PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (**http://bmjopen.bmj.com/site/about/resources/checklist.pdf**) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Coronary Heart Disease, Hypertension and Use of Biomass Fuel among Women: Comparative Cross-sectional Study.
AUTHORS	Fatmi, Zafar; Ntani, G; Coggon, David

VERSION 1 - REVIEW

REVIEWER	Sayali Mukherjee Amity University Uttar Pradesh Lucknow India
REVIEW RETURNED	11-Dec-2018

GENERAL COMMENTS	1. Strengths and limitations: only strengths mentioned. 2. Introduction: Page 3, Line 8: health effects of biomass fuel combustion – Authors may cite many recent references available for respiratory outcomes and COPD. 3. Introduction: Page 3 line 14 and 18, the phrase "published only as abstract" is not very appealing. 4. Introduction: Page 3 Line 15-18, authors have mentioned about reports from China but the references cited 14,15 are statistical softwares mentioned in the reference list of the manuscript. 5. Introduction: Page 3 Line 18-20, "a cohort study in Bangladesh found no significant association between use of solid fuel for cooking or heating and mortality from ischemic heart disease, cited reference 16 which deals with reports from Pakistan. Author's should clarify. 6. Introduction: Page 3 Line 20-23, cited reference 17 which is not published yet as per reference list its "Manuscript under preparation." Unpublished articles cannot be cited. 7. Subjects selected were women >40 years of age. Authors should specify an age limit for the subjects. The upper limit is not mentioned. Moreover, lots of complications arise with increasing age, especially in women approaching menopause or post-menopausal. 8. The study design should be explained logically. The basis of selection of study villages should

	be explained. 9. No references cited for Questionnaire and study measurement criteria e.g. scale of socio-economic status or BMI. 10. The representation and explanation of results need improvement. 11. Table 1, 2 and 3- Only percentages mentioned, p value should be included. The tables are too elaborate. Long-term users may be split into a different table. 12. The authors have mentioned that the control population (natural gas users) previously used biomass fuels. How is the conclusion on the outcome justified? 13. Discussion : Page 11, line 36-38 not clear. Reference [17] again cited in line 39 which is unpublished. 14. Discussion should be modified –improved and concised.

REVIEWER	DR SANDRA OFORI UNIVERSITY OF PORT HARCOURT, NIGERIA
REVIEW RETURNED	14-Jan-2019

GENERAL COMMENTS	Fatmi and co-authors present cross-sectional data on the association between biomass use and coronary heart disease and hypertension. The article is well written and adds to the growing body of knowledge on the associations between biomass fuel and cardiovascular disease. Minor comments: 1. In the mutually adjusted model why was the significance level reduced to p<0.1? 2. Typo on Page 6, line 8 (change 'WHP' to 'WHR)

REVIEWER	Kevin Mortimer Liverpool School of Tropical Medicine, UK
REVIEW RETURNED	20-Jan-2019

GENERAL COMMENTS	Overall This paper addresses an important under-researched topic and as such has some merit however the major methodological limitations unfortunately mean that the addition to our state of knowledge about the association between biomass smoke exposure and cardiovascular disease outcomes is quite limited. At the same time there is important debate currently around whether exposure to smoke from biomass fuels is really as harmful as has been claimed and this study does present data relevant to this debate - in this study the risk of CHD is much more strongly associated with affluence than biomass fuel use (which is associated with poverty). General Could be written much more concisely - the paper has the feel of being a thesis chapter that has been condensed down to make a paper but hasn’t been condensed quite far enough yet. There is quite a bit of repetition (e.g. description of methodology in both the introduction and methods) which needs to be taken out. Recommend sticking more strictly to having only introduction in the introduction section, methods in the methods section and results in the results section.

	Major My major concerns relate to the methodology which clearly impact substantially on the results and discussion. At this stage there is nothing that can be done about this other than include more discussion of these limitations. There is a big problem relating to the definition of the two main categories as important fuels have not been considered - e.g. charcoal and crop residues - as being in the biomass user group. The non-biomass user group is all women who do not use firewood or cow dung for cooking but this potentially includes some very dirty-burning fuels including coal and kerosine. Another concern in relation to the exposed/non-exposed group is that other sources of smoke exposure (e.g. burning of fuels for heating, lighting (esp. kerosine), mosquitos coils) have not been considered properly which may have led to exposure misclassification. Even after accepting the definition of exposure adopted for this study, the main division into exposed/not exposed if based on current short-term exposures which is unlikely to be very meaningful for outcomes that are determined by exposures over much longer periods (decades). Although the long-term user/non-user groups are likely more meaningful, many of the non-biomass users had a substantial (decades) long previous history of exposures. Overall therefore I'm concerned that there is a fundamental problem with the definition of exposures and the exposed/non-exposed groups that could in itself explain the main study findings. In contrast, the authors seem more concerned about limitations in relation to the definition of the outcome measures which I would consider more robust - especially those that were directly measured. Minor P1 line 54 typo 'Out' should be 'Our' P4 line 17 I'm sure there is a better form of words than 'women [who are] mentally incompetent' P4 lines 18 & 19 the justification for exclusion of women with advanced kidney or liver disease is not clear - why exclude these and not other advanced diseases - why exclude them at all. P4 line 60 BMI <25 is not normal as there are categories of underweight included here.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Sayali Mukherjee

Institution and Country: Amity University Uttar Pradesh Lucknow, India Please state any competing interests or state 'None declared': None declared

1. Strengths and limitations: only strengths mentioned.

RESPONSE:

Thank you for highlighting this error. The limitations of the study were covered at some length in the Discussion, but that was not reflected in the Summary at the beginning of the paper, which we have now revised substantially.

2. Introduction: Page 3, Line 8: health effects of biomass fuel combustion – Authors may cite many recent references available for respiratory outcomes and COPD.

RESPONSE:

The focus of our paper is the relationship of household air pollution to coronary heart disease, and the well-established link to respiratory disease is noted only by way of background. In support of that link, we cite several reviews, which we think provide adequate documentation. We are not aware of any more recent investigations that call the association into question.

3. Introduction: Page 3 line 14 and 18, the phrase "published only as abstract" is not very appealing.

RESPONSE:

We highlighted that one of the studies was published only as an abstract, because that meant it could not be fully evaluated. However, we agree with the reviewer that the wording of this section of the Introduction is not ideal, and on reflection we think that most of the material is unnecessary since it is covered by the systematic review which we cite (reference 11), or is better placed in the Discussion. We have therefore modified the text accordingly.

4. Introduction: Page 3 Line 15-18, authors have mentioned about reports from China but the references cited 14,15 are statistical softwares mentioned in the reference list of the manuscript.

RESPONSE:

We apologise for this error, which occurred because a section of text from an earlier draft of the paper was re-introduced in the Introduction without changing the references. We have now amended the text, and corrected the referencing accordingly.

5. Introduction: Page 3 Line 18-20, “a cohort study in Bangladesh found no significant association between use of solid fuel for cooking or heating and mortality from ischemic heart disease, cited reference 16 which deals with reports from Pakistan. Author’s should clarify.

RESPONSE:

We apologise. Again, this occurred because a section of text from an earlier draft of the paper was re-introduced in the Introduction without changing the references. We have now revised the text, and corrected the referencing accordingly.

6. Introduction: Page 3 Line 20-23, cited reference 17 which is not published yet as per reference list its “Manuscript under preparation.” Unpublished articles cannot be cited.

RESPONSE:

As the paper cited has not yet been accepted for publication, we have removed it from the reference list, and added a note where it is mentioned in the text (in the Discussion) that it is a manuscript under preparation.

7. Subjects selected were women >40 years of age. Authors should specify an age limit for the subjects. The upper limit is not mentioned. Moreover, lots of complications arise with increasing age, especially in women approaching menopause or post-menopausal.

RESPONSE:

We limited our investigation to women ≥40 years of age because the prevalence of coronary heart disease at earlier ages is relatively low, and including younger women would have made the study less efficient. However, there was no upper age limit. As reported in the paper, our analyses adjusted for age. We acknowledge that among the women, some may have been pre-, and some post-menopausal. However, we think it unlikely that after adjustment for other covariates, age at menopause will have varied sufficiently between users and non-users of biomass to cause important residual confounding of associations between biomass and coronary heart disease when adjustment was made also for age.

8. The study design should be explained logically. The basis of selection of study villages should be explained.

RESPONSE:

The methods used in the study are set out systematically under the sub-headings. As per STROBE guidelines, we cover: the study setting; recruitment of households and potential subjects; inclusion criteria for participants; the questionnaire, examination and measurements that were used to collect data on participants; the specification of outcome measures; the specification of the categories of exposure to biomass; methods of statistical analysis; justification for the sample size (which is contingent on the specification of exposure and outcome variables and the methods of statistical analysis); and ethical approval. The only aspect of this sequence which might be considered illogical is placing the specification of outcome measures before that of the main exposure variables, and we have now reversed the order of those two sections.

The basis on which the study villages were selected is already explained in the first paragraph of the Methods ("These were selected to give a mix in the fuels used for cooking within the study sample. Some of the villages had been supplied with natural gas for at least 10 years, whereas in others biomass fuel (wood and/or cow dung) was still being used."). We submit that this should be sufficiently explicit.

9. No references cited for Questionnaire and study measurement criteria e.g. scale of

socio-economic status or BMI.

The questionnaire, which was developed specifically for the study, had not been used in any previous investigation, and is therefore described at some length. We reference the Rose angina questionnaire from which the section used to ascertain angina was derived, and the Minnesota Code Manual of Electrocardiographic Findings, which was used to classify ECG traces. We did not derive a single scale of socioeconomic status, but describe how it was characterised using five variables ("the literacy of the participant (no vs. any literacy), the type of employment of her father during her childhood (manual or non-manual), the ownership and construction of her house ('pucca' i.e. made of concrete walls and roof or 'katcha/semi-pucca' i.e. made fully or partially of thatched walls and roof), the income level of the household, and the number of household assets owned from a list of seven"), each of which was included separately in the analysis. We do not think it should be necessary to reference definitions for measures such as BMI or waist/hip ratio, which are standard and well known.

10. The representation and explanation of results need improvement.

RESPONSE:

We assume that this refers to the specific points that follow, and we respond to them below.

11. Table 1, 2 and 3- Only percentages mentioned, p value should be included. The tables are too elaborate. Long-term users may be split into a different table.

RESPONSE:

Table 1-3 are descriptive, and (in accordance with STROBE guidelines) provide background information on exposure variables and covariates whose associations with coronary heart disease outcomes were analysed in Supplementary Tables 4 to 7 and Table 4. The statistical significance of differences in the distribution of covariates between users and non-users of biomass is not relevant to our study question (which concerned the association of coronary heart disease and hypertension with use of biomass for cooking), and does not inform the interpretation of our findings. Therefore, presentation of p-values would be inappropriate.

The optimal formatting of tables is to some extent a matter of personal taste. We have tried to minimise their number while conveying the key information, and without making any individual table too large or complicated. However, if the Editor would like us to format them differently, we would be pleased to do so.

12. The authors have mentioned that the control population (natural gas users) previously used biomass fuels. How is the conclusion on the outcome justified?

RESPONSE:

Some intervention studies have suggested that reductions in exposure to pollutants from cooking with biomass can produce changes in blood pressure and on ECG in the relatively short-term (a point on which we have now expanded in our Discussion). Thus, it was not unreasonable to look for differences in such outcomes between current (i.e. for at least the past year) users and non-users of biomass for cooking. Moreover, recognising that some effects of exposure might be longer term, we also compared longer term users and non-users of biomass. Even for this comparison we found no association with coronary heart disease outcomes. Nevertheless, we are careful not to conclude that no association exists. Rather, we say “it could be that an effect was missed because most who were not currently using biomass for cooking had used it in the past, and risk remains elevated for many years after last exposure”, which is exactly the point that the reviewer is making.

13. Discussion : Page 11, line 36-38 not clear. Reference [17] again cited in line 39 which is unpublished.

RESPONSE:

See response above. We now refer to this unpublished material in the text without including it as a reference.

14. Discussion should be modified –improved and concised.

RESPONSE:

It is difficult to respond to non-specific comments of this sort. Our experience is that when discussion is brief, reviewers ask for further clarification on points that have been omitted. It is clear, for example, that in the current paper, more discussion is wanted on the validity of the exposure classification (see below). We would be pleased to shorten our Discussion if the Editor wishes, but in doing so, it would be helpful to have guidance on which specific aspects of the Discussion are considered superfluous.

Reviewer: 2

Reviewer Name: DR SANDRA OFORI

Institution and Country: UNIVERSITY OF PORT HARCOURT, NIGERIA Please state any competing interests or state 'None declared': NONE DECLARED

Fatmi and co-authors present cross-sectional data on the association between biomass use and coronary heart disease and hypertension. The article is well written and adds to the growing body of knowledge on the associations between biomass fuel and cardiovascular disease.

Minor comments:

1. In the mutually adjusted model why was the significance level reduced to $p<0.1$?

RESPONSE:

The threshold p-value of 0.1 was used to select potentially confounding covariates for inclusion in the final regression models. As is often the practice, we opted for a relatively low level of significance to reduce the possibility that important confounders were omitted from those models.

2. Typo on Page 6, line 8 (change 'WHP' to 'WHR')

RESPONSE:

Thank you. We have made the correction.

Reviewer: 3

Reviewer Name: Kevin Mortimer

Institution and Country: Liverpool School of Tropical Medicine, UK Please state any competing interests or state ‘None declared’: None.

Overall

This paper addresses an important under-researched topic and as such has some merit however the major methodological limitations unfortunately mean that the addition to our state of knowledge about the association between biomass smoke exposure and cardiovascular disease outcomes is quite limited. At the same time there is important debate currently around whether exposure to smoke from biomass fuels is really as harmful as has been claimed and this study does present data relevant to this debate - in this study the risk of CHD is much more strongly associated with affluence than biomass fuel use (which is associated with poverty).

General

Could be written much more concisely - the paper has the feel of being a thesis chapter that has been condensed down to make a paper but hasn’t been condensed quite far enough yet.

There is quite a bit of repetition (e.g. description of methodology in both the introduction and methods) which needs to be taken out.

Recommend sticking more strictly to having only introduction in the introduction section, methods in the methods section and results in the results section.

RESPONSE:

We have shortened the Introduction as suggested, removed unnecessary duplication between the text of the Results and the tables, and moved one paragraph from the Methods to the Results.

Major

My major concerns relate to the methodology which clearly impact substantially on the results and discussion. At this stage there is nothing that can be done about this other than include more discussion of these limitations.

There is a big problem relating to the definition of the two main categories as important fuels have not been considered - e.g. charcoal and crop residues - as being in the biomass user group. The non-biomass user group is all women who do not use firewood or cow dung for cooking but this potentially includes some very dirty-burning fuels including coal and kerosine.

Another concern in relation to the exposed/non-exposed group is that other sources of smoke exposure (e.g. burning of fuels for heating, lighting (esp. kerosine), mosquitos coils) have not been considered properly which may have led to exposure misclassification.

RESPONSE:

In specifying our exposure groups, we focused on the major determinant of women's exposure to PM2.5, which was whether they used firewood or cow dung for cooking. In the community studied, these were the two main types of biomass that had been used for that purpose (we now make this clearer in our description of the study setting). We recognise that there were other potential sources of particulate pollution in the air of participants' homes, including the use of kerosene for cooking, use of biomass for heating, and environmental tobacco smoke. We therefore collected data on the prevalence of each of these in our study groups. As can be seen from our tables, kerosene was little used (3.7% of biomass users and 6.3% of non-users), while use of biomass for heating and exposure to environmental tobacco smoke were both more prevalent among the biomass users. It follows that any confounding effects would be positive rather than inverse, and thus could not explain the absence of associations with use of biomass for cooking. Furthermore, as we mention in what is now paragraph 3 of our Discussion, a linked air monitoring study demonstrated substantial differences in the levels of particulate matter in kitchens according to our classification of exposure. In houses using biomass for cooking, the mean 24-hour average PM2.5 concentration was 531 µg/m3, with a median of 136 and IQR 34-615. Corresponding concentrations in houses not using biomass for cooking were 69.9, 24.2, and 13.5-53.3 µg/m3. To put these figures in context, the EPA air quality standard for 24-hour average PM2.5 in outdoor air is 35 µg/m3. Our classification of exposure was not perfect (epidemiological studies never are), but it distinguished groups with large differences in exposure, and as such was fit for purpose.

We have now expanded our discussion of contrasts in exposure to make this clearer.

Even after accepting the definition of exposure adopted for this study, the main division into exposed/not exposed if based on current short-term exposures which is unlikely to be very meaningful for outcomes that are determined by exposures over much longer periods (decades). Although the long-term user/non-user groups are likely more meaningful, many of the non-biomass users had a substantial (decades) long previous history of exposures.

RESPONSE:

We recognise that at least some of the possible cardiovascular effects of indoor air pollution could depend more on long-term cumulative exposure than recent intensity of exposure. That is why we distinguished subsets of long-term users and non-users of biomass. However, we do not share the reviewer's confidence that recent exposures are irrelevant. We know from time series studies of outdoor air pollution, that the frequency of cardiovascular events varies from day to day according to levels of particulate pollution. Moreover, trials have suggested that interventions to reduce household air pollution from use of biomass for cooking can produce reductions in blood pressure and changes in ECGs (two of the outcomes that we investigated in our study) over the relatively short-term [1-3].

We have expanded our discussion to make this point.

Overall therefore I'm concerned that there is a fundamental problem with the definition of exposures and the exposed/non-exposed groups that could in itself explain the main study findings. In contrast, the authors seem more concerned about limitations in relation to the definition of the outcome measures which I would consider more robust - especially those that were directly measured.

RESPONSE:

See responses above. We remain concerned about the accuracy of the outcome measures, although care was taken to ensure their validity as far as we were able.

Minor

P1 line 54 typo 'Out' should be 'Our'

RESPONSE:

Thank you. We have corrected this error.

P4 line 17 I'm sure there is a better form of words than 'women [who are] mentally incompetent'

RESPONSE:

Thank you. We have now omitted this sentence (see below).

P4 lines 18 & 19 the justification for exclusion of women with advanced kidney or liver disease is not clear - why exclude these and not other advanced diseases - why exclude them at all.

RESPONSE:

These were specified in the original protocol, but in practice did not lead to additional exclusions. We have now omitted the sentence.

P4 line 60 BMI <25 is not normal as there are categories of underweight included here.

RESPONSE:

Thank you for pointing this out. We have modified the nomenclature here and elsewhere.

REFERENCES:

1. McCracken J, Smith KR, Stone P, Diaz A, Arana B, Schwartz J. Intervention to lower household wood smoke exposure in Guatemala reduces ST-segment depression on electrocardiograms. Environ Health Perspect 2011;119:1562-8.

2. McCracken JP, Smith KR, Díaz A, Mittleman MA, Schwartz J. Chimney stove intervention to reduce long-term wood smoke exposure lowers blood pressure among Guatemalan women. Environ Health Perspect 2007;115:996-1001.

3. Alexander D, Larson T, Bolton S, Vedal S. Systolic blood pressure changes in indigenous Bolivian women associated with an improved cookstove intervention. Air Qual Atmos Health 2015;8:47-53.

VERSION 2 – REVIEW

REVIEWER	Sayali Mukherjee Amity Institute of Biotechnology, Amity University Uttar Pradesh Lucknow Campus, Lucknow, India
REVIEW RETURNED	20-Apr-2019

GENERAL COMMENTS	The questions raised in my previous review have been revised appropriately. The paper can be accepted after confirmation from a specialist statistician about the authenticity of the statistical interpretation.

REVIEWER	Tangchun Wu Huazhong Univ of Science and Technology, China
REVIEW RETURNED	24-Apr-2019

GENERAL COMMENTS	1. Abstract, Participants: Please state clearly how biomass and non-biomass fuel defined. It appears imprecise to use “non-biomass” since only natural gas was included in this group. 2. Abstract, Results: Please provide p-values instead of saying “no association”. 3. Strengths: The authors state a high response rate, it would be worthwhile to provide the response rate directly. 4. Introduction: The authors may cite the recent study in China (doi: 10.1001/jama.2018.2151), which is the largest investigation so far on the association between household solid fuel use and risk of cardiovascular mortality. 5. Methods: The analytic strategy seems not clear to me, please list all covariates adjusted in your statistical model. 6. Methods: What do you mean by “mutually”? 7. Methods: Is there concurrent fuel use in this population? There may exist additive effect of household air pollution from various fuels sources on CHD. 8. Results: Generally, please re-organize the Results section logically to address the study questions. 9. Discussion: All participants in 14 out of 24 villages use biomass for cooking. Confounding by village may become a major concern that has not been discussed. Sample size is too small to investigate CHD, please discuss this limitation.

REVIEWER	PROF. GREGORY E. ERHABOR OBAFEMI AWOLOWO UNIVERSITY, ILE-IFE, OSUN STATE, NIGERIA
REVIEW RETURNED	15-May-2019

GENERAL COMMENTS	This article by Fatmi and colleagues provided additional information on the growing knowledge oF the association of biomass smoke exposure and cardiovascular diseases, the article is well written and focused, however there few specific comments Page 1 line 30, the same period should be specified Page1 line 50- the first statement had no link with the rest of the conclusion Page 15, line 12 and 13- In table 2, there should be data for 2-9 years and ≥ 10 years in column 3 for long term users of biomass Page 16, line12and 13- table 3, what is No and Yes relating to?

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Sayali Mukherjee

Institution and Country: Amity Institute of Biotechnology, Amity University Uttar Pradesh Lucknow Campus, Lucknow, India Please state any competing interests or state ‘None declared’: No Competing interest

Please leave your comments for the authors below The questions raised in my previous review have been revised appropriately. The paper can be accepted after confirmation from a specialist statistician about the authenticity of the statistical interpretation.

RESPONSE

Thank you.

Reviewer: 2

Reviewer Name: Tangchun Wu

Institution and Country: Huazhong Univ of Science and Technology, China Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below

1. Abstract, Participants: Please state clearly how biomass and non-biomass fuel defined. It appears imprecise to use “non-biomass” since only natural gas was included in this group.

RESPONSE

The wording of the Abstract is accurate. The comparison was between women who had used biomass fuel for cooking for at least the past year, and a group who had cooked only with non-biomass fuel over the same period (see section of Materials and Methods headed “Categories of exposure to biomass”). As can be seen from Table 2, within the study sample, all of the non-users of biomass used natural gas (including LPG) for cooking (because that was the non-biomass fuel that was available in the community studied). We do not think it is appropriate to give a definition of biomass fuels in the Abstract, but it is provided in the first sentence of the Introduction, and as we make clear in the Materials and Methods (first paragraph and subsection on “Categories of exposure to biomass”), within the communities studied, wood and cow dung were the only types of biomass fuel that were used for cooking.

2. Abstract, Results: Please provide p-values instead of saying “no association”.

RESPONSE

We provide odds ratios with confidence intervals, which are much more informative than p-values (see https://www.bmj.com/content/292/6522/746.short). When confidence intervals are given, p-values are redundant.

3. Strengths: The authors state a high response rate, it would be worthwhile to provide the response rate directly.

RESPONSE: Information on response rates is given in the first paragraph of the Results (“Interviews were completed with women from a total of 1073 households….. No-one declined to participate in the study, but 77 women could not be interviewed because they were not at home at the time when the survey team visited”). In the section on Strengths and limitations of the study, we have now added that the response rate was 93%.

4. Introduction: The authors may cite the recent study in China (doi: 10.1001/jama.2018.2151), which is the largest investigation so far on the association between household solid fuel use and risk of cardiovascular mortality.

RESPONSE

Thank you for drawing our attention to this paper, which was published after our report was first drafted. We have added reference to it in the Introduction and Discussion.

5. Methods: The analytic strategy seems not clear to me, please list all covariates adjusted in your statistical model.

RESPONSE

In the section of the Methods on “Statistical analysis”, we set out the analytical strategy for our logistic regression modelling:

“Logistic regression analysis was then used to assess the association of each of the four outcome variables with use of biomass fuel for cooking and other possible risk factors. First, associations with each potential risk factor were determined after adjustment for age. The main exposure of interest (user or non-user of biomass) was then carried forward into a mutually adjusted model along with all other risk factors which showed associations ($p \leq 0.1$) when examined individually. In addition, a second mutually adjusted model was fitted that compared long-term use and non-use of biomass for cooking.”

When this strategy was applied, the variables that were carried forward to the final regression models differed for the four outcomes. The variables that were included in those models are named in the section of the Results where the analyses are presented (e.g. “In analyses that adjusted only for age, hypertension was associated ($p \leq 0.1$) with older age, a higher number of household assets, higher frequency of consuming meat and eggs, and having a high BMI or WHR (Supplementary Table 4). When these variables were carried forward to the mutually adjusted analysis …..”. The specification of the models can also be found in Supplementary Tables 4 to 7, to which the text of the Results section refers.

We think it is best to present the information in this way. If we listed the variables for each model in the Methods, we would in effect be presenting results (which risk factors showed associations in analyses adjusted only for age) before we got to the Results section.

6. Methods: What do you mean by “mutually”?

RESPONSE

This is standard terminology that we and others have used in many previously published papers. If risk estimates for a set of risk factors are mutually adjusted, it means that the risk estimate for each variable is adjusted for all of the other variables in the model.

7. Methods: Is there concurrent fuel use in this population? There may exist additive effect of household air pollution from various fuels sources on CHD.

RESPONSE

As can be seen from Table 2, a small proportion of women (3.7% of biomass users and 6.3% of non-biomass users) had at some time in their lives used kerosene for cooking. Also, 54.4% of the women who cooked with biomass and 25.6 of those who cooked with other fuels reported that their homes were heated with biomass. Importantly, however, as we report in the third paragraph of the Discussion, a linked air monitoring study found that in the kitchens of houses using biomass for cooking, the mean 24-hour average PM2.5 concentration was 531 µg/m3, with a median of 136 µg/m3 and inter-quartile range 34-615 µg/m3 whereas corresponding concentrations in houses not using biomass for cooking were 69.9, 24.2, and 13.5-53.3 µg/m3. Thus, cooking with biomass was associated with much higher exposures to particulate air pollution, and we do not think our failure to find a relationship to CHD can be explained by negative confounding from differences in exposure to pollution from other sources.

8. Results: Generally, please re-organize the Results section logically to address the study questions.

RESPONSE

We have modified the order in which outcomes are listed in the Introduction and Discussion to accord with that in the Methods and Results (i.e. with hypertension first).

9. Discussion: All participants in 14 out of 24 villages use biomass for cooking. Confounding by village may become a major concern that has not been discussed. Sample size is too small to investigate CHD, please discuss this limitation.

RESPONSE

We have no reason to expect that village of residence would be a major determinant of CHD independently of use of biomass and the many potentially confounding exposures that we examined. Nor does the reviewer suggest one. Had we thought that it was a plausible possibility, we would not have used the study design that we chose (since the strong association between use of biomass and village of residence would have prevented meaningful adjustment for any confounding effect of village).

We already address sample size in our Discussion, where we say “The sample size achieved for the study was close to that planned, and the prevalence of the four outcomes was higher than had been assumed in the power calculations. Moreover, the upper confidence limits for the odds ratios relating to use of biomass were almost all <2. Thus, the absence of associations with biomass does not reflect a lack of statistical power.” We disagree with the reviewer’s assertion that the sample size was too small.

Reviewer: 3

Reviewer Name: PROF. GREGORY E. ERHABOR

Institution and Country: OBAFEMI AWOLOWO UNIVERSITY, ILE-IFE, OSUN STATE, NIGERIA
Please state any competing interests or state ‘None declared’: NONE

Please leave your comments for the authors below This article by Fatmi and colleagues provided additional information on the growing knowledge oF the association of biomass smoke exposure and cardiovascular diseases, the article is well written and focused, however there few specific comments

Page 1 line 30, the same period should be specified

RESPONSE

We have changed “the same period” to “during the past year”.

Page1 line 50- the first statement had no link with the rest of the conclusion Page 15, line 12 and 13- In table 2, there should be data for 2-9 years and ≥ 10 years in column 3 for long term users of biomass Page 16, line12and 13- table 3, what is No and Yes relating to?

RESPONSE

Our conclusions focus on possible explanations for the absence of associations between CHD and use of biomass for cooking. The first statement (“A linked air monitoring study indicated substantially higher airborne concentrations of fine particulate matter in kitchens where biomass was used for cooking.”) summarises information that is presented in paragraph 3 of the Discussion, and argues against there being an inadequate contrast in exposures to particulate matter. We think this is key to the conclusions that can be drawn (as did an earlier reviewer). Because the information came from a linked study and not from the results present in this paper, we think it belongs best where we have placed it in the section of the Abstract headed “Conclusions”.

By definition, women who were long-term users of biomass (i.e. currently used firewood and/or cow dung for cooking and had done so for at least the past 10 years – see section of Methods on “Categories of exposure to biomass”) were all current users, and could not have last used biomass two or more years ago.

In table 3, the categories “No” and “Yes” refer to the exposure in bold immediately above them – for example, whether the participant reported ever being hungry all the time during childhood because there was not enough food.

VERSION 3 - REVIEW

REVIEWER	Tangchun Wu Huazhong Univ Sci Tech
REVIEW RETURNED	18-Jun-2019

GENERAL COMMENTS	The reviewer completed the checklist but made no further comments.