Reviewer’s report

Title: Changes in triggering of ST-elevation myocardial infarction by particulate air pollution in Monroe County, New York over time: a case-crossover study

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Reviewer: Michela Baccini

Reviewer's report:

I thank the authors for having replied to most of my comments/questions. However, in my opinion few relevant points are still not satisfactorily addressed.

Major comments

1) The first point concerns the influenza epidemics. I understand the reasoning of the authors about the fact that in principle influenza could be on the causal pathway between exposure and outcome (if there are studies about this in the literature, I suggest to cite them in the paper), but I would like to stress that in time series analysis and case-crossover analysis of epidemiological time series, adjustment for this factor is usually done. Therefore, a discussion about this point is needed. A sensitivity analysis would be appreciated too, if information about influenza epidemics is available.

2) It seems to me not appropriate that the choice of the functional form for temperature in the model is done through AIC. When the aim is to adjust for a relevant confounder, the knowledge about the phenomenon arising from the literature should be accounted for, and the evaluation of the strength of the confounder should be based on the comparison between the effect estimate (the exposure effect) obtained under alternative models for the confounder (the model with linear temperature and the model with non linear temperature). Finally, the AIC values reported by the authors, for some lag, seem to not exclude a possible minimum after 4 df.

3) I appreciate the additional analysis to check the relevance of a possible interaction with clinical/individual characteristics. However, in the response, in the paper and in the caption, the authors write that they estimated a model with an additional interaction term between air pollutant and patients’ characteristic. I'm not sure, that this is what they did. In fact, such kind of model would have provided only three effects estimates (before, during, after), the same for patients with different characteristics. Are the results reported in Table S4 from separate models conducted on subset of patients with specific characteristics?
4) In discussing the effect modification due to the period, did you consider the role of the shape of the exposure-response function? Could the lower effect estimated in the AFTER period attributable to possible non-linearity of the exposure-response function?

Minor comments

1) About the IQRs used to scale effect estimates, are the IQRs estimated for the control periods different from the IQRs estimated for the entire study period? It seems to me that the distinction is fleeting and probably unnecessary.

2) Why are p-values reported in Table 3, but not in Table 4? I suggest to uniform the two tables (you could eliminate p-values from Table 3).

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