Author's response to reviews

Title: Atopic dermatitis and indoor use of energy sources in cooking and heating appliances.

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Author’s response to reviews: see over
ANSWER TO THE REVIEW: ‘Atopic dermatitis and indoor use of energy sources in cooking and heating appliances.’

Reviewer’s report:
Major Compulsory Revisions
1) It is clear that the indoor environment is characterized by a multitude of parameters of which not all were controlled in this study. As such, it is fair to state that by its nature and design, this study should not be expected to provide a definitive answer. Instead of describing what the data shows and putting this study in perspective to other studies, the authors spend too much effort trying to see in the data what is not there.
2) Nutritional and (personal) hygiene characteristics were not/poorly covered although the might very well be quite relevant e.g. no information about the diet, the use of cleaning products, whether the children were under medical treatment (medication), the presence of pets.... Moreover, no others atopic endpoints were included in the study (e.g. atopic rhinitis, asthma) although inhalation would be suspected the main source of exposure if the usage biomass fuels would have an impact on the infants health. The ISAAC phase III environmental questionnaire typically addresses more aspects. Please clarify if (why) the entire data set coming from the ISAAC questionnaire in Spain was (not) looked at, i.e. justify the selection (if any).

We have substantially changed the manuscript; we performed a new analysis addressing the reviewer comment about the influence of social status. Also we included new descriptive variables in table 2. However, we did not include rhinitis and asthma as there are another two studies in peer-view process addressing both disorders.

Minor Essential Revisions

Abstract
3) Explain abbreviation cOR in abstract: crude odds ratio.
We have corrected it.
4) Refine sentence (include “statistical”): “… electric cooking appliances the cOR was 1.11 (95% CI: 0.99-1.24), near to statistical significance…”
We have deleted it.

Introduction
5) Sentence does not read well: “Chemical compounds indoors originating from heating/cooking systems and tobacco smoke lower the quality of the indoor air children are exposed to and increase the concentration of toxic compounds indoors” The chemical compounds do not increase the concentration of toxic compounds, the heating/cooking systems and tobacco smoke do...
6) Sentence does not read well: “Diesel particulate matter, on the other hand, triggers and increases the production of IgE and hydrocarbons released during kerosene combustion [10].” Where is the contradiction explaining the use of “on the other hand”? The second part of the sentence addresses kerosene combustion, not diesel particulate matter.
We have rephrased the introduction.

Results

7) It should be made clearer that most of the mentioned “differences” are only small numerical differences.
8) The statement related to siblings is only somewhat for “younger siblings”.

We made the appropriate changes according to these two comments.

Discussion

9) The authors start the discussion section by stating that an association between AD and indoor energy sources was not found, but they continue by stating that there seemed to be a slight influence of biomass fuel use. This contradictory statement needs clarification and substantiation.

We have changed the discussion section substantially, and we tried not to fail in these inaccuracies or contradictions. However, we consider that a non-significant result cannot be taken in a strict way, and if there is some slight trend or ‘almost significant’ result, they deserve some discussion.

10) It would add to the study if it was checked whether the heating/cooking parameters are retained in the model when step-wise backward logistic regression modeling would be used. This would also add some information on the relevance of the energy source in the model predicting for AD. Does leaving out the energy source variable change anything in the model?

We performed the proposed analysis. In some cases the OR values changed, but in others not, with an undefined pattern. It would mean that the influence of some energy sources would be different according to its nature, as we have assessed in the present manuscript. But we consider that the inclusion of these results would be difficult to understand due to the high number of OR values.

11) Considering that none of the aOR related to electricity reached statistical significance and exceeded 1.16 the elaboration on electromagnetic waves is highly speculative and not justified considering that likelihood of confounding effects explaining the results related to electricity. One is even brought up by the authors, i.e. the hygiene hypothesis.

The results have changed slightly, so the discussion has been adapted to the new conclusions.

12) Study limitations: What about the possible relationship between parental education and reporting bias?

We have mentioned both aspects in the discussion

Discretionary Revisions

13) The study by Po et al 2011 deserves to be mentioned in the discussion section. Po JYT, Fitzgerald JM, Carlsten C. Respiratory disease associated with
We have considered not to include it in the manuscript because it more focused on women health, and in a developing country, which are not the main targeted population in our article.

14) There is a well known negative relationship between social status (maternal education) and smoking behavior, 2 variables in the multivariate model. The frequency of ED shows a positive association with both (trend only for social status). This deserves some elaboration. It is an interesting issue, and it has been already studied in the Spanish ISAAC group. In fact, we have included some explanation in the results section. However, the authors consider that it does not deserve more elaboration as the main objective of the manuscript is the use of domestic devices and AD.