Reviewer's report

Title: Air Pollution in Boston Bars Before and After a Smoking Ban

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Reviewer: Martie van Tongeren

Reviewer's report:

General

This manuscript contains some interesting, albeit a limited amount of data (only 2 results from 2 days), which may warrant publication. However, I believe that the paper will benefit if it is greatly reduced in size and more focussed on reporting the measurement data. The main message is the vast improvement in air quality after the smoking ban, and the positive health impact that this will have on bar workers. I don't think that the readers of this journal are particularly interested in the model (why use this if you have measurements) and the extensive description of the methods and calculation of the ventilation rates. Also, some of the levels of detail provided in the table can be reduced.

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

1) I feel that the paper will be improved by adopting a standard scientific structure (introduction, methods, results, discussion).

2) Page 4, 2nd par. Criteria for eligibility were the presence of visible smoke. I believe that this is a flaw in the design of the study. If you investigate levels of SHS before and after the ban, than you should measure according to a strict and representative sampling strategy, and the two bars/restaurants were noone was smoking should be included. The pre- vs post ban comparison can probably be adjusted by assuming background levels in the two bars/restaurants where no smokers were present.

3) Pages 5, 10-12. Reduce the descriptions of the methods and calculations of the ventilation rates to an absolute minimum.

4) Page 9 and elsewhere. Exclude sections on the description and results of the model. This is not necessary in this manuscript.

5) Page 10. See before, I don't think that the section of ventilation

6) Page 13, Figure 4. I think that this section is over interpreting the limited amount of data that is available. You have 6 measurements and you try to relate these to a number of different ventilation rates and smoking density. Also, I am not convinced that this provides strong evidence that PPAH are removed at higher rate than RPS. It would suffice to show the RSP and PPAH by smoking density which suggest a strong association.

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

1) Abstract - Provide some quantitative results in the abstract.

2) Page 3, 2nd par - "A large body of epidemiologic ... decreases in RSP concentration [10]." There is sufficient epidemiologic evidence of SHS that can be quoted here (e.g. IARC monograph Vol 83) and I suggest that this is quoted rather than use evidence from outdoor air pollution studies.

3) Page 3, last sentence change to: "... laboratory animals inducing lung and upper respiratory tract cancers when inhaled, and tumors ...".

4) Page 4, 3rd par. What type of dust is the factory calibration based upon. Often this is some type of
standard dust, which distinct physical characteristics which may be different from SHS. This may have quite a major impact on the absolute levels and should be discussed.

5) Page 6 last sentence. The fact that RSP and PPAH are correlated cannot be regarded as evidence that PPAH levels are due to RSP, as there may be other sources. The main evidence for this lies in the reduction of PPAH after the smoking ban.

6) Page 7, 2nd par. I that in the results section you should report the results and not include too much interpretation of data. The fact that indoor levels are 10 times outdoor levels, and almost 30 times higher then the hotel room is sufficient here. It cannot be concluded from this alone that this is due entirely to SHS. The major evidence from this comes from the reduction in these levels after the smoking band.

7) Page 8, 2nd par. It is sufficient to report on the reduction of RSP and PPAH levels. I don't believe that allocating a % contribution from smoking actually adds anything.

8) Page 12/13. Regression of RSP on PAH is based on only 6 measurements. It would probably suffice to show the plot and report the R-sq, but not report the regression equation.

9) Page 13, 2nd par. See 7)

10) Discussion, page 16-19. This can probably be reduced.

11) Conclusions #12. This cannot be deduced from the data and depends on the other sources of exposures. #13 This can also not be concluded from the data provided in the manuscript.

12) Table 2 and 3. I think it would benefit interpretation if these two tables are combined showing both pre- and post ban levels in one table. Clearly, some other columns will need to be deleted (eg. Area, ceiling, volume, total # present, etc.)

Discretionary Revisions (which the author can choose to ignore)

1) Page 7, 3rd par. There is some repetition in this paragraph, related to date and time of measurements.


3) Figure 1 and 2. The quality of these graphs is not great and I suggest separating RSP and PPAH in 2 distinct figures.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable

Statistical review: No

Declaration of competing interests:
I declare that I have no competing interests