPONSE: Yes, this is confusing. What we mean is that ‘positive experiences associated with planning and beginning labour at home may be overshadowed by negative experiences of women who are moving to hospital if transfer of care to a physician is required.

- We have changed the sentence in the introduction section, 7th paragraph “The current thought is…to an obstetrician led unit is required.”

2. 6th paragraph in introduction, last sentences would be more clear if it stated that women planning home vs. hospital birth might be more disappointed if care was transferred to a physician because it would mean that they would have to move to hospital and thus not give birth in their chosen setting.
RESPONSE: Thank you for this suggestion, this supports our hypothesis. We have added it to the introduction section, final paragraph page 3, 4 “Women who plan a home birth…..in their chosen setting.”

3. Was the translated LAS back-translated into English to check on accuracy of translation?

RESPONSE: Yes, this was done, we added this comment to the method section, page 5, paragraph sense of control, line 13 and 14, “The translated LAS-11…on accuracy of translation.”

4. I am curious to know why women from Indonesia or Japan were included with Europeans and North Americans in the ethnic background categories. This may require some explanation for non-European readers because it is different than census categories in North America.

RESPONSE: We have adapted the definition from Statistics Netherlands. The definition aims to make a homogeneous categorisation based on comparability of the non-native (non-western) population in the Netherlands to the native Dutch (and western population) taking into account socioeconomic and cultural aspects.

- This has been clarified in the method section, confounding factors, page 6 2nd paragraph: “The definition aims to make….socioeconomic and cultural aspects.”

5. The transfer rate was extremely high at 60.5% for nulliparous women. Can the authors include reasons for transfer of care?

RESPONSE: The 3 main reasons for transfer of care were meconium stained liquor (22%), medicinal pain relief (17%) and failure to progress during first stage (16%) or second stage of labour (16%). This has been added to the result section, page 8, 2nd paragraph “Main reasons included…second stage of labour (16%).”

6. In the discussion the authors cite reference 10 as essentially giving the same results as this study. It is important to say how this older study differed from the current one; i.e. they did not use the LAS.

RESPONSE: Indeed, they used another instrument to measure the experience of childbirth. We have included this comment in the discussion section, page 11, third line: “..although this study did not use the LAS.”

7. Table 2 – p values are not necessary as confidence intervals are given for differences in scores.

RESPONSE: We have removed the p-values in table 2, page 17.