Author’s response to reviews

Title: Prevalence and patterns of cigarette smoking before and during early and late pregnancy according to maternal characteristics: the first national data based on the 2003 birth certificate revision, United States, 2016

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Re: Resubmission of the manuscript # REPH-D-19-00244

Dr. Fernanda Ewerling

Assistant Editor, BMC Reproductive Health

Dear Dr. Ewerling,

Thank you very much for the opportunity to revise my manuscript Prevalence and patterns of cigarette smoking before and during early and late pregnancy according to maternal characteristics: the first national data based on the 2003 birth certificate revision, United States, 2016. I appreciate your comments, which have been very helpful in revising the manuscript, and would like to thank the two reviewers for their valuable suggestions.
As per your instructions, I have edited the text point by point using ‘track changes’ and responded to reviewers’ comments below addressing the issues accordingly. I have also expanded on some parts of the manuscript to make them clear in the ‘clean copy’.

I hope that the revised manuscript will now be considered for publication and if necessary, I will gladly make additional changes. I would like to thank you for your continued interest in my research.

Sincerely yours,

Anthony J. Kondracki

Reviewers Comments and Author Responses

REVIEWER # 1:

General comments:

The paper provides good information about a subject that has already been widely reported (maternal smoking and its patterns), but it is the first in the US to report data based on a nationally representative registry. The manuscript is clear in presenting a sound introduction, identifying what the paper is adding to what is already known. Overall the methodology seems sound, and in our opinion the study has two main strengths. First, it has good generalizability to all the United States. Second, it uses 7 categories of smoking status plus pre-pregnancy use; that clearly describe the whole spectrum of tobacco use in pregnant women, better than previous studies, and thus exploring these patterns which are unique to tobacco use during pregnancy and differ from adult or youth general patterns/statuses.

Abstract:

The author states in the abstract that "The highest smoking rates in pregnancy were among non-Hispanic White women (76.33%), women 25-29 years old (33.01%), and women with a high school education (41.57%)." The author should consider changing or removing that specific paragraph of the abstract.

I agree with the reviewer and thank you for pointing this out. I have edited the abstract accordingly to make it clear [Page 2, Lines 36-38].
Results:

The author introduces discussion or comparisons with existing literature in lines 227-229 and 247 -250. This section comments should be moved to the Discussion section. In fact, part of those comments (complete smoking cessation…rather than reduction…) is repeated afterwards in lines 310-312 in the Discussion itself.

Thank you for this suggestion. I found your comments very helpful and moved the sentences from the results to the discussion section and deleted the repeated sentences [Page 15, Lines 315-319] [Page 15, Lines 329-336].

Discussion:

In line 322 the author states that …. "Disparities by age and education may be attributable to understanding harms related to tobacco exposure". I consider that sentence inaccurate or at least not very comprehensive. It has been described that smokers in general show high levels of knowledge about harms related to tobacco use, so it is unlikely that this factor explains the difference. Other variables such as being or nor primiparous; motivation to quit or awareness (better than understanding) of harms, could be also playing a role. Smoking in pregnancy is known to be influenced by much broader inequalities that are hard to be measured by registries (attitudes, access to health services, poverty, cultural beliefs, etc). I would suggest expanding on those issue)

Thank you. This is a good point. I followed your advice and made the changes. [Page 15, Lines 329-336]

In line 325 the author mentions that "Current evidence is insufficient regarding cessation treatments options that could be recommended to pregnant women (e.g. bupropion, varenicline)” which is absolutely true. In addition to this fact, it could be added that proved cessation interventions such as counseling, feedback, financial incentives have positive but limited efficacy, which have shown to be even less effective in real world scenarios. (references below).

Finally, I recommend accepting this manuscript, provided these minor corrections are done.

Thank you so much. I have made corrections and expanded on smoking cessation interventions among pregnant women citing the relevant literature. [Page 16, Lines 338-351]
REVIEWER # 2

General comments

I commend the author for completing this work that I think will contribute to the literature on smoking during pregnancy, particularly with the use of this large national dataset.

It is my recommendation that this paper is published pending some revisions.

I found the paper to be quite compelling and the analyses to be thorough, comparing with other relevant literature.

Thank you very much for your positive comments and favorable opinion about my manuscript.

I think there should be more contact with relevant literature and perhaps a more nuanced statement of the contribution of this paper. For example, on pg. 5, line 106, is it the case that other studies have presented smoking prevalence among pregnant women using national samples, at least two separated by trimester/month of pregnancy? Additionally, several of these have involved characteristics that are associated with smoking during pregnancy. Perhaps the contribution here is that this sample size is much larger than those studies, and it is more recent?

Thank you very much for the suggestion and references. I cited two studies that you mentioned. [Page 13, Lines 272-276] [Page 15, Lines 320-322].

Minor points

Pg. 4, line 72-84 starting with "historically…" is interesting. I think a succinct summary would suffice.

Thank you very much. I made the changes in the text. [Page 4, Lines 81-85]

Pg. 5, line 88. Why is it that you present prevalence separated by race for the 1970's statistic, but then collapse for the 1999 statistic?

Thank you for bringing this up. I have edited the text providing prevalence rates [Page 4, Lines 81-83]

I’m curious why 95% CIs are not presented for prevalence estimates in Tables 1 & 2. Is it the case that each cell differs statistically from every other cell in these two tables?
Thank you for asking. A p-value <0.05 conveys information on statistical significance and the 95% CI (with its lower and upper bounds) provides more information about the precision of the point estimate. Yet, both the p-value and 95% CI have limitations of uncertainty. I noticed that some studies are using both the p-value and 95% CI; however, in the contingency table analysis of my manuscript I chose to use only the p-value.

In the discussion, if the author choses to include the above references, it would be helpful to make comparisons of the current results to those above.

Thank you. I appreciate your suggestion and have included the references. [Page 13, Lines 272-276] [Page 15, Lines 320-322].

In the limitation section, is it possible to mention miscarriage/spontaneous abortion resulting from smoking and underreporting of smoking?

This is a good point. However, information on miscarriage/spontaneous abortion is not available in the NCHS Natality file of the US standard certificate of live births.

Given the current study only involved a single year, do we expect these patterns identified to be stable over time?

Thank you for your worthy suggestion. I have included this information in the conclusion section of the manuscript [Page 17, Lines 365-368]