Reviewer’s report

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

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Reviewer: Stefanie Sarnat

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The authors present results of a time-series study assessing associations of desert dust days, PM10, and respiratory emergency department (ED) visits and hospital admissions during 2001-2006 in Athens, Greece. Transported desert dust can alter local PM size distribution and composition, including potentially health-relevant biogenic content. This manuscript thus tackles an important topic and adds to the literature on transported desert dust and health in Europe. Overall, the manuscript could be strengthened by inclusion of additional details and interpretation, as well as results of sensitivity analyses to support observed associations.

MAJOR COMMENTS

1. Focus of analyses considering desert dust effects. The authors suggest that desert dust may contribute to increased respiratory morbidity through increased PM10 levels as well as by altering PM size and composition. While the authors consider modification of dust effects by PM10, the majority of the manuscript is focused on desert dust effects adjusted by PM10 (and vice versa). The authors should discuss and interpret this focus more throughout - i.e., PM10 is not a traditional confounder in this context; and, by controlling for PM10 in the analysis, one can perhaps tease apart the non-mass related effects dust days. While the authors touch on this in the discussion, justification and interpretation of the different forms of analyses (adjustment vs. effect modification) could be clarified throughout.

2. Identification of desert dust days. Ultimately, the authors find little difference in PM10 levels between dust and non-dust days (Table 1), and they do not find any modification of desert dust effects by PM10 (lines 123-124). Some additional discussion on how desert dust days were identified is warranted. The authors classify days as a 'desert dust day' if: a) air mass transport occurred from the Sahara or the Arabian Peninsula (using back-trajectory analysis); and, b) if the ratio of PM10 from a remote to a centrally-located monitor was high (greater than the median for the year). Can the authors can clarify the second criterion? By remote vs. centrally-located monitors, do the authors mean rural (outside of Athens) vs. urban (inside the study area)? And, if so, on days with high desert dust, would these impacts be regional and thus impact both monitors similarly and/or simultaneously? Why not specify the criterion as high PM10 levels at the central site (relative to the season)?
3. Descriptive comparison of dust and non-dust days. The authors provide some characterization of desert dust days in Table 1, in which comparisons with non-dust days on various factors are made. Some additional characterization regarding the temporality and occurrence of desert dust days throughout the 6-year dataset is warranted. For example, when do dust days tend to occur (e.g., in certain seasons and/or years)?

4. Magnitude of effect estimates for desert dust days. The authors consistently observe very strong associations of desert dust days and respiratory morbidity (with estimates of ~50% increase risk of ED visits or admissions on dust days compared to other days). This magnitude of association is much larger than in other air pollution time-series studies, in which % increases in risk of 1-5% are usual (as observed for PM10 in this study). How do these results compare to previous studies of desert dust and health? Could uncontrolled confounding explain these very strong associations? To negate such concerns, the authors might present results of sensitivity analyses with tighter control for time (see minor comment 6 below), and consideration of other episodic environmental factors (e.g., pollen levels?) that could plausibly confound the observed desert dust effects.

MINOR COMMENTS

1. Lines 33-34 - the authors indicate that desert dust outbreaks alter particle size distribution and composition. Some additional discussion is warranted in the introduction on how/why PM size and composition is altered during dust outbreaks, and clarification on what the comparison is (i.e., altered compared to 'general' urban PM in Europe)?

2. Lines 61-64 - the authors selected 'control days' (or non-dust days) for descriptive comparison with the identified desert dust days. Control days were selected as the same day of week, same season, and similar temperature and humidity as the dust day. Please clarify how was season specified here - i.e., within the same season and calendar year? If not within the same calendar year, then perhaps some of the differences (or lack thereof) in comparison between day types could be due to long-term temporal trends.

3. Lines 65-66 - please specify the temporal metrics of daily air pollution concentrations of interest; were these all daily 24-hr average data used in the epidemiologic models?

4. Lines 67-69 - how many hospitals provided data towards the study, and approximately what fraction of respiratory ED visits and hospital admissions among the Athens population were captured?
5. Lines 80-81 - please specify the lag structure used to assess associations between dust days, pollution, and respiratory morbidity. How was lag structure determined?

6. Lines 80-81 - the authors controlled for long-term time trends and seasonality using a two-way interaction between year and month. Given within month trends in respiratory morbidity during some times of the year, I wonder whether smooth control for time may be needed in this analysis.

7. Lines 80-81 - if I have interpreted the text correctly, it appears that all 6-years of data were included in the main Poisson models (not only the selected dust and non-dust days characterized in Table 1). If so, the authors should clarify that the dust effects are in comparison to all other days in the time series (i.e., methods, results text, and tables).

8. Lines 89-91 - Table 1 shows the descriptive comparison of desert dust vs. non-dust days. Since these days combined make up only a small subset of the full 6-year dataset used in the Poisson models, I suggest including in Table 1, or as a separate table, descriptives of the full time period.

9. Lines 94-96 - patient age was missing for 14% of visits. What is the basis for replacing these missing values with the mean age of patients with the same diagnosis? I imagine that doing so adds noise to stratified analyses assessing effect modification by age. I suggest reporting results for age stratified analyses excluding those visits with missing age information.

10. Lines 155-156 - please provide further discussion of the advantage of focusing on respiratory morbidity; what do the authors mean by 'plausible biological explanation'?

11. Table 4 - please clarify the results layout. Are all results adjusted for the stated pollutant (NO2 or O3) and then PM10 and dust are added individually and combined, in addition to the gases already in the model? Inclusion of the single-pollutant associations for NO2 and O3 would provide a more complete picture of the results.

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