Author's response to reviews

Title: Maternal post-natal tobacco use and current parental tobacco use is associated with higher Body Mass Index in children and adolescents: an international cross-sectional study

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Author's response to reviews: see over
Thank you for the opportunity to review manuscript 1157056712165504 entitled ‘Current parental tobacco use is associated with higher Body Mass Index in children and adolescents: an international cross-sectional study’. We have revised the manuscript in light of the comments below and, as requested, have uploaded a revised manuscript which we hope you will find satisfactory for publication. Our article word count is now 3,397. We hope that you will be accepting of this given the additions and changes we have made in the light of reviewer comments.

Our specific responses and amendments are listed below.

Reviewer: Amy Taylor
Reviewer’s report:
This paper looks at the association between parental smoking and offspring BMI during childhood and adolescence, using data from a large international cross-sectional survey. They find evidence for positive associations between parental smoking and offspring BMI, which is a pattern consistent with much of the reported literature. This paper does not really add anything in terms of determining causality, but is perhaps of interest in describing this pattern across a number of different countries. However, I am not sure that the authors have made the best use of these data to do this.
Answer: thank you for taking the time to review this manuscript. Our responses to your comments and subsequent revision to the manuscript follow:

Major compulsory revisions
1. Given the diverse range of countries included in this analysis, it seems odd to combine the estimates, without investigating whether there is evidence for heterogeneity between the associations in different countries. It would appear from the figures that there is quite a bit of heterogeneity. Therefore I am not entirely convinced by the validity of the combined estimates presented in Tables 1 and 2.
The authors state that “many of the centres were from middle and low income countries from which data on the associations between parental smoking and BMI have not been previously reported”. However, the way the data are presented makes it difficult to compare estimates between different countries. Could the authors perform the analyses separately by centre and then combine in a meta-analysis?
Answer. Thank you for your comment on this. This was an area of much discussion within the author group, as we found that we had to strike a balance between word count and figure restrictions in our presentation of the data. We considered that it would be unwieldy to present 31 and 72 centres in the children and adolescent figures respectively, thus we elected to present the figures as a country / centre analysis. However, in response to this point and to points 3 and 4, we have produced a supplementary table with the basic characteristics of the contributing centres in which we have incorporated BMI estimates by each centre with standard deviations. This will enable readers to see centre by centre information. Readers will be directed to supplementary Tables 1 and 2 in the first line of the results section.
2. Comparing associations from different countries could actually be informative with regards to causality. It is likely that confounding structures will differ between countries – e.g. smoking may be associated with high socioeconomic status in some countries but low socioeconomic status in others. Therefore if an association is consistently seen across countries, it provides stronger evidence for causality. See http://www.ncbi.nlm.nih.gov/pubmed/20587678 for an example of this. It would also be interesting to see if there is an interaction with GNI.

Answer. Unfortunately we do not have a measure of child-specific socioeconomic status available for this study. As you suggested, we tested for an interaction between GNI and smoking, which was present in the case of paternal smoking and GNI, but not for maternal smoking. As a result we have added the following statements to the manuscript:

In the section ‘statistical analysis’

“Because of an interaction found between paternal smoking and GNI, separate estimates were made for paternal smoking at each GNI level for both children and adolescents.”

In the results section for children we have included the following comments:

“Because of a significant interaction found between GNI and paternal smoking, estimates are given for paternal smoking by GNI category.”

“In high GNI countries children of smoking fathers had larger BMIs, than those with non-smoking fathers, while in low GNI countries children of smoking fathers had smaller BMIs than those of non-smoking fathers (Table1).”

In the results section for adolescents we have included the following comment:

“There was also an interaction between GNI and paternal smoking, so a separate analysis was done by GNI and paternal smoking.”

And adapted the first paragraph of the results for adolescent females:

“Data were available for 98 238 females. For those not exposed to parental smoking, estimated mean BMIs were 19.32 and 19.72 kg/m2 for ages 13 and 14 respectively. After controlling for country GNI, centre, individual fast food consumption, age and measurement type, there was an association between BMI and both maternal (+0.23kg/m2) and paternal (High GNI +0.18kg/m2 and Low GNI -0.05kg/m2) smoking (Table 1). “

In the discussion section, we have added the following comment:

“Our finding that children and adolescents of smoking fathers in high GNI countries had larger BMIs than those of non-smoking fathers might be consistent with the observation that both smoking behaviours and obesity have tended to become more concentrated in lower socioeconomic groups within high GNI countries,32,33 although we did not have each individuals’ socioeconomic information to confirm this theory. The lower BMI found in children of smoking fathers in low GNI countries is more difficult to explain, but with only a small number of centres and participants contributing to the analysis, this result may be spurious.”

We have now adjusted Figure 1 to list countries according to high and low GNI for the paternal smoking association graphs.

We have adjusted Table 1 to separate out the paternal smoking analyses by GNI.
Each centre is listed in the supplementary tables and sorted by the proportion of current maternal smoking and then according to GNI.

3. I don’t think that Figure 1 is very informative. I would prefer to see a forest plot with beta coefficients and 95% confidence intervals for each centre.

   Answer: As outlined in point 1, the inclusion all centres into what is already a complex figure would be unwieldy. However, we anticipate the inclusion of the supplementary table with this information will suffice.

4. It would be useful to have some basic characteristics of the contributing centres in supplementary material—e.g. N for each centre, country, GNI, %females.

   Answer: We are very pleased to be able to provide this information in Supplementary Tables 1 and 2.

Minor Essential Revisions
1. It would be good to have 95% confidence intervals presented in the tables and text rather than standard errors.

   Answer: We consider the standard error to be more helpful in this instance as it enables the reader to see that the estimates of BMIs for the analysed groups are of a similar precision. This cannot be so easily elicited from confidence intervals. The reader has the ability calculate confidence intervals if they so desire.

2. In table 1, the footnote refers to estimated BMIs for boys but I assume this should be “children”.

   Answer: the footnote refers to boys as the estimated BMI was slightly different for boys than girls.

Discretionary Revisions
5. Were data on any other potential confounders (apart from fast food consumption) of this association available?

   Answer: The environmental questionnaire covered a range of possible confounders. Our aim is to assess each potential confounder separately, and then to introduce them collectively into our model, this will be done in a separate paper.
Reviewer:
Susan B Torrey
Thank you for reviewing this paper. Our specific responses to your comments and manuscript revisions follow:

Reviewer’s report:
Major compulsory revisions:
Line 1: Title: This study is really about post-natal parental tobacco use since it includes data from the first year of life. Suggest changing title.

    Answer: Thank you we have changed the title to ‘Maternal post-natal tobacco use and current parental tobacco use is associated with higher Body Mass Index in children and adolescents: an international cross-sectional study’

Line 102: The data for this study come almost exclusively from the environmental questionnaire. This section should contain more information about how that questionnaire was developed.

    Answer: We have added the following into the first paragraph of the methods section of the manuscript: ‘The questionnaire was developed by the ISAAC steering committee to assess potential risk factors for the development of asthma in children that had been identified in previous research. Where possible, questions previously published in the literature were replicated in the EQ, otherwise the questions were developed by the ISAAC steering committee. The EQ was optional for all participating centres, and instructions were provided in the event centres wished to translate it into the local language.’

Line 128: For the dose-response relationship, were these arbitrary distinctions or were they based on response relationships for other established consequences of exposure to secondary smoke?

    Answer: These categories were decided on by the authors of the manuscript. We have adjusted the paragraph addressing this question as follows: ‘Each explanatory variable was examined separately for both age groups. To assess the presence of a dose-response relationship in the children, the number of cigarettes smoked per day by the parents were categorised for the purpose of analysis as 0, 1-9, 10-19, 20-29, and 30 or more’.

Line 130: How was fast food/burgers defined? Is this a standard definition?

    Answer: This is exactly as described in the environmental questionnaire and no further clarification was given.

Minor essential revisions:
Line 111: Please provide more information about adjustments that were made for measured vs reported height and weight. Since BMI is the primary outcome measure, the variability with which it was calculated introduced significant bias (not only recall for reported, but selection for which children had measurements performed). So it is important to give us as clear a sense of where these data came from as possible.

    Answer: We included a binary measure in the model to account for measured versus reported heights and weights. However, this measure is confounded by centre, and so any differences found between centres are difficult to interpret.
Are these thresholds for excluding data standard? It might be helpful to mention what percentage of the data were excluded.

Answer: We decided upon these thresholds in an attempt to exclude unlikely BMI’s, and were unable to establish as ‘standard’ to use. We have outlined the numbers excluded as a result of these thresholds in Supplementary Figure One and for clarity have added the numbers in the manuscript as follows:

- For children in each centre, those in the top and bottom 0.5% of weights and heights (n=1,391), and those with heights less than 1.0 metre were excluded (n=346). Children with BMI less than 9kg/m² and greater than 40kg/m² were excluded (n=215).
- For adolescents in each centre, those in the top and bottom 0.5% of weights and heights (n=3,712), and those with heights less than 1.25 metres (n=904) were excluded. Adolescents with BMI less than 10kg/m² and greater than 45kg/m² were excluded (n=360).

The limitations need to be more specific about reporting and selection bias.

Answer: We have added the following into the last paragraph of the discussion: ISAAC comprised a self-selected group of centres without intent to represent any population. The subset of ISAAC Centres that then decided to utilise the Environmental Questionnaire is also a self-selected group. This paper outlines the findings only in the sample that participated in the study, thus there is the possibility that these results are not representative of the population.

Should include that the questionnaire was optional.

Answer: this has now been clarified in the first paragraph of the methods section as outlined in the response to your first comment.

Yours sincerely

Dr Irene Braithwaite

Corresponding Author