Author's response to reviews

Title: Pregnancy planning, smoking behaviour during pregnancy, and neonatal outcome: UK Millennium Cohort Study

Authors:

Anika Flower (ajmflower@gmail.com)
Jill Shawe (j.shawe@ucl.ac.uk)
Judith Stephenson (judith.Stephenson@ucl.ac.uk)
Pat Doyle (pat.doyle@lshtm.ac.uk)

Version: 2 Date: 29 October 2013

Author's response to reviews: see over
Author response to reviewer’s reports

Title: Pregnancy planning, smoking behaviour during pregnancy, and neonatal outcome: UK Millennium Cohort Study

We thank the reviewers for their useful comments. Our response is presented below in blue italics, inserted directly under the relevant reviewers’ text. We have included the reviewers’ comments in full for completeness. New and/or changed text in the revised manuscript is presented in red.

REVIEWER 1

Version: 1 Date: 3 October 2013
Reviewer: Alys Havard

Reviewer’s report:
This manuscript aims to answer an important and interesting question about whether planning pregnancy is a potential avenue for improving neonatal outcomes, over and above focusing on quitting/reducing smoking. Although data addressing this research question have the potential to make a valuable contribution to the field, the manuscript has a number of important limitations.

MAJOR COMPULSORY REVISIONS

My first concern relates to the retrospective nature of the data collection. The authors acknowledge that respondents were asked to recall events up to 2 years before they were interviewed, and they recognise that self-reported smoking is subject to under-reporting. The potential impact of this, however, is diminished by stating that rates of pre-pregnancy smoking were similar to rates reported elsewhere. Given the pressure on women to avoid smoking during pregnancy, the risk of under-reporting is greater for the questions regarding smoking during pregnancy, with less concern the reporting of pre-pregnancy smoking. In light of this, it is not reassuring that higher than usual quit rates (77-84%) were reported by participants. The implications of this likely underreporting should be discussed. Within this there should be some consideration of the potential for differential misclassification in the measurement of smoking during pregnancy – smoking was assessed after the outcomes (low birthweight and pre-term birth) had already occurred, leaving open the possibility for recall bias associated with the outcomes.

(i) This is an important issue and we thank the reviewer for raising it. We have added text to the discussion section under strengths and limitations, page 9. This text includes a discussion of the implications of potential under-reporting of smoking status during pregnancy, including the possible existence of differential under-reporting related to outcome.

My second major concern relates to the measurement of smoking. The comparisons reported in Table 3 involve the group of women who smoked before pregnancy. Some of the women in this group quit smoking during pregnancy, or
changed the amount they smoked. The comparison therefore seems to be never smokers vs women who did not smoke during pregnancy (because they quit) + women who smoked less during pregnancy + women who smoked the same amount during pregnancy. It is difficult to know how to interpret the results of these comparisons.

(ii) We thank the reviewer for this comment and agree that the comparisons of those who decreased/quit and those that did not (Table 4) are generally more informative than Table 3, which compares outcomes for never smokers and a combined group of women who had ever smoked. We recognize this is a crude comparison, but defend keeping this in the paper because it gives an overall comparison of non smokers v smokers which we believe is useful context before Table 4.

The comparison between women who decreased/quit their smoking during pregnancy and those who did not (Table 4) seems to be more meaningful, however, I have concerns about the validity of this distinction. The question from which these groups were identified is worded as “Did you change the amount you smoked during your pregnancy?”. It seems to be assumed throughout the manuscript this ‘change’ is equivalent to a reduction, although this assumption is not stated anywhere, nor is any justification given.

(iii) Change does mean reduction. We have added text to the Methods on page 4.

It is not explicitly stated how smoking during pregnancy was categorised when entered in the pregnancy planning models as a covariate, but it is possible that the results in Table 2 are also affected by the problematic measurement of smoking discussed in relation to Tables 3 and 4.

(iv) The smoking co-variate used in the pregnancy planning model was categorised into four groups: never smoker, ex-smoker, smoker who quit or cut down during pregnancy, and smoker who did not change amount smoked during pregnancy.

Insufficient information is given regarding the methods for calculating the population attributable risk fractions, making it impossible to assess the likely validity of the estimates produced.

(v) We apologise for the lack of detail on this. We have added text to the methods section, page 5 and 6.

The implications of the findings regarding the impact of smoking on neonatal outcomes after adjusting for planning are not discussed and should be. My interpretation of the findings is that smoking during pregnancy has an impact on birthweight that is over and above the effect of other health behaviours (with planning for pregnancy acting as a proxy measure for other health behaviours during pregnancy). I would think that this has been demonstrated previous research by directly measuring those other health behaviours contributing to
neonatal outcome.

(vi) We have added a sentence to the Discussion on page 11. However, interestingly, adjustment for pregnancy planning over and above other confounding factors does not change the measure of effects to a major degree. This is demonstrated by only small changes in the odds ratios between adjustment model 1 (not including pregnancy planning) and adjustment model 2 (including pregnancy planning) (table 3 and 4).

MINOR ESSENTIAL REVISIONS

It is stated that the survey designed oversampling and response rate were accounted for using variables that had already been created. A web address to the home page of the ESDS is provided, when a direct link or citation of a document describing the methods for creating these variables should be provided.

(vii) We have added a new reference (Methods, page 4) for a technical report which describes sample weighting

It is stated that the missing data on the 4.5% of participants who did not survive to 9 months of age (when the interviews were conducted) is unlikely to significantly impact the results. It is also not acknowledged in the limitations section that 4% of records had missing smoking information. Given that neonatal death is likely to be strongly related to both of the outcomes, and the missing smoking status is likely to be related to the smoking during pregnancy exposure, there is a strong risk that these missing data contributed to a bias.

(viii) We agree that missing data may have contributed to bias, and predict that the effect of such bias would have been to underestimate the effect of smoking during pregnancy and adverse outcome. We have added text to the discussion on page 10.

MINOR ISSUES NOT FOR PUBLICATION

Figure 1 appears to be missing a legend.

(ix) Legend added

The 3rd last sentence of the Results section of the abstract appears to be missing a word. I think it should end ‘did not change’ instead of ‘did change’. The first sentence of the Conclusions section of the abstract: typographical error with ‘heath’ written instead of ‘health’.

(x) Thank you, this is now corrected
REVIEWER 2

Version: 1 Date: 14 October 2013
Reviewer: Jamila Mejdoubi

Reviewer's report:

- Major Compulsory Revisions

1. In the second paragraph of the background section the authors say that there is no information on the health impact of pregnancy planning itself. Why is pregnancy planning important? How can health care providers address this? Are health care providers able to address this? Because there are high risk pregnancies (Mejdoubi et al 2011) (young age, low education level etc.) that are unplanned. How are you going to approach these women? Or do you want to address only low risk pregnancies? This should be explained in the background.

_We thank the reviewer for these comments. We have added a sentence to this paragraph on page 3. We have not included further detail on potential interventions (e.g. health care provider issues) because operational intervention issues were not objectives of this study._

2 In the second paragraph of the methods section the authors write that if women reported to smoke 1 cigarette a day they were classified as smokers. What if the women said to smoke only one cigarette a week, is she a smoker? Maybe it is better to state that if women reported to smoke zero cigarettes a day, she is a non-smoker.

_The question at interview asked about smoking on a daily basis. If a woman reported smoking less than one cigarette per day, that woman was classified as a non-smoker. These variables were fixed and beyond our control to change._

3 You analysed if women had adverse pregnancy outcomes. Why did not you analyze “small for gestational age”? this is also an important adverse pregnancy outcome.

_We agree that SGA would have been a useful variable to study. Unfortunately, this information was not available in the MCS cohort data available to us._

4 In the results section you used four group smoker, non-smoker etc. and then you used crosstabs and chi-square analyses. I do not think that it is the right way to analyze this, because there are more than two groups. For dichotomous outcomes you can use this analyses and also logistic analyses. But to analyse more than two groups you should apply different analysis.

_We respectfully disagree. Since the outcomes (Low birthweight and prematurity) are dichotomous, it is possible to use Chi square for more than..._
two levels of an exposure variable. But we do agree that logistic regression is the appropriate method for assessing adjusted effects (which we have used).

5 Which confounders did you use in table 1, because you stated earlier that education level and SES was different between planners and non-planners. The confounders used in adjusted models are presented at the bottom of the relevant tables (2,3,4).

6 In the discussion section you should explain how your results are relevant for health care providers. How can health care providers address this issue and especially among you women who are low educated and usually do not plan their pregnancy.

Thank you for this comment. We wish to draw the reviewers attention to the final paragraph of the Conclusion section.

- Minor Essential Revisions

7 If you refer to an article, do not refer in the middle of a sentence but at the end of a sentence

Noted. The editor may wish to comment on this.

8 What does NICE stand for? Write the full name down.

Done (page 3)

9 The third paragraph of the background section: Millenium cohort data (MCS)

10 the second paragraph of the methods section (page 4): Smoking status during …including the “how.. You forgot the word “question”.

Thank you, corrected.

11 Why did you used data for singletons only? This should be clarified in the methods section.

When analysing low birthweight and prematurity it is usual practice to exclude multiple births because multiplicity is the main indictor of these outcomes.

12 Which confounders did you include in the models?

The potential confounders are listed in the Methods section, and the confounders actually used in the models are presented in the footnotes of tables 2,3,and 4.

13 clarify the term “higher birth order”

This is any birth order over 2, as presented in appendix table 1.

14 the authors should not report the results as “ % increased odds” because it is an odds not a relative risk.

We take this point, but it is usual practice to use relative risk in this context.

15 Pregnancy Planning section on page 7: you do not have to write down the p level of the effect modifier.

On page 8 we state “No effect modification between pregnancy planning and smoking was detected (p>0.05)”

In the table

16 Change Muslim/Islam into Muslim

Done
- Discretionary Revisions
17 Use a different term for Avoiding behavior.
We have changed “behaviours” to behaviour” on page 3.
18 the third sentence of the Background section (page 3) remove “for both women and men”
We consider it important to leave this in, to make the point that men also need to plan for pregnancy.