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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Saharan dust levels in Greece and received inhalation doses" *by* C. Mitsakou et al.

Anonymous Referee #3

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The manuscript entitled "Saharan dust levels in Greece and received inhalation doses" by Mitsakou et al., present the analysis of the outputs of SKIRON model in order to evaluate the impact of long-range transport from Sahara to PM10 exceedances and inhalation doses in major urban areas in Greece. The manuscript provides some insights in the importance of dust on air quality and public health by combining the outputs of two models but it lacks sufficient measurement evidence to support that the suggested inhalation dose of 600 μg/day is sound and reliable, which may result in ineffective or no-action policies and mandates by the state government to address the air pollution in Greece. More specifically:

Introduction

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Interactive Discussion
Discussion Paper

- *The end of the second paragraph, page 2 "In a recent study,..... from Sahara dust transport (Kallos et al., 2007)":* The cited reference is not a peer-review publication. Previous studies in the region suggested that Sahara dust is an important component of PM and maybe an important contributor to PM_{10} exceedances especially in rural and background environments, but its significance is diminished in urban areas that are characterized by dry conditions because of the contribution of mechanically-generated dust from paved/unpaved roads and construction activities. If this statement relies on the data presented here, authors should not cite this reference. I would strongly suggest to either modify and, cite other peer-review references, or remove the statement.

Methodology

- *General Statement:* Given the accumulated knowledge that the DREAM model did not accurately model dust concentrations from Sahara and transport over southern Europe, as compared to well-established GOCART and DEAD models, and that there is no published evidence of how the outputs of SKIRON models compare against ground measurements, it is strongly advised to include a description of the accuracy, performance and sensitivity of the SKIRON model in this publication. This is further evidenced by this manuscript in which (Figure 3), the regression analysis indicated poor-to-moderate temporal correlations between measured and modeled PM_{10}concentrations, with dust levels being one order of magnitude higher than PM_{10} mass concentrations.

- *Section 2.1.2:* The air quality site in Thrakomakedones is primarily used to address the magnitude of the photochemistry pollution in Athens, and usually records the highest ozone concentration in Athens during summer. While the site is classified as urban background, it is merely true that the site is free of anthropogenic impacts because (i) it is located in an area that experienced fast

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growth and new developments for the Olympic Games in Athens with the local municipalities being among the most populated in Athens metropolitan area; (ii) a rather extend network of unpaved roads and minimum (if any) maintenance of paved roads, while the soil texture in the area in described by fine (less than 50μm) particles and (iii) the site is located in a canyon between the Aegaleo and Parnitha mountains which connects the Athens valley to Thriasio valley, a heavily industrialized and polluted region. To address these issues, I would strongly suggest that authors include more sites of the air quality network including those located upwind of downtown Athens, like Piraeus and in Thriasio Pedion to substract cases in which dust is related to local activities. If this is not possible, authors should at least make clear statements throughout the manuscript (abstact, discussion and conclusions) that their estimates are on the high-end and may include contributions of local or regional anthropogenic sources that tend to be observed regularly and consistently.

- *Section 2.1.3, first paragraph "Thus,..... monitoring stations"*. Authors mentioned in the previous paragraph that PM_{10} concentration measured using b-attenuation monitors which provide 5-15 minute average values and definitely hourly values. Why authors decided to use 24-hr averages? Is it possible to examine the 1-hr data, since they are going to provide significant information to confirm that PM_{10} episodes were, indeed, caused by long-range transport as compared to local sources? For example, if PM_{10} is associated with mechanically-resuspended road dust, it should follow a pattern that is similar to traffic.

- *Section 2.1.3, Page 6, end of the paragraph:* Please provide more information on the weight function and related references to support that this function is reasonable.

Results and Discussion

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- *Section 3.1, First paragraph, "The histograms in..... monitoring station" and: Figure 2.* Use box plots showing the mean, median, 10% and 90% percentiles for the box boundaries and minimum and maximum for the whiskers. The diagrams should be for each month/year starting from January 2003 to December 2006, in order to obtain information of the monthly and annual variations. This will be more beneficial for the authors and the readers to understand the temporal patterns as well as the range of PM_{10} concentrations. (In fact, authors discussed about the range of PM_{10} concentration in the second paragraph).

- *Second paragraph (middle) "According to the above analysis..... mineral dust transport*". It is well known that Saharan dust outbursts tend to occur in mid-spring and late fall. The cited peer-review references analyzed the seasonal patterns of PM10 in background location with minimal (or absent) contributions of anthropogenic sources. There is accumulated evidence that, for example, road dust emissions are substantially higher in spring. In addition, despite that there is a substantial variation of $PM_{2.5}$ sources between winter and summer (e.g. central heating), there is no monthly variation of $PM_{2.5}$ levels in Athens. Authors should revisit this statement and include the possible contribution of other sources as well as peer-review publications on the seasonal variation of PM_{10} in urban areas.

- *Page 7-8, end of paragraph "Furthermore..... is at the southeast part":* Is there any documentation showing that emissions from these industrial settings are higher during winter? Could authors provide more evidence on that? Maybe emissions inventories of PM_{10}?

- *Page 8, second paragraph, Figure 4 and Table 2*: How authors evaluate the relatively poor correlation coefficients? Is this due to the contributions of other coarse (or fine) particle sources? What the R would be if low PM_{10} levels (e.g less than $50 \mu g/m^3$) were to be excluded from the analysis? Wouldn't this be a more reasonable approach since authors are focused on PM_{10} exceedances

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only?

- *Page 8, last paragraph:* Authors need to provide more insights on the important of slope and intercept. If the notion of slope being indicative of dust contributions and the intercept being suggestive of urban background, then, for example, for the city of Heraclion, dust accounts for about 20-40 μg/m^3 of PM_{10} (since dust is responsible for $\sim$40% of PM_{10}), while in Table 3, it is reported that average dust concentrations varied from 5.3-9.3 μg/m^3. Please clarify this discrepancy?

- *Page 9, "A more comprehensive..... urban atmosphere*": While sea salt spray may be of significant importance on aerosol mass for background and remote areas, it is highly unlike that they constitute a significant fraction of PM in urban environments. Authors should consult a large body of source apportionment studies (papers by Philip Hopke). The low contribution of sea salt in areas that are influences by anthropogenic sources was also indicated in a detailed particle characterization and source apportionment study of 160 remote location in US including sites in Hawaii and along the east and west coasts. Authors should revisit this statement taking into account the significant body of peer-review publication on sources of PM in urban areas.

Overall, the manuscript presents an effort to address the significance of windblown dust from Sahara on PM10 levels in urban areas. Authors clearly present the relationships between dust levels, local air quality and inhaled doses but they omit to address the assumptions and limits of their approach, including the contribution of other sources of coarse particles. This can be easily solved by examining other sites in Athens and the background site in Aliartos, or if available, the background site in Crete. At minimum, authors should clearly state the limitations of their analysis in their manuscript. As a result, the manuscript may be accepted for publication at the journal with extensive revisions.

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Interactive Comment