Reviewer’s report

Title: Urban air pollution and emergency room admissions for respiratory symptoms: a case-crossover study in Palermo, Italy.

Version: 1 Date: 19 December 2010

Reviewer: Jennifer Peel

Reviewer’s report:

This manuscript is generally well-written and addresses an important public health problem – the link between ambient air pollution and acute morbidity (and specifically ED visits for respiratory disease). However, I have several major concerns about the manuscript. There is a rich literature regarding ambient pollution and acute respiratory morbidity – and specifically for respiratory ED visits from around the world (U.S., Canada, Europe, Australia, etc.) that is not acknowledged or referenced in this manuscript. The evidence for the relationship of many ambient pollutants (PM, ozone, CO, NOx) and acute respiratory morbidity (including ED visits) is well-established. In fact, the US Environmental Protection Agency considers the evidence either sufficient to establish a causal association or suggestive of a causal association for these pollutants. Therefore, it is not clear what this relatively small, single-city study is adding to the evidence/literature. This is not to assert that single-city studies are not able to make important contributions; however, this study does not have anything unique that will make such a contribution. Furthermore, the manuscript emphasizes ‘traffic-related’ pollution but does not discuss the growing literature regarding traffic-related pollution/exposures and health. There is a recent report from the Health Effects Institute that summarizes the field for both exposure measures and health effects (and this report and other similar literature are not referenced). While the dominant source in the study area may be traffic (although this is not discussed), the ambient measures used in this manuscript are not necessarily the best measures to use as surrogates for traffic-related pollutants – certainly not SO2 and PM10. Therefore the assertion that this study is examining ‘traffic-related’ pollutants is overstated and not justified. Finally, the manuscript overstates some of the results – for example, the evidence from the stratified analyses (age, sex, season).

Major Compulsory Revisions

1. Abstract: The statement that “…there is little information on health risks of short-term exposure to ambient pollutants by using emergency room admissions as the indictors” is not accurate.

2. Abstract: Results (ORs and 95% CIs) always need to accompanied by the unit increase.

3. Abstract: Results section states that associations were observed for both seasons, while the Conclusions section states “…particularly during the warm
season.” These statements do not match.

4. Background: Include better references as described above.

5. Background, 2nd paragraph: The point of this paragraph is not clear. Please clarify.

6. Methods: Please clarify – are all pollutants measured at all 10 monitoring stations? Please provide reference for B-ray attenuation method for PM10. Why are ozone and PM2.5 not included? Why is ozone not included? Is there anything known about ozone and PM2.5 concentrations in the study area? Where any criteria used for calculating pollutant metrics (24-hour average and 8-hour max average) when there were missing values in the hourly data (e.g., 18 of 24 hours need to be non-missing in order to calculate the 24-hour average or 6 of 8 hours need to be non-missing to calculate an 8-hour average)?

7. Methods, Page 7: The paragraph starting “The completeness criteria…” is not clear. Please clarify. What are the ‘…other measures…’?

8. Methods: Why are NO, NOx, and NO2 all included? The authors even state that they are correlated.

9. Methods: Are only residents of Palermo included in the analysis? If so – please include. If not – why not?

10. Page 8: Please explain what is meant by ‘…overestimation bias…’; it is clear that including repeat visits in a day may overestimate the number of independent visits, but how would this lead to a bias in the results?

11. Methods: How was the unit increase for pollutants chosen? Typically the SD or IQR are used, or a value that is typically used in other literature. Particularly for SO2, 10ug/m3 is a very large increase – too large for the range of concentrations observed in the data. This is why there are some very strong ORs observed in the results.

12. Why was apparent temperature modeled as a quadratic term? Typically temperature is modeled using more flexible splines.

13. Methods: Were the distributed lag models done using constrained or unconstrained distributed lags models?

14. Methods: How were the influenza epidemic peaks modeled?

15. Methods: Was a statistical test for interaction/effect modification performed?

16. Methods: How were the respiratory diagnoses chosen?

17. Methods should state that visits for ages under 15 were not included. Why were they not included?

18. Results: There is an emphasis on statistical significance in the results and Discussion section. Interpret the results (e.g., the magnitude and patterns).

19. Results: How were conclusions about which strata were ‘stronger’ made? What is this based on? Were there statistical tests of interaction? Were the widths of the CIs considered?

20. Results: Clarify statement about same day lag. What lag is being presented
for the main results and why? Are these the distributed lag results as described in the Methods (lags 0-5)? If not – why not?

21. Table 2: state what lag is being presented here.
22. Figure 4: state what lag is being presented here.
23. Discussion: needs better review of similar literature (more and better references).
24. Discussion: The discussion overstates the results (e.g., “…clear difference by season…”) and connection with “traffic-related pollution.”
25. Discussion, Page 13: Explain statement about CO and correlated pollutants. Why would this apply to CO and not to other pollutants?
27. Discussion, Page 14: Describe “…controversies regarding the use of multipollutant models…”
28. Discussion, Page 15: Please clarify why cooking, dusting, cleaning, and child care would be relevant in a study of short term ambient concentrations of pollution and respiratory morbidity. These factors are not typically confounders because they are not related to ambient concentrations.
29. Discussion, Page 15: Please explain the limitations discussed. Why would these individual behaviors be important in this study? What is meant by “…lack of ICD codes in admission records…”. From the methods it appeared that the nosologist coded the records. Is this not accurate?
30. IRB approval is not mentioned in the manuscript.

Minor Essential Revisions
1. Methods: Why are humidity, wind speed, pressure and precipitation mentioned as being collected but not used in the analysis?
2. Page 8, Statistical Analysis: Order of analyses described is confusing; perhaps start with descriptive analyses on health and pollution data and then describe logistic regression models.
3. Results, page 10: Unclear what ‘events of interest’ are – perhaps state respiratory ER visits.
4. Results: not clear why results for relative humidity, wind and precipitation included?
5. What is Figure 2 adding to the manuscript? Probably not necessary (could be stated in text without figure).
6. Table 1: Include standard deviation.
7. Page 14: the term ‘Geo-orographical’ is not clear.

Level of interest: An article of limited interest
Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests.