Author's response to reviews

Title: A prospective study of effects of psychological factors and sleep on obstetrical interventions, mode of birth, and neonatal outcomes among low-risk British Columbian women

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Author's response to reviews: see over
Dr. Cecily Begley  
Associate Editor  
BMC Pregnancy and Childbirth  
Trinity College School of Nursing and Midwifery  
D'Olier Street  
Dublin  

November 6, 2011  
Re: A Prospective Study of Effects of Psychological Factors and Sleep on Obstetric  
Interventions, Mode of Birth, and Neonatal Outcomes among Low-risk British Columbian  
Women MS: 1045194310572936

Dear Dr. Begley,

I thank you and the reviewers for the very careful examination of our paper. I am writing this cover letter to describe our point by point response to the comments of the reviewers for BMC Pregnancy and Childbirth. I will describe the changes I have made in point form. We have had a paid statistician working with us from the outset of the project. He has again reviewed the paper following the changes we have made. I am incorporating his comments in two of the points and will identify those comments as emanating from our statistician. I will begin with our response to Linda Kvist’s comments followed by our response to Julie Quinlivan’s comments.

1. In response to the point about the problem with the Chi-square analyses of obstetrical and neonatal outcomes replicating our analysis using logistic regression, I have removed the sections of the paper and tables that included the Chi-square analysis of obstetrical and neonatal outcomes. We did not use the Chi-square analysis to develop our hypotheses. The hypotheses were developed a priori from the literature.

2. I have removed the second sentence in the data analysis section about a statistically significant obstetrical intervention. With the Chi-square analyses of obstetrical interventions and neonatal outcomes removed there is no longer any reference to relative risk in the paper.
3. We only created the dichotomous variable for adverse neonatal outcomes for the logistic regression analysis. Children were coded as a one if they had any or all of the adverse outcomes so they would not confound each other. We have removed the Chi-square analyses for neonatal outcomes where that possibility could occur because we examined outcomes separately.

4. The table where we report the only significant finding from the logistic regression analyses has been altered to include Wald statistics. We did not select variables to be included in the regression analysis based on statistical tests. As stated previously, these were a priori hypotheses developed from the literature. We did not regard reporting all of the analyses by table for non-significant findings as useful for the reader or for journal space. Although we need to repeat the hypotheses in the results section, doing so takes considerably less space than replicating all of the non-significant analyses in tables.

5. I have clarified the comment about health issues in the first sentence of the results section.

6. Because I have removed all of the Chi-square analyses for interventions and outcomes, there is no longer a reference to increased risk of prematurity being associated with sleep deprivation.

7. I have gone through the manuscript and removed any vague language by either providing the definition of the term or removing the terms.

8. I have included the number of the participants recruited and the number of women participating in the abstract as requested. Comments were also added about locations for recruitment. There is now a comment about the total number of participants on p. 8 of the manuscript.

9. I have signified the significant results in the abstract and which analyses were stratified for parity.

10. I reduced the length of the sections about psychological states in pregnancy and sleep deprivation. I removed the first version of the aim of the study in the background and relocated the statement about potential use of the findings to the discussion section of the paper.

11. I changed all references to characteristics to only maternal characteristics.

12. I briefly specified the difference associated with NICU levels 2 and 3, see p. 8.

13. I developed the section about the contribution to the literature arising from our finding about childbirth fear predicting epidural use, see pp. 14-15.

14. I added considerable detail about how some of the limitations we encountered could have been avoided, see p. 16-17.

15. In the discussion and conclusion sections, I added some suggestions for interventions and how those interventions might be studied in future, see pp. 15 and 17.

16. The generalizability of the study has been addressed in the limitations section, see p. 17.

17. I have reduced the length of the methods section. It is difficult to do so by a significant amount because the statement of the hypotheses accounts for almost 2 pages. We have tried to amalgamate the hypotheses in previous drafts of our paper and returned to the original format because I felt there was lack of clarity. In each hypothesis, different variables were controlled. I have defined adverse neonatal outcomes in the abstract and also in the purpose of the study on p. 5.

18. I have checked with my statistician who had indicated it is appropriate to use the term associations in relation to Chi-square analyses.

19. In the background section of the paper, I have removed the word ‘the’ in sentence two.
20. I have cut paragraph 2 from the introductory section and combined the paragraphs in the section on psychological states and reduced the length of the paragraph on sleep deprivation. Those suggestions were very helpful.

21. As I indicated, we have had a statistician involved in the project from its inception. I have replicated the reply from our statistician here. “Regarding the issue of multiple comparisons, we chose to apply a Bonferroni correction. As the Bonferroni correction is a tool for family-wise type 1 error rate rather than the study-wise error rate, we applied it only to the family represented by our 7 maternal outcome hypotheses/comparisons (0.05/7 = 0.007) rather than to all of the analyses undertaken in the study. In addition, we have included the odds ratios to allow the reader to judge how large and potentially meaningful the effects are. I have deemed the use of the more conservative alpha (0.01) to be sufficient correction for our preliminary Chi-square analysis looking at maternal characteristics. I agree with the reviewer who has suggested that the Chi-square analyses of maternal and neonatal outcomes be dropped.”

22. I only included the variables used in the analysis for this paper, see p. 8. Because only Julie Quinlivan commented on the length of the questionnaires section, I reduced the length by tightening commentary on the measures but did not resort to only including references. There is now only essential information about the measures incorporated in the paper.

23. All references to premature have been amended to preterm.

24. Discussion of Fenwick’s paper has been altered as requested, see p. 15.

25. The paragraph on the findings about epidural anaesthesia has been made more prominent, see pp. 14-15.

26. Any comments about birth management by care providers have been deleted from the manuscript.

I hope that these point by point comments address all of the reviewers’ concerns. I would be happy to put you in contact with our statistician if you feel it is necessary. I did not incorporate tables one and two in the paper because they are in landscape format and the instructions indicated any tables must be in portrait format. All authors have contributed to the design and conduct of the study and to the writing of the paper and as such all merit authorship. All authors have seen and agreed the final version of the paper. We have submitted this paper only to your journal and we have no conflicts of interest to declare. We thank your for your kind attention to this matter.

We look forward to hearing from you in due course.

Kind regards,

Dr. Wendy Hall