Author's response to reviews

Title: Home Exposure to Arabian Incense (Bakhour) and Asthma Symptoms in Children: A Community Survey in Two Regions in Oman

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Author's response to reviews: see over
The revised title is:

Home Exposure to Arabian Incense (Bakhour) and Asthma Symptoms in Children: A Community Survey in Two Regions in Oman

REVIEWER'S REPORTS (quote in red and green text) AND RESPONSE (black)

A. Reviewer: Hanaa Banjar

Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore):

• “Background: Needs to mention the incidence of asthma in Oman”

R1.1: Although we previously gave reference to this information, we have now mentioned it in the background as requested with the addition of one reference (#2). The references were re-arranged accordingly

• Subjects: Needs to mention the difference between the prevalence of asthma in Muscat and Sharqiyah.

R1.2: An example of self-reported asthma diagnosis in 13-4 year old children was added. The full detail on the difference between the various regions for all asthma symptoms in both age groups (6-7 and 13-14 years) is available in reference # 4 (previously # 3) which indicated in the relevant section of methods.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

• Table 2, under bakhour use at home, 3 or more times; There was no mentioning of the p-value
• Figure 1: needs to add in the legend, the meaning of the star sign on other Smokes

R1.3: Figure 1 and all Tables were carefully revised and the above observations were rectified.

Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

• Definitions: Probable asthma- Is not accurately defined

R1.4: Our original categorization of the children into three groups was an attempt to define a group of children who are very unlikely to have current asthma for use as “normal control” to compare with children who fulfilled the definition of current asthma. However, this created an intermediate group (those who did not fit the current asthma definition, but reported current or past history of non-specific respiratory symptoms), which we called “possible asthma” indicating that there
asthma status cannot be determined on the basis of the questionnaire. Understandably, this group created confusion as reflected by this referee comments and referee # 2 (David S. Chi) together with comments of both referees on the results relating to this categorization of the children.

We admit this was not the standard way of categorization and perhaps we tried to get too much from a questionnaire! In fact all ISAAC (International Study of Asthma and Allergies in Children” studies (including the Global ISAAC Steering Committee publications) as well as similar questionnaire-based surveys used two categories of subjects only, when addressing risk factors for asthma: those who fit the adopted definition of asthma and those who did not without sub-categorizing those who did not fit the definition into “normal and possible” as we did. Therefore, in accordance with these studies and in agreement with suggestions of the 2 referees, we re-categorize our subjects into two groups: children with current asthma as previously defined and children with “no current asthma” comprising all children who did not fit the definition of current asthma. As a result, our previous normal group (with negative responses to all questions and the intermediate group are now grouped together as children with no current asthma, and we re-analyzed and presented the data accordingly.

This change resulted in a slight reduction in the relevant Odd Ratios values, but their statistical significance was maintained, and our previous interpretations and conclusions remained valid.

• Results:

1. Table 2: Can repeat the statistics to include only 2 groups: The non-asthmatic and the asthmatic excluding the probable asthmatics.

   R1.5: Perhaps the referee meant excluding “possible asthmatics”. This was addressed by re-categorization as mentioned above (R1.4). In addition, based on suggestion # 3 (see below), and after careful review, we agreed that Table 2 was redundant and it was deleted as suggested.

2. Table 3 and table 4 have many items repeated and can be unified in one table (e.g. bakhour use in the room, gender, region, father education, mother education, and parental smoking).

   R1.6: Although we agree with the referee regarding the repetition of several parameters in both tables, the two tables are showing two different dependant variables (the two primary outcome variables) and cannot be combined without seriously affecting the data presentation and analysis. The dependent factor in Table 3 (now Table 2) is “reporting that child is affected by exposure to bakhour and in Table 4 (Now Table 3) is current asthma. Both these dependent variables may be affected by the various listed factors in each of the tables to different extent.

3. Table 2 can be deleted and added to table 5

   R1.7: Table 2 has been deleted as indicated above (R1.5).

4. PFT was not done to confirm the definition of asthma or the effect of the triggers

   R1.8: As indicated in the methods, this was a questionnaire based survey, and like all similar studies, objective measures such PFTs were not done. This was already highlighted in the study limitations.
5. Authors contribution: Needs to mention them by their names but not abbreviation (as OA, AA, BM)

R1.9: This is as per BMC Journals formatting

1. Is the question posed by the authors well defined? Yes

2. Are the methods appropriate and well described? No,

The methods were not appropriately described. The author compared 2 districts of Oman (Muscat and Sharqiyah) which were not mentioned in the original question, and mentioned many findings for this comparison in the results and Discussions (Table 1). The Author may need to change the question to include the comparisons of the 2 districts. The definition of possible asthma is confusing and not accurate: As it consists of children with respiratory symptoms (not defined which symptoms, is it cough, snoring, noisy breathing, or what?).

R1.10: All of these observations have now been addressed: The data on prevalence of asthma in the two regions are mentioned (see R1.2) and the objective of comparing the two regions was added to the study aim. We have also revised the title to be more reflective: “Home exposure to Arabian Incense (Bakhour) and Asthma Symptoms in Children: A Community Survey in Two Regions in Oman”. The new title highlights the three main aspects of the study:
- Home exposure to bakhour in term of frequency and pattern of use
- Its relationship with asthma symptoms in term of prevalence and triggering
- The study design and setting

As described previously (R1.4), the recurrent concern regarding the confusing definition of “possible asthma” has been resolved. The respiratory symptoms are well characterized in the widely cited ISAAC questionnaire. In addition they are listed in Table 1 and Table 5 (now Table 4).

3. Are the data sound? No. Some of the data is accurate, but there are many confusing and conflicting results, e.g.
- Table 1 in core asthma symptoms section (Night cough in the absence of cold is 17.2 in Muscat compared to 23.4 in Sharqiyah) and in another statement (sleep disturbance on night or more / week is 1.5 in Muscat compared to 0.5 in Sharqiyah)

R1.11: These are accurate figures and they have no conflict. They are exact questions as per the ISAAC core questionnaire addressing different aspect of asthma despite apparent similarity. The first (Night cough in the absence of cold) refers to any dry cough at night in the past 12 months which is a common typical asthma symptom. The second (sleep disturbance one night or more /week) refers to sleep disturbance due to any of asthma symptoms and used as an index of asthma severity depending on its frequency. Sleep disturbance one night or more per week indicates severe asthma and therefore much less frequent than night cough in the past 12 months. In fact we have previously shown (ref. # 3) that the prevalence of sleep disturbance and other severe symptoms such as speech limiting wheeze in our children were relatively highly compared to developed countries suggesting poor asthma control.
• Table 2, the frequency of never or rarely using Bakhour per week was higher in asthmatic compared to non-asthmatic (23.3% vs. 19.8%) , at the same time frequency of using Bakhour 3 or more times per week is more frequent in asthmatic compared to Non-asthmatic (59.3% vs. 56.3)???. I cannot understand these conflicting results.

R1.12: Again these figures are correct. There is in fact no conflict if one realizes that there is an intermediate category (1-2 times per week) which was reported to be more in non-asthmatics. These figures simply mean that parent of asthmatics either don’t use bakhour (never or rarely) or use it frequently (3 or more). In any case, Table 2 has now been deleted.

• In Table 4, and page 10, 3rd paragraph: the author mentioned that “Bakhour use of 1-2 times per week was associated with lower prevalence of current asthma in simple regression analysis (OR=0.63, 95% CI 0.44-0.91), this association was not significant after adjusting for all other factors”. I conclude from this finding that there is no effect Bakhour on current asthma, which was the whole mark of the study.

R1.13: The referee’s conclusion is in agreement with our stated interpretation and conclusions. Based on previous literature and our clinical experience, we hypothesized that exposure to bakhour may be a risk factor for asthma. However, as we clearly indicated in the abstract, results and discussion, our study found no association between use of bakhour and the prevalence of current asthma. On the other hand, (as indicated in the results and discussion), our study showed that bakhour exposure is a common trigger (precipitating) factor of asthma symptoms (the fourth most common trigger factors of asthma symptoms among asthmatic children – Figure 1) and was significantly associated with the risk of having breathing difficulties among asthmatics (reporting the child is affected by exposure to bakhour – Table 3 now Table 2).

4. Does the manuscript adhere to the relevant standards for reporting and data deposition? Yes

5. Are the discussion and conclusions well balanced and adequately supported by the data? The discussion was long and needs to be more concise. The conclusion is not appropriate, as the exact effect could have been measured by pulmonary function test.

R1.14: The discussion has been revised for English and the consequent change resulting from the refined approach to analysis and presentation of the data described above (R1.4 and R1.5). We also removed unnecessary details of previous studies making the discussion more concise.

Although, we agree with the referee that the exact effect might be better elucidated by PFT measurement, we believe that the stated interpretations and conclusions are appropriate for the study of this kind.

6. Are limitations of the work clearly stated? Yes

7. Do the authors clearly acknowledge any work upon which they are building, both published and unpublished? Yes

8. Do the title and abstract accurately convey what has been found? No. The study included the comparison between 2 regions in Oman, but didn’t compare other regions.
R1.14: These have been addressed (please see R1.1 and R1.10).

9. Is the writing acceptable? Yes

**Level of interest:** An article of limited interest

R1.16: We are pleased that the other 2 referees described the article as “of importance in its field”. As we indicated in the introduction, it is important to “explore the less characterized risk factors related to life style, culture and home environment which may be peculiar to different populations”.

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the Statistics.

R1.17: As indicated in the authors’ contribution section, the second author (A. Al-Maniri) is an epidemiologist. He was responsible for all the data analysis as well as the response to the reviewers’ comments relating to data analysis. As mentioned above (R1.4) the data were re-analyzed based on the newly adopted categories.

**B. Reviewer: David S. Chi**

This is a community based study of a public health issue investigating the relationship of Arabian incense burning (bakhour) and the prevalence of asthma in Omani children. The study was a continuation of a previous work of the International Study of Asthma and Allergies in Children (ISAAC) and offered some interest data. Generally, the report is straight forward and concise, but a couple of tables should be better edited.

Discretionary Revisions: None.

**Minor Essential Revisions:**

1. English writing needs to be checked extensively.
   
   R2.1: This has been done throughout.

2. In the Discussion section, when data were mentioned/discussed, it will be clearer and easier for reader if the Table number from which the data was shown was cited in the text.
   
   R2.2: Although, this not a standard way, we referred to tables and figures in the discussion as appropriate.

3. Page 4, line 13, in “have,” “,” should be deleted.
   
   R2.3: Done

4. Page 9, line 7, the number in “(39.7 versus....)” needs to match that in Table 2, which listed as 39.1.

   R2.4: As mentioned in our response to the first reviewer (R1.4-R1.7), Table 2 has been deleted.
5. Page 11, line 12, the sentence, “The few previous studies on the same were case control and based on a dichotomous variable of the exposure (users and non-users)” is not clear and needs to be re-written.

R2.5: This sentence was removed as it was redundant since the mentioned studies were highlighted in a later section of the discussion.

6. Page 13, line 2, in “......80% asthmatic children compared to 66%.,”, 66% of what? It needs to be spelled out.

R2.6: 66% of normal. This was added.

7. Table 1 is very confusing. I believe all the numbers in Table 1 are percent, except “sample size” and “mean age.” But only “males” indicates (%). I would suggest that all actual numbers are given with percentages in parenthesis. The P-value should be footnoted regarding the type of statistical test.

R2.7: Table 1 has been revised and all the above points were addressed. The sample size and demographics (mean age and gender distribution were removed from the table and presented as text in the first paragraph of the results. The remaining parameters are all frequencies and presented as %. The types of statistical tests are standard and already mentioned in data analysis sections of the Methods.

8. The title of Table 2 should be re-worded and omit the “n” numbers of each category. The term of the categories of asthma are not used uniformly. For instance, in the title of Table 2 the asthma is possible was termed as “undetermined,” while in the Table heading it was “indeterminate.” Also, in the title of Table 2 the term for the asthma is unlikely was “Non asthmatic,” while in the Table heading it was “Non asthmatics.” The authors should decide to have a uniform term and use it throughout text and Tables consistently.

R2.8: As mentioned in our response to the first reviewer (R1.4-R1.7), Table 2 has been deleted.

9. Why was the frequency per week of using bakhour divided into two groups in Table 1, but 3 groups in Table 2? The ‘Never or rarely” group in Table 2 is dubious and needs to be redesigned.

R2.9: In fact, there were three categories for the frequency of bakhour use per week (never or rarely, 1-2/week and more than 2 per week). As noted by the reviewer, in Table 1 we combined the first 2 categories as (0-2 times) while maintaining the three categories in Tables 3&4 (now 2&3). We therefore, changed to the same three categories in Table 1 for consistency.

10. In Table 2, according to my own calculation, the sum of three groups is 1069 and 990 for the “Non asthmatics" and “indeterminate," respectively, but they were listed as 1073 and 992, respectively in the Table heading. They need to be corrected. Since this inconsistency was found here and other places (see below), it would be important for the authors to double check the numbers throughout the text and tables.

R2.10: As noted earlier, Table 2 has been removed. In addition, we have carefully double checked all the numbers in the remaining tables and text and
we confirm that they are all correct and any apparent inconsistencies were clarified.

11. In Table 2, all “No” rows should be deleted as they are not needed. The type of the statistical test for this table also needs to be footnoted. 12. In Table 3, for consistency, add “confidence intervals” in front of (CI) (the same for Table 4) and in “Bakhour use ‘n’ the child room,” it missed an “i” before “n” and child should be child’s. 13. Figure 1 “**” needs to be footnoted.

R2.11: The problematic Table 2 has been deleted (see above). We also agree with and applied all remaining suggestions.

**Major Compulsory Revisions:**

1. The aspect of statistical analysis needs to be reviewed or checked by a statistician, as I am not an expert in statistics.

R2.12: Please refer to response #R1.17 on this.

2. Page 14, line 3, in “…association (protective effect) which….” It is not a protect effect, just a negative association! Thus, “(protect effect)” needs to be deleted!

R2.13: Done.

3. The numbers of father and mother of no school education are listed 973 (41.1%) and 1,328 (55.3%), respectively, in Table 3. If the total number is 2441, the % of father and mother of no school education should be 39.9% and 54.4%, respectively. Again, the authors need to double check the data!

R2.14: As mentioned above all the numbers were double checked as suggested. The apparent minor difference between what was presented in table 3 for some variables and what was calculated by the reviewer (the reviewer assumed that the sample size was 2441 for all independent variables) was due to the fact that during regression analysis, subjects with no response to a specific variable were removed from analysis. Thus the analyzed sample size for some variables was slightly less than the total sample of 2441. The actual total response for each variable can be derived by adding up the presented actual responses in each of its categories. From table 3, it is clear that the analyze sample size for most variable was 2441, except for three variables (bakhour use = 2435, father education = 2370 and mother education = 2401). Since it is possible to quickly know the total response for each variable from the table, we feel presenting the total response per variable will make the table busy without any additional benefit. However, if you feel necessary, we are prepared to do so by adding a row for each variable indicating the total response.

4. Since the surveys were completed by parents. With due respect, could it possible that forty percent of parents with no school education completed the survey. This reviewer also wonders how accurate and validity the survey could be.

R2.15: This is a valid concern, however, the number of both parents having no school education is much less. To maximize the accuracy of the questionnaire, we gave instructions on how to fill the questionnaire indicating
that parents may seek assistance in filling the questionnaire, and for this reason, we included a question to identify the person who completed the questionnaire. In addition, the consistency of our results over the three ISAAC surveys (more than 7000 children in ISAAC Phase One in 1995 and more than 8000 children in ISAAC Phase three in 2001 and this survey (2441) from different regions of the country and for different age groups (6-7, 14-13 and 10 years age groups together with our strict adherence the ISAAC protocols argue against a significant impact of parent education on the quality of the questionnaires.

**Level of interest:** An article of importance in its field

**Quality of written English:** Not suitable for publication unless extensively edited

R2.16: As mentioned previously, the entire manuscript has been thoroughly revised both addressing specific points raised by the referees as well as global editing.

**Statistical review:** Yes, but I do not feel adequately qualified to assess the Statistics.

R2.17: Please refer to response #R1.17 on this.

**C. Reviewer: Yung-Ling Lee**

A very nice paper with simple conclusions for the associations between Arabian incense use and childhood asthma. The sample size of the study population was large and the analytic methods were proper. The parts of results and discussion are too long and could be significantly shortened. Besides, only some minor revisions are needed.

1. Authors should reference tables or figure in the discussion part, such as adding "(Table 4)" at the end of the first paragraph of page 12.
   
   R3.1: Done

2. In figure, authors should address the meaning of the “star symbol”.
   
   R3.1: Done

3. In abstract, the time of study conduction should be added.
   
   R3.3: the time of the study was included in the methods section which we believe sufficient.

4. Page 4, the last 7th line, should be clarified.
   
   R3.4: The paragraph starting with (Studies from the Far East ….), was rephrased.

5. page 8, the first line, “returned”
   
   R3.5: Corrected

6. Page 11, the 10th line, “The few previous studies on the same were case…..” should be clarified.
R3.6: Done (please refer to response # R2.5) on the same sentence.

7. Page 12, the last 2nd line, “In Qatar, a case control study…” should be clarified.
   
   R3.7: The sentence was rephrased.

8. Page 14, the last 4th line, authors should added “as” after “Incense burning has also been reported”

   R3.8: Done

**Level of interest:** An article of importance in its field

**Quality of written English:** Needs some language corrections before being published
   
   R3.9: Done

**Statistical review:** No, the manuscript does not need to be seen by a statistician.