Author's response to reviews

Title: Use of traditional cooking fuels and the risk of young adult cataract in rural Bangladesh: a hospital-based case-control study

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Manuscript: Use of traditional cooking fuels and the risk of young adult cataract in rural Bangladesh: a hospital-based case-control study

Responses to Reviewers’ Comments

Reviewer #1 Version 2

Major compulsory revisions
1. While the addition of information in Table 4 has improved understanding of the distribution of fuel use in the study, there are striking differences in fuel use between men and women. For example no men use cow dung with wood and only 2 men use cow dung in combination with other fuels. The majority of men report no exposure to any cooking fuels at all. Because of these differences there is inconsistency in the reference groups in the ORs for men and women in table 4. For women the reference group is those who use only wood, while for men the reference group is those who use no fuels. However the majority of the analyses shown in other tables combine the results for men and women and include those men who used no fuels with the never group of other types of fuels. I suggest that the ORs should be removed from table 4 and the data provided only for descriptive purposes. Since so many men do not report any cooking fuel exposure (reflecting the lack of cooking) the analyses should also be run separately for women only, and reported in an additional table.

It is true that in the earlier Table 4 the reference groups for males and females are not the same. Although the authors do not think this is a major problem (the cases and controls were matched on sex so that the association between sex and case status could not be demonstrated anyway), they have followed the reviewer’s suggestion of removing the odds ratios and displaying the information for descriptive purposes only.

Furthermore, because of the different patterns of exposure to cooking with the various biomass fuels (different sets of combinations and differing levels of exposure), models for ever use of the fuels have now been constructed for males and females separately and each shown in the new Table 5. The comparator categories for each fuel variable in the female model (i.e., rice straw and cow dung) then is non-use of that fuel but accompanied with use of wood/dry leaves. In the male model, the comparator category is non-use of wood/dry leaves or any other biomass fuel. (The few males who reported exposure to rice-straw and/or cow-dung were omitted from this model as their numbers were too small to yield any meaningful values.) However, when modeling lifetime exposure, the full set of subjects was initially included as the different exposure patterns and levels should be largely accommodated within the ordinal variables for cooking fuel exposure. Nevertheless, as only males would be represented within the no exposure to wood/dry leaves category, an additional analysis was done including only females. Here, of course, there is no category for “no exposure” to wood/dry leaves. As Table 6 shows, there is essentially no difference from the all-subjects model in the associations for lifetime exposure to rice straw and to cow dung.

As a further point on this in the Discussion the authors should consider their exposure based on personal use of cooking fuel since the men in the “no fuel group” clearly would
have lived in households with cooking fuels. The difference between personal and household exposures should be discussed.

Additional comment is given in the Discussion regarding the lack of marked differences in pollution levels between kitchen and living rooms in poor Bangladeshi houses. However, as we lacked information on what the non-exposed subjects generally did when cooking was going on in their house (they may have been in the house or away from the house) it is difficult to gauge how much of an effect exposure in the household, as opposed to exposure while actually cooking, would have had on the estimated associations.

On page 14 the authors state that sex was not a modifying effect in the analysis by fuels but it would be difficult to test this given the differences in fuel use. The authors should explain how they did this or the statement should be modified.

The adoption of separate sex models for ever use and the additional model for females alone for lifetime exposure hopefully renders the associations more explicit.

2. The results in the text and tables (including the abstract) should include the exact exposure and comparator groups, for example since there are no people who only use cow dung, the odds ratios are based on the use of cow dung with either wood or with wood and rice straw compared to those who use only wood or wood and rice straw or none.

As in the above response, the separate models shown in Table 5 for each sex and the additional model shown in Table 6 for the female subgroup should resolve the confusion. Since the models are multivariate models, it is perhaps not strictly appropriate to refer to exposure and comparator “groups”; rather the model displays categories of exposure, with their association with outcome being adjusted for other covariates in the model. Thus, in Table 6, the associations of the various magnitude categories of lifetime exposure to each biomass fuel are already (at least partially) adjusted for differences among these magnitude categories in the exposure levels to the other biomass fuels.

3. Since the group in this study who use cow dung also use fuels with increased ORs (i.e. wood or rice straw), it is likely that the results are biased in some way by incomplete adjustment for wealth or some other factor due to ownership of buffalo. The authors do address this possibility in their discussion but a stronger statement on the potential bias should be included in the conclusions of the Abstract. The last sentence of the Conclusions should be removed because the potential biases do not warrant this sentence.

The conclusions of the Abstract have been revised to summarize the findings (i.e., associations), rather than implying effects on risk of developing cataract.

4. References. The authors have too many references on factors that are not central to the study question or are set in very different populations. There are too few references on the health effects of biomass fuels, especially studies carried out in Bangladesh or northern India where biomass fuels use is common.
Some examples are:

Bikis et al. Indoor air pollution from particulate matter emissions in different households in rural areas of Bangladesh. Building and Environment 2008.


I suggest they carry out a more in depth review of this area which would better inform the study discussion.

Some relevant information from these references has been incorporated into the Discussion.

Reviewer #3 Version 2

The authors have improved their paper in light of the comments from the reviewers. There are still steps needed to be taken before it is of a standard suitable for publication, in my view.

Major compulsory revisions
Some of these are reiterated from my first review, as they had not been incorporated:

1. The discussion is still much too long. The details on other risk factors for cataract in this study is not really relevant and can be substantially reduced.

Those parts of the former Discussion section have now been considerably shortened and the numbers of references substantially reduced.

2. There is a fundamental difference between risk (i.e. cumulative incidence) and odds, and these terms cannot be used inter-changeably. In particular, your conclusion in the abstract is not correct, i.e. "the use of rice straw increases risk... the use of cow dung fuel... reduces the risk". All that can be concluded from a case-control study is that the odds of exposure is higher or lower among cases compared to controls. This MUST be corrected in the abstract, and in the discussion.

The authors accept that the implication of “risk” from a case-control design is an over-interpretation. The text has been revised in several places in the Abstract and Discussion to refer only to “association” rather than “effect” or “risk”.

3. The final conclusion is still too strong. Further studies and biological support needs to be obtained before this recommendation can be made. Perhaps the authors can rephrase this to say "may be a useful approach, but further evidence is needed".

The final conclusion has been revised to refer to “association” rather than “risk”.
4. It is not usual to present p-values to 3 d.p. unless the p-value is less than 0.01. I would suggest giving all p-values to 2 d.p., and those that are <0.01 to 3 d.p. (or more as appropriate).

All p-values have been changed to comply with the reviewer’s suggestion.