Author's response to reviews

Title: Parental smoking and child poverty in the UK: An analysis of national survey data

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Author's response to reviews:

We thank the reviewers for their positive comments and suggestions for improvements. We have tried to address their comments in our revised manuscript, and hope that it is now acceptable for publication in BMC Public Health.

Reviewer 1 (George Thompson)

Reviewer's comment:

1) Line 21: Suggest rewording ‘interventions which effectively enable low income smokers to quit’

Authors’ response: We have reworded this sentence as suggested.

Reviewer’s comment:

2) Line 23: suggest ‘To our knowledge, the impact of parental smoking on child poverty in the UK has not previously …’. See previous work:


• Thomson et al. Tob Control. 2002 Dec;11(4):372-5;

[Likewise for p.10, lines 1-2: suggest ‘a contributor to UK child poverty that has not, to our knowledge, previously been quantified in this context’ ]

Authors’ response:

We thank the reviewer for bringing these papers to our attention and have included a short review of the existing international literature, including the suggested references.

We have reworded the sentence on p. 10 as suggested.

Reviewer’s comment:

2) P.5, lines 15-17: It is not clear why the assumption was made – as those in poverty are likely to have a greater smoking prevalence than those in the ‘routine and manual occupational socio-economic group’. Is there no other relevant survey data? Mention in Discussion?

Authors’ response:

We agree that smoking prevalence among people in poverty is likely to be higher than the routine and manual group, but prevalence data broken down into the detail necessary for this study are not available in groups defined in terms of poverty. We therefore use the routine and manual group as the best available alternative. We have clarified this in the methods section, and explained that this is a conservative assumption in relation to the main outcomes of the analysis. The implications of these assumptions are mentioned in the discussion (p. 11 lines 15-19).

Reviewer’s comment:

3) p.6: Lines 22-23: This is the reported cigarette usage – but reported usage is not dependable. Are there no objective ways to estimate usage for those in
poverty? If not – mention the need in Discussion

Authors’ response:

There are no objective measures available. We have added a comment to this effect to the discussion.

Reviewer’s comment:

4) p.8, lines 11-15: The heading leads one to expect a result about children in households with smokers, but you give a number of children ‘with adults who were married or civil-partnered’

Authors’ response: We have corrected this subheading.

Reviewer’s comment:

5) P.10, lines 5-6: As with the Abstract I suggest rewording ‘effective interventions which enable low income smokers to quit’.

Authors’ response: We have reworded this sentence as suggested.

Reviewer’s comment:

6) It would also be helpful if at least a reference could be made to the recent reviews of effective tobacco policy interventions to increase health equity:


And to the debate between comprehensive and upstream solutions to smoking equity, and individual intervention approaches: eg.


Authors’ response: We have now made reference to the health equity reviews (p. 11 line 2-3) and the debate around different approaches. (p. 11 line 4-8)

Reviewer’s comment:

7) There is also the unmentioned ethical and political aspect of government collecting tobacco tax from the households of these 1.5m children (perhaps estimate this revenue), but using less than 5% of the revenue in dealing with tobacco addiction. That is, using tobacco as a way to transfer money from the addicted poor to spending for the general population.
Authors' response: We have mentioned this issue on page 11, line 5.

Reviewer's comment:

8) The Further Research section needs to point out the problems of not having a smoking question in the UK census. Such a question would enable researchers to see children in households with smokers (not just with parents who smoke) and by household income.

Authors' response:

We agree that this is an attractive option and have added a comment to this effect in the discussion.

Reviewer's comment:

9) p.8, line 13 – the reference is not shown.

Authors' response: We have added this reference.

Reviewer's comment:

10) The references need work: Refs 1, 3 are incomplete, Ref 2 needs an author and city, etc.

Authors' response: We have amended the references where required.

Reviewer 2 (John Rijo)
Reviewer's comment:

1. The literature on tobacco expenditures exacerbating poverty has not been reviewed at all in the article. BMC public health being a journal with international readership, the survey of literature should not be restricted to U.K. alone.

Authors’ response: As suggested also by Reviewer 1, we have included a brief review of the international literature in the introduction.

Reviewer’s comment:

2. The organization of the article needs to be substantially changed for it to become less confusing to the reader.

Authors’ response: We have edited according to the reviewer’s suggestions below.

Reviewer’s comment:

3. Tables in the article are not self-explanatory and adds confusion.

Authors’ response: We have edited according to the reviewer’s suggestions below.

Reviewer’s comment:

4. The methods used are not clearly stated. Some calculations in Appendix need more clarity and should be moved to the method section as an appendix may not be there in the published paper.
Authors' response: We have edited according to the reviewer’s suggestions as described below.

Reviewer’s comment:

Page 3 line 2: How’s children defined in this study?

Authors’ response: We have added this definition to the paper (those under 16, and those aged 16-19 who are dependent (living with parents an in full time education or in unwaged government training)) (p. 5 line 14-16)

Reviewer’s comment:

Page 3 line 23: It has been estimated elsewhere in the world in few countries. Please review the literature carefully.

Authors’ response: We have added a short review of the international literature to the introduction.

Reviewer’s comment:

Page 3 line 25: The paper is not really providing an estimate of the cost of smoking in this context as claimed here. At best it provides a certain dimension of cost of smoking.

Authors’ response: We have rephrased this sentence to emphasise the uncertainty in our estimates. (Page 4 Line 7)
Page 4 line 19: “modified OECD equivalence scale”—citation needed.

Authors’ response: We have added in the relevant citation.

Reviewer’s comment:

Page 5 line 5: be specific on which part of the appendix you are referring to.

Items in Appendix are not arranged in the same order as it is referred to in the body of the paper. This is confusing.

Authors’ response: We have edited the relevant sections to ensure that we make it clear which part of the appendix we are referring too, and have re-ordered the appendices (some parts have been moved to the main text as described below).

Reviewer’s comment:

Page 5 line 25: you say the smoking prevalence in general population was 20%.

In Appendix 1A second column, however, you say the base smoking prevalence is 33%. How do you reconcile these?

Reviewer’s comment: The reference to smoking prevalence in the general population is made only to provide evidence of variation in smoking prevalence by marital status (Page 6 line 9-10). We have explained that the base smoking prevalence used for our analysis is 33%, based on an assumption that smoking prevalence in households in poverty would be the same as that in households in the routine and manual occupational socio-economic group. We have moved a table from the appendix into the main text to help clarify our methods for estimating smoking prevalence by marital status in poor individuals. (now Table 1)
Page 6 line 2: be specific on which part of the appendix you are referring to. In Appendix table 1A it is not clear how you came up with the weights in the third column. However you did it, those weights are distorting the base smoking rate substantially to unacceptable levels which substantially alters the numbers you are finally estimating.

Authors’ response: We have moved table 1A from the appendix to the main text to aid clarity (now Table 1), and added a footnote to explain how the weights were obtained. The weight is based on estimates of smoking prevalence from the general population (20%) and by marital status (27%, 14% and 33% in single, married or cohabiting) respectively. While it is a limitation that we do not have data on smoking prevalence by marital status in the low income group, given this variation in the general population, we do not feel that we are distorting the base smoking by using this method. This limitation is acknowledged in our discussion (Page 11 line 21-24).

Reviewer’s comment:

Page 6 line 5: It is not clear how the weights were obtained.

Authors’ response: See previous comment. We have moved the relevant table from the appendix to the main text so that this is clearer, and added a footnote to explain how the weights were obtained.

Reviewer’s comment:

Page 6 line 6-9: When you apply a certain smoking rate of single parent to the total number of children in these households how do you account for the fact that these houses consist of houses with different numbers of children, some with a single child, some with two children, some with 3 or even more. This is not clear here. There are some numbers in the tables for poverty among children with different household sizes. However, it is not clearly explained how they were arrived at.
Authors’ response: We had tried to make our calculations clear in the appendix; however we acknowledge that this was not sufficiently clear. We have added some rows to table 2 which, particularly when read in conjunction with Appendix 1, we hope provides sufficient information.

Reviewer’s comment:

Page 6 line 12: What was the data used by Jarvis et al.? What was its sample size? Was it a representative data? Are you right in using these proportions to your data? None of this is clear in your discussion. Again you refer to an appendix in the next sentence without pointing to the exact location within the appendix.

Authors’ response: We have addressed these points on Page 6.

Reviewer’s comment:

Page 7 line 5-7: But the average prices typically paid by the poor may be even lower right? So if you apply the averages from the entire population to estimate the expenditure on cigarettes by poor you may be highly over estimating it.

Authors’ response: This is correct; unfortunately we have no estimate of the price of cigarettes smoked by our study population. We have added an acknowledgment of this point to our methods section.

Reviewer’s comment:
Page 7 line 8-9: Are you sure that of the 73% of female and 59% males who smoke manufactured cigarettes none of them smoke HRT? Are these figures exclusive? Are there none who smoke both cigarettes and HRT?

Authors’ response: We have combined those that smoke only manufactured cigarettes and those who also smoke HRT but mainly smoke manufactured cigarettes. We have added this information to the text (page 7 line 22-24).

Reviewer’s comment:

Page 7 line 21-22: It is not clear why a 70% cut off was selected here. Why would you rather not deduct the cigarette expenditure from the entire sample above the 60% of median? It is not clear why this artificial cut off is necessary.

Author’s response:

The focus of our study is one those below the poverty line and those drawn below it by expenditure on tobacco. Based on our cost calculations, no households above 70% of the median income will be drawn into poverty by tobacco expenditure, and it is therefore appropriate to focus on this group. We have added a sentence to explain why we have used this cut-off (Page 8 line 15-16).

Page 8 line 4-6: This is problematic. The assumption here is that every two-smoker households between 60% and 70% of the median income has same expenditure on smoking. This is quite unrealistic as the smoking rates (frequency) can vary substantially among the smokers within this group. The previous studies that calculated the number of those drawn into poverty after
accounting for smoking expenditures deducted the actual amount spent on cigarettes by each of the households from their present income and checked how many of those had incomes below the poverty level income after such subtraction.

Authors’ response: The reviewer is of course correct to suggest that subtracting actual expenditure from present income at the household level is the ideal way to conduct this analysis; unfortunately, we do not have access to data on expenditure on smoking at the individual household level. We have therefore applied our estimates of average expenditure, while emphasising that our estimates are approximate. We hope to conduct more accurate analyses with more detailed data in the future.

Reviewer’s comment:

Page 8 line 13: Error Reference

Authors’ response: This reference is correct.

Reviewer’s comment:

Appendix 3 Step 2 f: Did you mean total number of children or smoking parents

Authors’ response: We have corrected this to read ‘number of children’.