Reviewer’s report

Title: Desert dust outbreaks and respiratory morbidity in Athens, Greece.

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Reviewer: Massimo Stafoggia

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The authors investigate the association between desert dust outbreaks and respiratory morbidity in Athens, Greece. Furthermore, they try to address the (possibly) independent effect of PM10 concentrations, also adjusting for co-pollutants. Compared with previous publications, the authors limit the analyses to a small sample of days during their study period: 132 with dust advections, and 177 matched days with no advections, and similar distributions in terms of days of the week-month-year, temperature and humidity.

In general, the paper is interesting in its field, because it addresses a topic with conflicting evidence, and it does so focusing on an outcome, respiratory morbidity, with clear a priori biological plausibility. However, there are several methodological choices which require further clarification.

1. Identification of desert dust days. The authors applied a combination of back-trajectories and high PM levels in rural stations, and they identified 10% of the days during the study period, 2001-2006, as dust affected. I would strongly suggest the authors to use, as sensitivity analyses, other approaches, such as atmospheric models for example, and see whether additional days are identified. Furthermore, there are several approaches suggested in literature to quantify the dust burden attributable to desert episodes. These might be applied to better select cases and controls.

2. Case-control approach. It is not clear how the case-control matching is retained in the analysis, and how the authors solve the problem of discontinuous time series introduced by the selection of the studied days. In other words, by including only 300 days in the analysis, all the advantages of time series approaches are lost, and it is not clear how the use of dummy variables for year:month can solve this problem. Furthermore, while it makes sense to match control days to case days by some parameters (DOW, month, temperature), it is not clear how such matching is accounted for in the following Poisson model. I would suggest the authors to apply, as sensitivity approach, an alternative model where the whole time series is used. In this way the estimate for PM10 would be more robust and reliable, whereas the estimate now provided is not, because it only refers to 300 days and it is not clear how it is driven by other time-varying factors such as season and meteorology (for example more dust days occurring on specific seasons, and the same for control days since they are matched by month and temperature).
3. Study period. It is 2001-2006. Authors should do their best to expand to a more recent period. Since the study is single-city, it is not so novel and powerful. Authors should try to expand the series and do additional analyses to provide new result in a study not entirely original from the start.

4. Role of dust in the PM-morbidity relationship. It is not clear, from the study hypothesis, whether dust occurrence is considered as a confounder or an effect modifier in the PM-morbidity association. This is quite crucial for the interpretation of the results. I would suggest the authors to provide a DAG and to explain the meaning of the arrows pointing to resp. morbidity in terms of independent vs confounded or mediated effects of dust and PM.

5. Distribution of other pollutants. Finally, the authors should explain in more details why the other pollutants are higher on non-dust days. Is it because anthropogenic emissions are lower on dust days, or because dust episodes tend to occur on periods with lower pollution in general? This might explain why PM is only moderately higher on dust days compared with non-dust days.

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