Reviewer's report

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

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Reviewer: Dolores Catelan

Reviewer's report:

The paper investigates the association between traffic-related air pollution and ER visits for respiratory symptoms in the city of Palermo (Sicily, Italy).

The paper is interesting for the journal but needs some revisions to be suitable for publication.

The manuscript has some interesting results but limitations of the design of the study (see specific comments) have to be clearly addressed at least in the Discussion section. My feeling is that the authors are too confident on their "statistically significant" results. I suggest to put more emphasis on the limitation (both statistical and epidemiological) of this study.

1) Major compulsory revision.

Introduction

I feel like the introduction does not give a full explanation about both the state-of-the-art on ER visits for respiratory disorders and air pollution and the motivation of the study.

At the end of the Introduction section I still do not have a clear idea of what is already present in the literature, why we need a new study and what is the new point address by the Study.

What are the mail results of the cited paper? My suggestion is to summarize a couple of reviews.

The authors have to explain in the introduction why they decided to conduct a study in the city of Palermo. Otherwise it seems like an exercise driven by the availability of the data.

Methods

Health data

The authors included 5 over 6 ER hospitals in the study. I wonder if the selection criterion are sufficient to exclude major selection bias. The authors must say something about this point, in particular in the discussion section.

Statistical Analysis

The authors say “Because the case and the control were the same person, confounders related to individual factors (e.g. age, gender, smoking habits,
nutrition conditions, and so on) as well as time invariant or slowly varying risk factors were controlled by design.[18] This is correct only when you perform the analysis stratifying by individual. I do not think authors did the analysis in this way.

This study is at the ecological rather than individual level. This raises at least two statistical problem. First, the estimated association between pollution and health outcome association may not be the same as the desired individual level relationship, due to the possible presence of ecological bias.

Second, pollution and health data are spatially misaligned. Indeed pollution is measured at a fixed number of monitoring station. The monitored concentrations are averaged to obtain a single value to be used in the statistical model.

The authors have to address these points at least in the Discussion section.

The authors included temperature as a quadratic term in the model. I think this is a very poor choice. Why do not consider a lagged effect also for this meteorological variable? I think the authors need to consider also interaction terms between temperature and age classes.

Did the authors consider other meteorological variables in the analysis (e.g humidity)?

At page 9, the authors say that “…95% Confidence Interval were used to assess statistical significance”. Following Stern and Smith (Sifting the evidence—what’s wrong with significance tests? BMJ, 2001), I strongly suggest to not confused 95% CI with statistical significance and thus to report 90% CI together with the numerical p-values associated with the effect measure (Ors).

2). I also suggest to avoid the use of the expression “statistically significant” in the manuscript.

Minor comments.

Methods

The authors say that “diagnoses at each ER visit were coded by experienced medical record nosologists… ”: how many? Are the same in the five hospitals?

Results

In table 3 results are reported as ORs in relation to an increase of 10 µg/m3 of pollutants.

The authors need to reports in the paper (and abstract) the results accordingly (I mean they need to explicitly write that ORs referred to that increase in pollutant concentrations).

Titles of Tables and Figures are not self-explanatory.

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published
Declaration of competing interests:

I declare that I have no competing interests.