Atmos. Chem. Phys. Discuss., 13, C7293–C7298, 2013
www.atmos-chem-phys-discuss.net/13/C7293/2013/

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The regime of desert dust episodes in the Mediterranean based on contemporary satellite observations and ground measurements" *by* A. Gkikas et al.

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Received and published: 25 September 2013

Response to Reviewer 4

We would like to thank the Reviewer for the useful comments that helped us to improve our manuscript. We tried to reduce the length of the manuscript and reproduced the figures in order to improve their quality, according to his suggestions. Below are given point by point answers to the comments (also provided in Italics).

"... The authors conclude that they have characterized a decreasing trend of dust episodes over the years due to the NAO index decrease."

We would like to clarify that in the present manuscript no reference is made to NAO index changes related to tendencies of dust episodes.

"In general, the article is too long compared to the amount of reported results."

The length of revised manuscript has been reduced as much as possible (by more than 3 pages).

"Figures and tables should be reprocessed as often difficult to read."

Done.

"The last section on the backtrajectories provides little information."

This section has been removed from the revised manuscript.

"The "new algorithm" is a combination of threshold, already known and used in the literature. A novelty could be to calculate distribution for each criterion in order to take into account the uncertainties attached to each parameter used."

We cannot fully understand the meaning of this comment. If the Reviewer refers to the uncertainty of obtained results with our algorithm associated with the selection of specific thresholds for each parameter (criteria), due to the existence of different values in literature, we would like to clarify that we tried to make the optimal choice in each case. However, in order to assess the possible uncertainties we have performed sensitivity studies for the selected thresholds and reference to their results is made in lines 391-395.

"A key point of the study is presented in section 3.1: this is not possible to directly correlate surface PM< measurements (even if this is "background" stations) and long-range transport of mineral dust plumes, coming from Sahara and crossing the Mediterranean area at altitudes often between 2000 et 5000m. The authors cite articles clearly showing there is no obvious between these two quantities. However, they used this assumption to validate their algorithm."

We would like to notice that our results do not show that "... is not possible to directly correlate surface PM< measurements ...". On the contrary, in lines 427-429 we state that "The overall comparison is relatively satisfactory taking into account the different nature of compared data, i.e. surface PM measurements against columnar satellite AOD products.". We only demonstrate that problems mainly arise in summer, and we discuss the reasons in our text. In overall, we believe that despite the difficulties in attempting such comparisons useful results can be obtained. Nevertheless, we have made specific references to the difficulties with such comparisons at the beginning of Section 3, lines 406-409.

"Another weak point is the number of data used: there is more available PM and AOD surface measurements, especially in the eastern part of the Mediterranean area. Why the authors did not used all these data? This is a crucial point in case of a statistical study, searching for trends. With the low amount of data used here, the validation of the multi-threshold algorithm is not ensured and the trends results are thus certainly not statistically representative."

Of course, the general availability of surface PM and AERONET stations, at a first look, might seem much higher than the stations considered in our analysis. However, we would like to stress that such stations were selected and used only when satisfying specific criteria on data availability. More specifically: (i) their period of measurements should overlap with ours (2000-2007), (ii) their location must fall within the area with available satellite data, (iii) they should have data on days with identified DD episodes with our algorithm and (iv) they have data with high accuracy (especially for PM). For these reasons the final number of (PM and AERONET) stations was significantly reduced. These have been reported in the manuscript (lines 269-274).

"The abstract is very long and not represents really a synthesis of the results."

The length of the abstract has been reduced by 8 lines.

"The classification of the dust episodes needs some clarification: a common episode

lasts 1 day, a strong episode 6 days and an extreme one 4 days: how do the authors use the words "strong" and "extreme"?"

We would like to clarify that the terms "strong" and "extreme" for DD episodes are defined based on the intensity, i.e. AOD values, of the episodes. This is explained in the text, Section 2.5, lines 304-308. The duration (number of days) and the intensity (strong and extreme) of episodes are two independent characteristics. In lines 587-591 (Section 4.1.3), where reference to the duration of 4 and 6 days is made, we just refer to the maximum duration of strong and extreme DD episodes, respectively (i.e. specific episodes).

"The introduction is very complete and clearly presents the state of the art in this domain. The authors claim that "the novelty of the paper lies in its complete coverage of the region". This is probably true, but not sure: the bibliography on the dust studies in the Mediterranean area is huge and this is always risky to say that we are the first. Even if this is the case, a spatial coverage extension is not really sufficient to justify the publication of a scientific paper in a peer-reviewed journal. I encourage the authors to delete this statement and to more focus on real and important results in the abstract."

We acknowledge that the number of existing studies dealing with dust in the Mediterranean basin is high. However, we would like to remind that our study differs from the others in that it specifically deals with dust episodes. Our specific statements, to which the Reviewer's comment is reported, actually referred to this fact. However, we have removed from or modified in the Abstract and Introduction the relevant sentences.

"The others sections are, in general very long and contains a lot of bibliography: they could be certainly shortened to go directly to the novelties of this work. The conclusions are already well known and review papers exist showing all these results (see for example Scheeren et al., 2003, ACP; Millan et al., 2005, J. of Climate; Rodriguez et al., 2007, Environ. Chem. Lett.; Kulmala et al., 2011, ACP; among others)."

Please note that Section 4.3 has been removed from the revised manuscript. Also, we have added in that references provided by this and another Reviewer (Dr. F. Dulac).

“The table and figures need some work to be readable and useful. The colors in the table are not very useful.”

In the published ACPD paper Table 1 does not have colors. The figures in the revised manuscript have been reproduced as suggested by the Reviewer.

“The figure 1 is not very useful as it: there is no need to have a picture but just informations on the map.”

We believe that Figure 1 is useful because it shows the location of: (i) the PM and (ii) the AERONET stations used for comparisons with our algorithm.

“This map clearly shows there is no data used in the eastern part of the domain.”

Actually, there are stations in the eastern Mediterranean, although with less dense distribution than in the western basin. As for the selection of stations the Reviewer is referred to our answer to his previous comment (first comment of Page C3 in this response).

“The flowchart in figure 2 is not very useful, since only one two lines are different, depending on strong or extreme episodes, and ’land’ and ’sea’. The authors can remove this figure and just write: "over the sea, the additional criterion of r_eff > 0.6 is applied" (to add line 288).”

We prefer to keep the flowchart as it is. It clarifies that the algorithm has been implemented separately over land and sea and clearly depicts the steps of our methodology. In accordance to the Reviewer comment we have added in line 313 the phrase “only over sea”.

“The map in figure 3 is difficult to read: difficult to link the size of the circles to the values. Try another way to express this result. The scatter-plot is not readable: even if point exists for high concentrations/high AOD they are not numerous and are masking

the real information for AOD < 1.5 and PM < 200 ug/m3 (and not ugr/m3).”

In order to improve the readability of Fig. 3-i we have increased the size of the circles. Concerning the scatterplot, we prefer to keep it as it is and not to exclude the points corresponding to high AOD values, because this is not correct from the physical point of view. However, in accordance to a comment of another Reviewer (Dr. F. Dulac) we have replaced the initial Figure 3-ii with four seasonal graphs, which we believe are more readable.

“Figure 5, 6 and 7 are certainly done with basic plot software: the triangles are completely unrealistic and this is not possible to publish results like this. The graphical software interpolates all results and clearly shows the lack of data. Use square to plot values only where data are available, as in Figure 8.”

In the revised manuscript all these figures have been reproduced with another tool in order to avoid the problems mentioned by the Reviewer.

“The Figure 10 has no interest: the trajectories are covering the whole region and we can see nothing.”

The figure, along with the relevant section, has been removed from the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 16247, 2013.