

Author's response to reviews

Title: Population tobacco control interventions and their effects on social inequalities in smoking: placing an equity lens on existing systematic reviews

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Responses to BMC peer reviewer comments

Thank you for the opportunity to revise this manuscript in response to the reviewers’ comments, which we have addressed as set out below. Revisions to the manuscript are marked in yellow.

Reviewer 1: Lindsay Stead

Major compulsory revisions: None

Minor discretionary revisions:

General comments: Does the manuscript adhere to the relevant standards for reporting and data deposition?

Yes, but I would like to know the references for the 6 reviews that addressed a relevant intervention but were excluded because they did not mention any socio-demographic data.

These have been added (references 35 to 40).

LS 1a. Abstract: In the first sentence of the background the authors use the term upstream interventions, but later the term population-level interventions. I suggest using only the latter term in the abstract.

This has been changed to population-level interventions.

LS 1b: Abstract: it would be helpful if space allows in the abstract to list all the types of intervention for which reviews were found. Only price, youth access laws and bans are explicitly mentioned in the abstract which may hinder retrieval.
These have now been added.

LS 2: Unless there is a word limit for the abstract, more detail on the 19 included reviews would also be helpful.

More detail on the reviews and the specific interventions has been added.

LS3: Results page 9 line 16, it would be useful to specify here the reference numbers for the 3 reviews that specifically addressed socio-demographic differences although it does become apparent later.

These references have been added.

LS 4: In relation to check point 6 (Are limitations of the work clearly stated?) the authors consider that their findings support the further use of systematic reviews for addressing questions about equity effects of interventions. I agree that a systematic approach has to be part of the process for attempting to identify whether there are existing data to answer these questions. But as they themselves note, the function of a systematic review in this context will probably be to identify primary studies that, individually, may have relevant data. It is less likely that a systematic review will add value by pooling or synthesising results from multiple studies, which might be done in different sociodemographic settings, do not seem very likely to be able to answer the question about equity effects. Differences between studies in the populations studied will be confounded by multiple other differences in design and intervention characteristics. The critical issue is whether individual studies have included sufficiently varied populations and been large enough to report suitably powered sub-group analyses. The authors do, I think, make this point themselves but it could perhaps be more explicit.

We accept this point, but this is addressed in the first paragraph on page 13 in which it is stated that “these effects are generally presented as averages across the entire population, and existing systematic reviews tend not to indicate whether the effects vary for different sub-groups”. This is further addressed in the sentence “this depends on the availability of such data in the primary studies, and it is likely that in many of these data on distributive effects were not reported, or were not analysed”.

Reviewer 2: Scott Tomar

Major compulsory revisions: None.

Minor essential revisions

ST 1: It would be useful if this paper included a table that summarized the findings of each of the tobacco outcomes, by socio-demographic group (where possible).

The authors accept that a table that summarises the main tobacco outcomes for each review by socio-demographic group would be useful, and have in response
to this revision attempted to produce such a table. However, it has not been possible to achieve this within the size constraints of a table being added to the main text of the manuscript. This is due to the fact that some of the results are reported at the overall review level (i.e. the entire review has included studies of population-level interventions), whilst others are based on one or two primary studies within the reviews. This leads to reliance upon ‘vote counting’ the number of studies within the reviews for which there was a significant result and those for which there was a non-significant difference. Given the number of studies within the reviews, and the number of different outcomes assessed within each of the reviews, it is unfortunately impractical to attempt to summarize the results from all of the reviews within one table.

ST 2: The table included as a supplementary file does not use categorizations consistent with those used in the text (i.e. increasing price of tobacco products; restricting young people’s access to tobacco products; and restricting or banning smoking). The table includes six systematic reviews categorized a “community-based programmes”, which is not informative in a review in which all included systematic reviews were supposed to be on community-based interventions. Two systematic reviews were categorized in the table as focusing on “reductions in ETS”, but the text (page 11) describes four reviews on that outcome.

It is stated in the text on page 7, that where reviews covered more than one type of intervention, the dominant area determined its classification. This has additionally been added below the table in the supplementary file. It has also been clarified below the table that although reviews categorised as addressing “community-based programmes” included primary studies that assessed youth access restrictions, increasing the price of tobacco products, or restricting or banning smoking, these reviews were actually focussed on multi-component community-based interventions some facets of which may not have been a population-level intervention. Likewise, for the two reviews (reference numbers 24 and 26) that assessed smoking bans and restrictions and reported on exposure to environmental tobacco smoke, the dominant area of each of the reviews was to assess the effects of smoking bans or restrictions in the workplace on smoking prevalence and consumption, but some of the included primary studies additionally reported on levels of environmental tobacco smoke. Both of these reviews were therefore categorized as assessing smoking bans and restrictions. A footnote has been added under the table to clarify that these reviews included primary studies that additionally reported on levels of environmental tobacco smoke.

ST 3: Although the text identifies 4 reviews that examined the effects of bans or restrictions on exposure to ETS (refs. 24, 26, 33, 34), it then cites a different review (23) in discussing the findings.

This has been corrected to read 24.

Discretionary Revisions
ST 1: The authors use the term “gender” and “sex” interchangeably, but the terms “gender” is incorrect in this context. Gender is a sociological construct, where nearly all underlying original studies included in the reviews were actually measuring the biological variable of sex.

“Gender” has been amended to “sex” throughout the manuscript.

ST2: Page 5, bottom. More correctly: …”We included reviews that (rather than “which”) were “borderline systematic ……”

This has been amended to read “that”.

ST 3: Page 5, bottom. Actually it appears that the aim is to determine what could be inferred rather than what could be deduced from existing reviews. This paper does not use deductive reasoning.

This has been amended to read “inferred”.

ST 4: Page 12. Should read “there were (rather than was) little available data”.

This has been amended to read “were”. --