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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.

C. Mitsakou et al.

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The authors' responses to the comments are provided below, along with the reviewer's comments (in italics):

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 PM_{10} 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.

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Reply: The purpose of this work is not to introduce or propose new policies but simply to draw the attention of the scientific community on the inhaled amounts of desert dust at different exposure levels. The dose of 600 μg/day is not proposed as any threshold on defining policies; the calculated inhaled amounts of dust are considerable since they are comparable to the respective doses in known polluted environments (i.e. heavily-polluted urban or smoking areas). Having in mind that under severe dust episodes, recorded concentrations even 2500 μg/m^3 are evident in the Mediterranean (for example in Crete, see Gerasopoulos et al., 2006) the 600 μg/day dose is a rather conservative quantity (note that we assumed a typical daily exposure of only 6 hours). Moreover, our simulations are not without sufficient support from observations. We have analyzed the PM_{10} records from the national monitoring network; we correlated model outputs with these observations and checked the validity of the results. We believe the arguments of this work are well justified because they are based on observational evidence and results from well tested modelling tools. We made an attempt to better clarify some issues in the revised version of the manuscript.

Introduction 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.

Reply: We agree with the reviewer's comment, this is not a cited reference. Although this is a twice- reviewed report by the Greek Ministry of Environment and submitted also to EU (DG Environment). We provided this reference mainly for readers studying

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air quality in Greece. This report is also written in English and is available upon request.

It is worth mentioning here that such analysis is regularly performed in other south-European countries and especially in Spain and Italy, where there is a plethora of related peer-review publications. We added some of these in our revised manuscript. As it is cited in such publications, the contribution of Saharan dust to PM_{10} exceedances in both rural and urban areas is without doubt (see Querol et al., 2001; Rodriguez et al., 2002; 2003; Perrino et al., 2008). We disagree with the reviewer on his characterization "its significance is diminished in urban areas" for a simple reason: the partitioning is different because of the superimposed contribution from traffic, unpaved roads and/or construction, but the quantity remains the same. Usually, all monitoring stations exhibit high PM_{10} concentrations during an episode, since the dust plume occupies large areas for many hours at least, while satellites like MODIS provide data on dust plumes towards areas under consideration.

Obviously we dealt with urban areas for two reasons, firstly because the largest part of population resides in urban areas, where we have significant contribution from anthropogenic sources, as the referee mentioned, and therefore the exceedances occur more often and secondarily due to the fact that most of the monitoring stations, at least in Greece, are located in urban areas.

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.

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Reply: It would be very helpful for us as model developers to share the "accumulated knowledge" the reviewer has concerning the bad SKIRON/Dust or DREAM (as called from others) performance and especially related to GOCART and DEAD. We are surprised by the reviewer's comment concerning the "inaccuracy" of a limited-area model with high resolution and the "high accuracy" of global models that are known for significant underestimation of dust amounts. As stated in the abstract of Zender et al. (2003) "... The model (DEAD) underestimates the transport and deposition... An underestimate of long-range transport of particles ...". In the same publication, it is stated that "The lower estimates of DEAD are much closer than GOCART to Prospero [1996b] ...". These are a few comments from the developer of DEAD model, Dr. C. Zender. Similar comments concerning the performance of global models dealing with the dust cycle could be found in many publications. It is surprising to have a suggestion for using a global model to study dust in urban environments.
SKIRON/Dust or DREAM models have been widely applied for the determination of desert dust production, transport and deposition with satisfactory predictions, so in addition to the studies referenced in the manuscript for the evaluation of SKIRON model we would like to inform the reviewer on some relevant peer-review studies carried out during the last years and in other countries:

Rodriguez, S., X. Querol, A. Alastuey, G. Kallos, O. Kakaliagou, Atmospheric Environment, 35: 2433-2447, 2001.
Rodriguez, S., X. Querol, A. Alastuey, M. Viana, Environmental Science and Technology, 37: 216-222, 2003.
Balis, D. S., V. Amiridis, S. Nickovic, A. Papayannis, C. Zerefos, Geophysical Research Letters, 31, doi: 10.1029/2004GL019881, 2004.
Viana, M., C. Perez, X. Querol, A. Alastuey, S. Nickovic, J. M. Baldasano, Atmospheric Environment, 39: 5343-5361, 2005.
Astitha, M., G. Kallos, P. Katsafados, Atmospheric Research, 89: 358-364, 2008.

However, in order to better support the validity of the model results in the manuscript,

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we cited the above works and made the necessary changes in the revised text.
We consider the reviewer's suggestion on organizing a validation exercise as outside of the scope of the present study. Such exercises have been done in the past even from other groups where the operational results could be on regular use (e.g. Papayannis et al., 2008).

By the phrase "...with dust levels being one order of magnitude higher than PM_{10} mass concentrations" the reviewer obviously compares the monthly averaged PM_{10} values of Fig.2 with the daily dust concentration values of Fig.3. Daily-averaged values of dust concentration during dust episodes obviously exceed the monthly-averaged PM_{10} values (dust outbursts are episodic with high peaks). However, after the reviewer's suggestion we replaced the histograms of Fig.2 with box plots in order to gather more information on the PM_{10} measured values (min/max values, 10%, 90% percentiles).

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 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 contribu-

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tions of local or regional anthropogenic sources that tend to be observed regularly and consistently.

Reply: First of all, we find inconsistency between the reviewer's description and his comments. The characterization of the selected area as suburban-background comes from the Greek Ministry of Environment (www.minenv.gr). The Olympic installations are far away from the monitoring station (few km), while it is known that the dust produced by construction settles quite fast because of the high granulation and other unfavorable conditions. Even the reviewer claims that the soil texture of the area produces particles less than 50 μm. As it is known, in PM_{10} monitoring instruments, particles larger than this size (PM_{10}) are excluded from sampling. Only in TSP measurements particles larger than PM_{10} are considered. As far as we know (from personal visit) there are not that many unpaved roads in the vicinity of the monitoring station and of course with heavy traffic that can produce dust clouds. In addition to this, it is worth-mentioning that most of the area is covered by pine trees that assist in dumping of locally produced dust. The residential area of Thrakomakedones is not located in a canyon between the Aegaleo and Parnitha mountains as the reviewer describes, but in the foothills of the mountainous region of Parnitha. The area is well protected from such transport from the industrial area as the reviewer states. As a conclusion, we selected the specific monitoring station as a site with small exposure from local sources of particulate matter as compared to the other areas of the Athens basin.

One issue that is missing from the reviewer is that the used SKIRON/Dust model configuration has horizontal grid spacing of 0.24° x 0.24° ($\sim$ 25 km x 27 km), while the Athens basin has the minor axis 17 km (E-W) and the major 27 km (N-S) by approximation.

Based on the above, the last suggestions of the reviewer to include more sites of the monitoring network inside the Athens basin will not affect our analysis. We have done such analyses but no additional information was mined, except the variability in total PM_{10} concentration due to local effects and the gaps on monitoring time series. Actually, this issue is well known from previous works related to air pollution in Athens

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basin.

Section 2.1.3, first paragraph "Thus, ... monitoring stations". Authors mentioned in the previous paragraph that PM_{10} concentration measured using β-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.

Reply: Here, we do not examine all factors controlling the variation of PM_{10} concentrations in the different Greek urban areas, but we made an attempt to determine the influence of the long-range dust transport. Dust transport episodes last for at least one day (regularly 2-5 days), where high PM concentrations are recorded day and night hours. Under such situations, local variability becomes secondary contribution to the recorded amounts. Therefore, an average daily value represents dust outbreaks more realistically without uncertainties associated to the daily cycle.

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.

Reply: In the version of the manuscript that appears in the ACPD website (revised after the first reviewer's comments), it is clarified that the weight function is based on inverse distance weighting. In any case, the horizontal grid resolution of the model is 0.24°, so the accurate localization of the monitoring stations does not provide significant advancements, having in mind the topographic characteristics of the Athens basin as described in previous comment. Dust plumes have horizontal extends of the order of hundreds of km and in the vertical up to 4-5 km. So if there is a dust plume over Athens, it is almost independent of the position of the stations of the monitoring network. This comment should be valid in case of very high model discretization (e.g.

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3-4 km).

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).

Reply: We partially agree with the reviewer's suggestion to use box plots in Fig.2. However, we disagree with his suggestion to prepare it for each year separately because the 4-year period we used in our analysis is not adequate for consisting dust climatology. The seasonal variability is something useful to demonstrate here, so we prepared Fig.2 based on this concept.

Second paragraph (middle) "According to the above analysis ... mineral dust transport". It is well known that Saharan dust outbursts tend to occur in midspring and late fall. The cited peer-review references analyzed the seasonal patterns of PM_{10} 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.

Reply: The main objective of the present work is focusing on cases with dust outbreaks. During such situations, the transported quantities of desert dust are such high that make the variability (seasonal or daily) from local sources secondary. However,

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PM_{10} peaks in spring and autumn cannot be solely attributed to desert dust transport, but there is accumulated contribution from local sources (e.g. traffic). In any case, the seasonal variability of local sources, such as road dust emissions, is not that significant in Athens as in north-European cities. This is mainly attributed to the fact that the use of wheel-chains and salt on the roads during winter and early spring is limited to no more than 2-3 days every few years. The respective paragraph in the manuscript has been re-written in order to reflect the situation described above.
We do not know the source of $PM_{2.5}$ measurements the reviewer has for Athens, but the Greek Ministry of Environment has small records with large gaps that are difficult to be used for a clear imaging.

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}?

Reply: The phrase in the previous version of the manuscript is misleading, we do not claim that the industrial settings at the outskirts of Volos release larger amounts of emissions during winter. There is no information concerning industrial emissions in this town and to our knowledge an emission inventory does not exist. However, having in mind the local topographic and landscape characteristics of the area we can say that stagnation conditions and the contribution from central heating during the cold period of the year may lead in high particle concentrations. Appropriate changes in the text have been made.

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 50μg/m3) were to be excluded from the analysis? Wouldn't this be a more reasonable approach since authors are focused on PM_{10} exceedances only?

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Reply: The methodology we followed is exactly the one proposed by the reviewer, that is low PM_{10} levels are excluded from the analysis. As we clearly state "... in Fig. 4a-d scatter plots between predicted dust concentrations and measured PM_{10} concentrations for the cases of daily exceedances (observed PM_{10} values greater than the daily EU limit of 50 μg m^{-3}) are presented ...". We find this description clear enough.
The relatively low correlation coefficients can be attributed to various reasons, one can be the specific geomorphological characteristics of each area, for example the town of Volos is adjacent to a steep mountain that receives considerable amounts of precipitation, so the washout effect there is important. Another reason that leads to the low correlations is the trade wind system of the area, such as the etesians along the Aegean during the summer that are unfavorable to dust transport from the African continent. In addition, the poor correlations could be also obtained due to the limitations of our atmospheric/dust transport model, namely the global database used for the identification of dust sources and desert dust classification, as well as the relatively low horizontal resolution of the model (0.24° x 0.24°) that was the optimal choice of our operational runs given the wide model domain and the available computer resources. We are currently working on optimizing the above limitations. The necessary clarifications have been included in the revised text.

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/m3 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/m3. Please clarify this discrepancy?

Reply: Here, there is a clear misunderstanding: the scatter plots present the correlations between observed PM_{10} concentrations **for the cases of daily exceedances** and predicted dust concentration values, and thus the slope of the trend line is indicative of dust contribution to PM_{10} concentration **when exceedances occur**. On the

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other hand, Table 2 (not Table 3) reports the annual average dust concentration per year where all days are accounted and not only these with dust contribution to exceedances. An additional clarification has been added in the manuscript to avoid such misunderstandings.

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.

Reply: The quantification of the sea salt contribution to the urban PMs is not within the scope of this paper. For the information of the reviewer we provide some references where sea salt amounts in certain coastal urban areas could be significant (Cetin et al., 2007; Yin et al., 2005). We assume that the reviewer agrees on that there is contribution, small or large, in the subject area.

Overall, the manuscript presents an effort to address the significance of windblown dust from Sahara on PM_{10} 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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Reply: We believe that in the revised manuscript the strong and weak points of the proposed methodology are clearly stated.
The proposed examination of other sites has been done before the drafting of the manuscript. In the present form only the most relevant analysis is provided in order to save space. We hope that the reviewer will agree with our approach.

Finally, we would like to thank the reviewer for the constructive comments and we hope that the revised version is clear enough and misunderstandings cannot occur.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 11967, 2008.

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