Author's response to reviews

Title: Socio-demographic factors associated with smoking and quit of smoking among 426,344 pregnant women in New South Wales, Australia

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Version: 3 Date: 2 November 2005

Author's response to reviews: see over
November 2, 2005
The Editor
BMC Public Health
Email: info@biomedcentral.com
http://www.biomedcentral.com/bmcpublichealth

Re: 5333114257474145 - Socio-demographic factors associated with smoking and quit of smoking among 426,344 pregnant women in New South Wales, Australia

Dear Editor

Many thanks for providing us the reviewer’s reports. We are pleased to submit a revised version of the article incorporating all the specific points raised by the two reviewers. Please find the attached revised version of the article and a detailed response to each of the reviewer’s comments.

Reviewer 1

Comments #1: The paper does not clearly motivate or delineate its major contribute above existing literature. More detail regarding smoking among indigenous peoples in Australia could be useful here.

We have made a clearer primary focus, to identify very high risk groups among those who continue to smoke in pregnancy (SIP). This sharpens the descriptive epidemiological analysis.

Only 2.5% of the NSW mothers are from indigenous community. A future study can be done to discuss smoking issue among Aboriginal (indigenous) women only, which has already been focused in conclusion section (page 11). We have focused more on this special population, with an examination of the interaction with age and antenatal care attendance [table 2]

Comments #2: The paper would benefit from a sharper focus on its purpose and contribution to the literature.

As per suggestion, last paragraph of the background section been modified (page 4).

Comments #3: Given that the paper’s objective is descriptive epidemiology – the paper could be made more succinct and focused.

The old versions of Table 2 & Table 3 have been combined as Table 3. A new table 2, identifying extreme risk groups, is now added to focus the paper better. The background and discussion section been revised.

Comments #4: The recent work by, Neil Benowitz who examines the genetic predisposition that people have for smoking, would add to discussions in the 3rd paragraph of the Background section (page 3).
As per suggestion the recent work by Neil Benowitz been added in the background and discussion section (see reference 21).

Comments #5: Reporting bias and other likely study biases and their implication is needed in the paper.

In the methods section validity of the midwives data items been already mentioned (page 5, reference 36). The data are comprehensive of all mothers. However, reporting bias, especially in a behavioural attribute like SIP may be differential over time; there is limited data to support or reject this; recent data validates the SIP measure, but it may change over time [with non disclosure masking prevalence rates]; however, even if this were true, it would be unlikely to alter the correlates of SIP.

Comments #6: There are numerous instances where use of language/sentence structure does not read well and could be worded better. Some examples include: “and quit of smoking” in the title of the paper (“smoking cessation” would read better), “elderly mothers aged over 35” paragraph 1, page 9 (older mothers would be preferable here), ”women smoked”, Tables 1 and 2 (“smokers” would be better here,) page 4 (paragraph 2), page 7 (paragraph 4, sentence 3), page 8 (paragraph 2, sentence 7), page 9 (paragraphs 1 and 3). Use of plurals, capital letters and missing words need careful checking throughout the paper.

Language has been improved as suggested: “quit of smoking” been replaced “smoking cessation”, “elderly mothers aged over 35” been replaced by older mothers’ and ”women smoked” been replaced by smokers.

Comments #7: page 4 (paragraph 2), page 7 (paragraph 4, sentence 3), page 8 (paragraph 2, sentence 7), page 9 (paragraphs 1 and 3). Use of plurals, capital letters and missing words need careful checking throughout the paper.

Use of capital letters and other minors issues been corrected.

Comments #8: Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

As per suggestion, the whole paper has been revised.

Comments #9: (page 4-5) I was unclear how complete the MDC was in relation to the number of births recorded in NSW. Pregnant women are highly mobile and have different maternity service providers. Also, given that it relies on the attending midwife to complete a notification, is it possible that this may not always have occurred? Some elucidation of this in the paper would be useful.

In Australia, birth registration is compulsory. In the methodology section it has been clearly mentioned that the NSW MDC is a surveillance system covering all births in NSW public and private hospitals, as well as home births (page 4). The validation of the NSW MDC data items are mentioned in the methodology section (page 5, reference 36).
Comments #10: The formatting of references require checking through as there are differences in style with some references containing bold font for volume numbers and some not.

As per suggestion all the references been formatted in line with the journal framework.

Comments #11: Tables need to standardised (e.g., some reference groups are denoted by 1.00 to two decimal places whereas others are written as 1.0 to one decimal place).

As per suggestion tables been standardised in terms of decimal point. For percentage one decimal point and odds ratios two decimal points were used.

Comments #12: The variables used in the modelling of adjusted ORs need to be added as a footnote to the tables.

As per the suggestion, the variables used in the modelling to produce adjusted ORs are added in footnote (although tables are clear about the variables included in the model).

Discretionary Revisions (which the author can choose to ignore)

Comments #13: Given the size of the samples – the reporting of significance tests at the alpha =0.05 may be unnecessary. Invariably any comparison with these sample numbers will be ‘statistically significant’. I suggest removing all p-values from the manuscript and instead only report effect sizes and confidence intervals.

Most of the variables were significant at p<0.01 and couple at p<0.05 level. In general as a whole for significance, ‘p<0.05’ has been used for interpretation. From a theoretical point of view there is nothing wrong about the use of p<0.05, and it is internationally acceptable whether sample size is large or small. The 95% confidence interval for odds ratios are already in the Table 1, also whether the variables were found significant or not in cross-tabular analyses been in indicated. So there is no need for additional confidence intervals, as those in the adjusted ORs should suffice.

Comments #14: (page 16) The distribution of the categories of tobacco use could be specified. This would include the numbers (percentage) of mothers responding to (1)-(4) and the numbers missing.

The mothers whose smoking status are missing or inadequately described or not stated are excluded from the analysis. As per suggestion a footnote been added under the tables.

Reviewer 2

Comments #1: I do not think that the comparison between women falling into the categories “heavy” and “light” smoker is particularly worthy of publication. To my knowledge this distinction has no proven clinical significance and hence the implications of being in either category are unclear. Additionally at a different point
in the manuscript, the authors allude to the fact that it is difficult to validate women’s reports of heaviness of smoking during pregnancy. This means that biochemical validation of the heaviness of smoking is probably needed to objectively allocate women to different categories. Additionally as women’s rates of metabolising nicotine increase in pregnancy and this is likely to have an impact on the number of cigarettes smoked, it is difficult to state in advance what constitutes heavy and light smoking in pregnancy.

Although the clinical significance of heavy (10 or more cigarettes) and light smoking (less than 10 cigarettes) has not been proven yet. However, there is lack of information about the statistics for quantity of smoking during pregnancy. So the statistics on quantity of smoking during the pregnancy may be useful in future research.

Comments #2: Tables 1 and 2 present factors associated with continued smoking and smoking cessation during pregnancy respectively. This is fairly repetitive and, as one might expect, very similar explanatory factors are associated with these two outcomes in multi-variate analyses. I would recommend removing the analysis of factors associated with smoking cessation during pregnancy. The public health problem for which we need a solution is smoking during pregnancy and consequently, it makes more sense to identify the factors associated with this and the characteristics of this group of women.

As per suggestion, to reduce the analyses the old versions of Table 2 & Table 3 been combined as Table 3. Yes, there are some similarities on factors associated with smoking during pregnancy and factors associated with smoking cessation during pregnancy. However, identification of the specific groups which are less likely or more likely in favour of smoking cessation during pregnancy will be more useful for the intervention to initiate for smoking cessation during pregnancy. The identification of highest risk groups is now shown in the new version of table 2.

Comments #3: Title: “quit of smoking” is cumbersome.

As per suggestion, “quit of smoking” been replaced by smoking cessation.

Comments #4: Abstract: The results section of this is fairly repetitive and could be improved by reducing analyses as stated above. The conclusions section in the abstract does not make reference to the unique findings of the survey and could be made without the study having been performed.

Edits have been made to shorten both results and conclusions in this regard. The conclusions section in the abstract been rephrased.

Comments #5: 3rd paragraph, page 9, final sentence: This is unclear.

This sentence been deleted.

Comments #6: The discussion section should discuss the novel, interesting findings of the survey more clearly. For example, the discussion could suggest components of smoking interventions that are appropriate socio-economically disadvantaged women
who do not engage with ante-natal care services early in their pregnancy.

Discussion section been revised, especially around the need for audience segmentation in intervention development.

Comments #7: Another interesting finding from the survey of which little is made is that smoking rates amongst young pregnant women are much higher than those amongst young women in population generally. This may be a reflection of the finding from other surveys, that pregnant women who smoke in pregnancy may have very different attitudes and beliefs compared to others (See Owen and Penn Health Development Agency surveys on UK HDA website). I feel that if the authors gave this finding more prominence, it would improve the utility of their manuscript greatly.

The work of ‘Owen and Penn’ already mentioned in introduction section (page 4), and included in the reference section (see reference 27). As per suggestion, the discussion section also added this reference (page 8).

Yours sincerely,

For the authors,

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