Reviewer's report

Title: Short-term effects of ambient particulates and gaseous pollutants on the incidence of transient ischaemic attack and minor stroke: a case-crossover study

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Reviewer: Maria Jose Medrano

Reviewer's report:

The topic of this paper is relevant. Cerebrovascular diseases are one of the leading causes of death and disability in European countries and its relation to air pollution is not clear yet. Moreover, as the authors say, there is not similar research on the specific target on TIA and minor stroke. Lastly, the study design is interesting, as most studies on the matter are ecological time series analyses, so this case-crossover design adds a new methodological approach.

The study is well written and clearly presented, but some aspects need revision, especially as the results are unconclusive. My specific comments are below:

Major Compulsory Revisions:

1. The study is restricted to TIA and minor stroke, the less severe forms of cerebrovascular disease, probably because of data availability. I wonder if inclusion of fully established stroke cases would have clarified the results in one sense or the other. As the data set used did not include these cases, the alternative would have been using other data sources to focus also on stroke, i.e. hospital discharge data. I suggest the authors should add this analysis or alternatively, comment on this in the discussion.

2. As the authors say, sample size is small so statistical power can be compromised and this could be the reason for the non significant results. This problem is aggravated by the case-crossover design which is less efficient statistically than Poisson time series analysis. At the same time, cases are separated in two groups accordingly to the two different geographical location of the cases. I suggest combining the two groups and to include geographical site as a covariable in the models, or alternatively explain the rationale for not doing it.

3. The authors should explain why they have not considered all pollutants jointly in a model to assess their independent effects.

4. The authors should comment on the contradictory results for NO, which apparently increased risk in Manchester and decreased it in Liverpool. Could these results be just artifacts?

Minor revisions:

1. In my opinion, not correcting for multiple comparisons is not so important in this study.
2. Table 4. Omit symbol ‖ for p<0.05 in table and in footnote.

In brief, this is an interesting paper on a relevant matter, but several important limitations in the subjects included, the exposure measures, the analyses performed and the results obtained, imply that essential corrections are needed.

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

**Quality of written English:** Acceptable

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

**Declaration of competing interests:**

'I declare that I have no competing interests'