Reviewer's report

Title: Residential exposure to motor vehicle emissions and the risk of wheezing in a city-wide cross-sectional study of 7-8 year-old schoolchildren in Nicosia, Cyprus.

Version: 1 Date: 11 February 2010

Reviewer: Bert Brunekreef

Reviewer's report:

Comments on Middelton et al manuscript

This paper relates wheeze in children in Nicosia, Cyprus, to various proxies of traffic pollution.

Comments:

1. the association between traffic pollution and wheeze in children has been studied many times, and the authors do not make sufficiently clear what this study adds to the existing literature. So the first question is: what is special about this study that we could not learn from studies that were already published?

2. The authors used ISAAC questionnaire data obtained in 1999/2000, with a high response rate of over 80%. They then used a digital road map purchased from a private company to assist in exposure assessment. It is not clear when the map was made, and no information is given as to the accuracy and precision of the map. This could be added. The authors refer to recent expansions of Nicosia, so the question about when the map was exactly made is pertinent.

3. Emission data were obtained from a 2001 database, i.e. a point in time close to the questionnaire administration years. Some discussion is needed whether the European data base used was appropriate for the car fleet of Cyprus. The representation in table 1 is a bit puzzling, showing cumulative PM and NOx in three buffers, giving as units kg/km. If the unit is kg/km, it should not sharply increase with increasing buffer size so I suspect the unit is different, e.g. total mass produced in the buffer. This needs to be clarified. Specification is also needed for the time units (kg per day? Week? Year?).

4. The authors used wheeze and asthma as primary outcomes, and in some analyses adjusted for ‘co-morbidity’, i.e. eczema and rhinitis. This is strange – several studies have documented associations between traffic pollution and these outcomes as well, and adjusting for them runs the risk of over adjustment. This part is best removed from the paper.

5. The authors in table 2 present a detailed analysis of associations between potential confounders and the three main outcome variables. This is unnecessary, this information distracts from the main results in table 3.

6. The authors mention the analysis in which both home and school addresses
were used only in the text (p 13), mentioning that many more kids were ‘exposed’ when considering schools too. It would be nice to add another table with this analysis. Several studies on traffic pollution and wheeze in children have used the pollution at the school address as primary exposure variable, arguing that that’s where kids spend much of their day time when activity levels and pollution are high.

7. The authors seem to make too much of the non-linearity of their findings. The sample is really too small to draw firm conclusions about the shape of the exposure response function, especially given the crude nature of the exposure assessment with a significant proportion of home addresses not geocoded at the address but at the street level.

8. There is no need to dwell on the general time series literature on p 17, that is not very relevant for this type of study.

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

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**

'I declare that I have no competing interests'