

Peer Review Report

Review Report on Associations of household solid fuel use with falls and fall-related injuries in middle-aged and older population in China: A cohort study

Original Article, Int J Public Health

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EVALUATION

Q 1 Please summarize the main findings of the study.

This article reports the results from a population-based cohort study of Chinese adults aged 45 and older to evaluate the association between the use of solid fuel for cooking and heating and the incidence of falls and falls-related injuries. The results indicate an increase in falls and falls-related injuries with indoor solid fuel use for cooking, heating and combined use for both. In view of these results the authors recommend promoting cleaner fuels for cooking and heating for the Chinese older population.

Q 2 Please highlight the limitations and strengths.

Strengths: national population-based (large) prospective cohort study.

Limitations: because of data collection system could not do a survival analysis; some residual confounding cannot be excluded

Q 3 Please provide your detailed review report to the authors. The editors prefer to receive your review structured in major and minor comments. Please consider in your review the methods (statistical methods valid and correctly applied (e.g. sample size, choice of test), is the study replicable based on the method description?), results, data interpretation and references. If there are any objective errors, or if the conclusions are not supported, you should detail your concerns.

I read with interest the results of this national prospective cohort study to evaluate the association of indoor use of solid fuels for cooking and heating and the incidence of falls and falls-related injuries in the middle-aged and older Chinese populations.

While I am not an expert in this field, and so I cannot comment on the relevance of the study and the cited literature, I have some methodological concerns and I believe the reporting could be improved to improve clarity and reproducibility. I detailed them below.

In particular, with regards to the reporting, I suggest the authors follow the STROBE guidelines and checklist.

Major comments

Page 3-4: I think the Study population (2.1) section could be improved substantially as it is currently quite unclear. In particular, it is lacking some important aspects of data collection i.e. how did it take place (e.g. survey, interviews) and when/how often. This also refers to the overall response rate mentioned in the second paragraph (line 62). First, this is only reported for baseline, so it is not clear what the response rates at different follow-up waves were. Also, response rates, as well as the total sample included in the analysis, are normally reported in the Results section.

Page 5, line 100-103: unclear why t-tests and chi square tests were carried out to test for differences in falls and injuries between groups, as it is not the aim of the study and testing for baseline characteristics is generally not advised. If the authors want to report differences in the falls and injuries for the clean and solid fuels groups, this needs to be stated clearly and reported with the effect measure and confidence interval (although this is already done in the regression analysis and reported in Table 2). In general, if testing for any difference, the effect sizes and relevant confidence intervals must be reported; then, additionally, the p-value may be added, but it is not necessary.

Page 6, line 109: subgroup analysis and stratified analysis are two different things. The subgroup analysis tests for differences in the treatment effect (exposure effect in this case) by covariates, which is what the authors comment on later in the results and discussion, but it is done using treatment-covariate interactions in the regression model. The stratified analysis instead rerun the main analysis in two distinct samples (strata) identified by covariate categories, indeed the confusion with the subgroup term. From the way it is reported it seems to me that the authors have performed a stratified analysis, but it is unclear. Also, if a stratified analysis was performed by sex and area of residence, the models in the specified strata cannot be adjusted by sex and residence, respectively, as they are performed on a specific sample (e.g. only male, only rural, etc). The authors should clarify and change the analysis and/or the text throughout the manuscript accordingly.

Line 111: unclear how the trend analysis was performed, more details are needed, also on how it is reported.

Results section, line 111 (and Table 1): Table 1 is normally used as baseline characteristics, but in this table the outcomes are also reported, and the characteristics reported are split by outcome results. I would prefer to see a more "classical" baseline characteristics to show the characteristics of the participants in total and it could also include the outcomes i.e. how many experienced it in total. Then, if authors want, they can report an extra table with outcome differences by characteristics.

Page 7, line 131-136: I am still confused by the trend analysis because the odds ratios reported are those of the logistic regression model 3 so it is unclear if an extra analysis was done and how. If not, then simply remove it. Also, these results are not reported in Figure 2, which I also do not think add anything to what is already reported in Table 2.

Line 143: (refer to previous comment) if a stratified analysis was done, the interpretation is incorrect because the odds ratios refer to two different residential groups i.e. you are not testing for the interaction between solid fuel use and residential areas etc. This need to be checked and, if needed, corrected in the whole document, discussion included.

Page 9, line 178-180: (refer to my previous comment) if a proper subgroup analysis with interaction terms was not used, this higher association for a group cannot be claimed. Also, these analyses are generally underpowered, as also pointed out later by the authors, so I would use a less strong language ("our results suggest that there may be" rather than "our results showed").

Page 10, line 192-195: same comment as above regarding the subgroup analysis interpretation.

Page 11, line 225: unclear statement about the test power for of subgroup analysis – in which sense is inaccurate? Was a power analysis for subgroup analysis done? Subgroup analyses are generally underpowered (and power analysis for them not performed) so only used as exploratory, so the authors can say something along these lines (if a proper subgroup analysis was done) but the term "inaccurate" is a bit vague.

Minor comments

Page 4, line 64: it is unclear why/how there could be subjects who were younger than 45 years or whose age was missing if this is from a population-based study of adults aged 45 and older. It needs to be clarified maybe after adding the required details of data collection.

Line 66-68: anything about covariate adjustment should not be in this section but in the Statistical analysis (2.5) section like for the other covariates.

Page 5, line 95: age should have the unit/option for response (years) in brackets like reported for the other covariates, not "continuous".

Line 102: When referring to categorical variables I would add "dichotomous and categorical" because usually categorical refers to variables with different categories while dichotomous to the ordinary yes/no binary variable.

Page 6, line 111: the term "reference point" is not common, use "reference category" or simply "as reference".

Page 7, line 146-150: it's the model that is "adjusted for" the covariates, so it is better to rephrase it.

Associations were found also in the unadjusted model, not only when adjusted for covariates.

Page 11, line 217: While that it is probably true, I would suggest removing the claim about the sufficient power because a power analysis was not performed (not reported at least).

Tables:

The number of events for combined cooking and heating should also be reported, like in the other categories. All tables have footnotes at the bottom which are not present (referred to) anywhere in the table.

PLEASE COMMENT

Q 4 Is the title appropriate, concise, attractive?

Yes

Q 5 Are the keywords appropriate?

Not all of them, CHARLS is the name of the population-based study, not sure it should be listed as keyword

Q 6 Is the English language of sufficient quality?

Yes, but general writing/language could be improved because very basic and unclear in some parts

Q 7 Is the quality of the figures and tables satisfactory?

No.

Q 8 Does the reference list cover the relevant literature adequately and in an unbiased manner?)

Unsure (not my expertise)

QUALITY ASSESSMENT

Q 9 Originality



Q 10 Rigor



Q 11 Significance to the field



Q 12 Interest to a general audience



Q 13 Quality of the writing



Q 14 Overall scientific quality of the study



REVISION LEVEL

Q 15 Please make a recommendation based on your comments:

Major revisions.