

Reviewer's report

Title: The Effect of Ambient Air Pollution on Respiratory Health of School Children: a Panel Study

Version: 1 **Date:** 11 April 2007

Reviewer: Jane Koenig

Reviewer's report:

General

This is an ambitious study with an interesting experimental design for obtaining exposure information on a group of individuals. In general I review it positively but I have a few comments and concerns. The chief concern is incomplete detail about data collection and analysis. The reader needs to know how often all endpoints were measured. Next we need to know what exposure metric was used in the analyses: hourly lag times? 24 averages? . I also would like the authors to edit the conclusion. A null study does not necessarily mean that there is no association between the variable of interest. It may be that circumstances simply did not allow an adequate test of the hypothesis. Observing no change in measures of airway inflammation can be due to a lack of a true association OR lack of the crucial variable that would have detected the association. Therefore I recommend that the authors qualify there conclusion to state simply that there was no significant effect IN THIS STUDY.

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

Introduction:

At the end of the first para, could add that residential wood use is growing in the New England states and upstate NY.

Under the section on Pollution monitoring.

It is not sufficient to refer to previous work. Authors need to cite the type of air monitor used and other details of the monitoring station. Heated monitors?? It also would be good to cite any indoor sources of particulate pollution. Proximity to kitchen eg.

Participants

It would have been preferable for the investigators to note the time of last med use in the subjects using rescue inhalers. Or perhaps they did and if so those data should be entered into the statistical analysis.

Exhaled breath

The authors state that lung function measurements were undertaken twice daily. However they do not give us that information for exhaled breath collection. That information needs to be added to the mss. (for diary cards and urine samples as well).

EBC pH. I thought the protocol for measuring pH required gas stabilization. The authors need to justify their decision to not use degassing.

Statistics

I question the use of a non-parametric test. Also I don't understand what comparisons are being tested. This is a repeated measures experiment design which gives it a high statistical power. I don't think the sign test takes advantage of the repeated measures. Which variables were entered into the mixed regression model? Also, we are not told the lag structure for the analysis. If the investigators have continuous PM data they could look at hourly lags. If not they would use 24 hr averages but they might find something interesting using 1, 2 and 3 day lags. It appears a more robust statistical analysis is recommended.

Results

I have concern about the uncontrolled use of medication by children with asthma. As mentioned it would be best to note the time of latest med use when performing lung measurements. Actually the better design would be to tell the children not to use meds prior to 4 hr before measurement. However if the investigators don't have those data perhaps they could stratify there analysis on med use.

Throughout the document the word data is used as a singular noun whereas it is plural. Authors need to change data was to data were. See first sentence under Lung Function Data.

Under this section the correlation between am FEV1 and outdoor air is mentioned. Apparently it goes away when subjects are stratified by asthma/non-asthma. However what happens if they remove the med users from the asthma group??

I recommend a Results table showing the effect estimates and p or t values.

Second page of Discussion.

It seems that the authors are downplaying their lung function /outdoor air pollution results. In Results they stated found a correlation ($p=0.034$).

Also in discussion there needs to be some qualification of the urinary 1-hydroxypyrene data. Certainly wood smoke was not the only air pollutant to which the subjects were exposed. This biomarker is not specific to woodsmoke!

We are given no basis to evaluate the conclusion that EBC is not useful for assessment of population health effects of air pollution since we do not know how often it was measured and have been given no statistical results.

In my opinion this is a potentially interesting study. However prior to publication it needs further work: more complete description of exposure protocol; a comparison of exposure at the school to exposure in the community; more complete description of data collection. More description and results of statistical analyses.

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

No page numbers!

Discretionary Revisions (which the author can choose to ignore)

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

Level of interest: An article whose findings are important to those with closely related research interests