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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.

A. Gkikas et al.

nhatzian@cc.uoi.gr

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Response to Reviewer Francois Dulac

We would like to thank Dr Dulac for his thorough review and the useful comments that much helped us to improve our manuscript. Below are given point by point answers to the comments (also provided in Italics).

Major comments:

"It seems to me absolutely necessary to state all along the paper "intense desert dust (IDD) episodes" rather than "desert dust (DD) episodes" to discriminate the high dust

events of interest in this work from the more common dust events ("intense" is for instance needed in ms. title and title of section 4)."

We agree with the Reviewer's comment on the necessity to make clear that the investigated dust episodes are not common but rather intense. However, the use of term "intense" would probably confuse the readers since already the terms "strong" and "extreme" for dust episodes are used in the manuscript. Therefore, we prefer to just keep using the terms "strong" and "extreme" DD episodes, which are clearly defined. Nevertheless, following the Reviewer's suggestion we added in the title the term "intense" whereas we also added the same word in various cases in the body text of revised manuscript. In line 24, we replaced the phrase 'The regime of desert dust (DD) episodes...' by 'The regime of intense desert dust (DD) episodes...'. Furthermore, in line 292 we have added the word "intense" before "desert dust". Finally, we have also added a sentence in lines 304-308 which helps the reader to understand that in the present study only intense desert dust episodes are considered.

"It seems most unlikely to me that the distribution function of AOD follows a Gaussian distribution. A log-normal distribution is much more probable. This is worth checking because this implies that using the geometric standard deviation rather than the standard deviation to define the threshold AODs would be more appropriate and rigorous."

We agree with the Reviewer's comment that the distribution function of AOD is not a Gaussian but looks more like to a log-normal one. We also agree that, as stated, because of this the geometric standard deviation would be more appropriate than the typical standard deviation that we used. Therefore, we have recomputed the AOD thresholds using the geometric mean and standard deviation values and compared them to the typical ones already used. In general, the geographical patterns are similar for both strong and extreme DD episodes though there are some differences in magnitude. As for strong episodes, these differences are rather small, for example typical AOD thresholds vary within the range 0.4-1.2 and the geometrical thresholds range from 0.4 to 1.6. On the other hand, large differences exist for extreme DD episodes,

with the typical thresholds ranging from 0.6 to 2.2 while the geometric ones varying from 1 to more than 10 with almost their entity being larger than 2. However, such AOD values are extremely rare and using them would be unrealistic from the physical point of view. For these reasons, we prefer to keep using the existing thresholds.

"It seems to me inappropriate that the land class includes both a few pixels in North Africa and most in South Europe, since they have very different statistics in terms of intense dust episodes. Rather than averaging parameters of all land pixels in the study window, it would seem more adapted to have several land pixel classes, e.g. Iberian Peninsula, the rest of southern Europe, Turkey and Middle East, and North-West Africa."

We would like to note that in Table 1 an effort was made to present averaged results over extended subregions (western, central, eastern basin, separately over land and sea) in order to provide an overall aspect of the regime of DD episodes. Of course, further averaging over smaller scale regions like those suggested by the Reviewer can be made as well. However, such information is already inferior to that provided at the pixel level scale, like done in Figure 6 of the paper. However, in order to address the Reviewer's comment, we have produced the seasonal contributions to the total annual frequency of occurrence of strong and extreme DD episodes in a figure.

"The areas covered should be made clearer on a map (land vs sea pixels and limits between the 3 considered basins)."

Done (see modified Figure 1).

"In the methodological section, it seems necessary to me adding a figure that plots the reference (geometric as suggested) average AOD550 in the study window, that could be used to show limits considered between land and sea pixel classes and between the different sub-basins(presently missing), and possibly some desert areas where data are missing. This will help clarifying whether the white colour in Figs 5-8 represents missing data and/or the 0 value. Since threshold AOD values used for strong and ex-

treme episodes are likely to be significantly variable spatially, it seems also necessary to me adding maps of the distribution of those threshold AOD values (AOD+2sigma and AOD+4 sigma)."

According to the Reviewer's suggestion, we have produced in a Figure the geographical distributions of the mean and the associated standard deviation AOD value above the study region, separately over land and sea which show how these quantities vary from a region to another. We would like to clarify that white shaded pixels correspond to cases with missing data. This has been added in the caption of Figure 5 of the paper. Also, we provide in a Figure the geographical distributions of AOD thresholds for strong and extreme episodes separately over land and sea.

"In Fig. 4i, we have low AODs (down to something like 0.11 or 0.12): it is hard for me to believe that this data set is restricted to intense dust cases with an AOD larger than the average AOD of the given pixel +2 sigma. We do need the reference figure that provides the thresholds."

Please note that the values of 0.11 or 0.12 reported by the Reviewer correspond to AERONET measurements, and therefore they are not used in our algorithm but they are just compared to its products. The algorithm makes use of MODIS AOD values, which are higher than 0.4.

"We critically miss information on the overall statistics of the data set. For instance the algorithm needs TOMS-OMI and MODIS coincident data, which are most probably not always available. The ms. deserves additional maps showing, for each pixel and for the 4 seasons (and possibly for the overall period), the total number of days with all requested data available for the search of intense aerosol episodes."

As suggested by the Reviewer, we have produced and give in a Figure the maps showing the number of days for which there are available data from MODIS and TOMS-OMI on an annual and seasonal basis. However, we did not include these figures in the manuscript because it would become further lengthy.

"Fig. 3ii is not appropriate. It must be replaced by several seasonal graphs with appropriate scales to be readable. Robustness of regressions must be checked without the few isolated points at very high AOD (>3). Such very high AODs may look suspicious: as mentioned in section 3.1 when comparing surface PM to MODIS AOD, cloud coverage 80% may be associated with overestimated AODs. We therefore expect that corresponding images are especially screened in order to check the consistency of such extreme values. A supplement table listing the days with all selected extreme events over land and sea would be worth, providing very useful information to readers for further studies of such extreme events, and giving Reviewers a way to check by themselves whether remaining cloud contamination may not be suspected. More generally, since pixels with unusually high AODs are searched and since the total number of events is relatively limited, I would expect that an additional subjective screening of full resolution MODIS data is performed by authors after pixel selection by the objective algorithm, in order to give more confidence that no doubt remains on possible cloud contamination. For instance it would be particularly doubtful if isolated pixels were found in the extreme event category without neighbours in the strong event category."

The initial figure (Figure 3-ii) has been replaced by four seasonal graphs as recommended by the Reviewer. According to the Reviewer's suggestion, we have repeated the comparison omitting the cases with very high AOD (higher than 3) and we have recomputed the seasonal correlation coefficients. Except for summer, the computed correlation coefficients decreased substantially, having values ranging from 0.16 (winter) to 0.38 (autumn). This shows that, indeed, the regression analysis is somewhat sensitive to the number of points considered. However, we believe that the relevant points must be kept in our analysis since the corresponding ground measurements indicate high PM concentrations (> 200 μg/m3) indicating the existence of episodes. According to our analysis, indeed, as already stated in our manuscript and reported by the Reviewer, the AOD values higher than 3 are usually associated with cloud fractions higher than 0.7, thus possibly affecting the accuracy (overestimation) of the satellite retrievals. However, despite this, these cases are characterized by large surface PM

values (200-1300 μg/m3), which shows that desert dust episodes might have occurred. Concerning the suggestion of the Reviewer to provide a table listing the days with extreme events, we would like to note that the overall number of episodes is 333, which makes difficult to list them in a table format in a journal publication. With regards to the Reviewer's comment on possible cloud contamination, we have repeated our analysis and recomputed the number of DD episodes excluding the cases of isolated pixels without neighbours. The results as shown in a Figure that we produced are very similar to the original ones.

"The spotted character of the distribution maps looks a bit suspicious to me. The presence of hot spots or relative maxima far from sources in the middle of the sea (see Figs 5-6) implies a convergence of air masses over the given pixel but could likely result from discontinuous fields due to a lack of data. This is another reason why it is critical to provide statistics on the original data set and to tentatively evaluate how many events could be missed due to missing satellite data. A good way is by looking at surface PM in those background stations where high PM generally means African dust (see for instance the discussion on the representativeness of satellite series in section 2.2 of Moulin et al., JGR, 1998). Looking for possible high AERONET values not seen by MODIS and checking whether they were associated to south trajectories may be a complementary tool."

The statistics on data availability for the operation of the algorithm has been given above (Figure 3). It is true that in overall there is a significant loss of days due to unavailable satellite data. Therefore, we agree with the Reviewer that this is a source of uncertainty in our results and we highlighted this in the Methodology (Section 2.5, lines 316-319). We would also like to note that we agree that ground based measurements could be used as a complementary tool, as the Reviewer pointed out. Nevertheless, we believe that this would not be able to essentially improve the situation, just because the number of stations (e.g. AERONET, PM) is limited with respect to the number of geographical cells in our study (585).

“I do not see the interest and justification of interpolation that generate triangle shapes in Figs 5-7. Those maps should better be plotted using the individual pixels as in Fig 8.”

In the revised manuscript, Figures 5, 6 and 7 have been reproduced, as suggested by the Reviewer, by using information at pixel level.

“At end of section 3.2, it is not correct to state that the red curve in Fig. 4ii is monomodal. Fig. 4ii shows that the ratio between the two modes is definitely not balanced, but the small mode remains clearly present and its absolute concentration appears not so much different in the two data sets.”

We agree and we have accordingly modified the text (lines 500-503).

“The data set of IDD at the pixel scale is pretty low for most pixels. Distribution maps (Figs 5-8) would better be accompanied by maps of associated standard deviations. It is also critical that the total number of events for each pixel is easily known. I suggest to add in Figure 5 a double scale that provides not only the frequency per year as now but also indicates the total number of days over the 7-yr period.”

We believe that the total number of events for each pixel can be easily obtained just by multiplying the values of Figure 5 of the original paper by the number of years, i.e. by 7. However, according to the Reviewer suggestion, we have computed the total number and the associated standard deviations, and provided this information in two figures, which have been included in the supplementary material (Figures S1, S2).

“Continental Spain is over represented in the PM data set with as many as 15 over 21 stations. This should be justified (isn’t there any other public data set available?). It is compulsory to check whether the Spanish pool of data does not drive the conclusions from the PM10-AOD comparison (Fig. 3), e.g. by computing correlations using the two subsets of stations (Spanish peninsula vs. other stations), especially in summer when the correlation is poor.”

It is true that the Iberian Peninsula has most of surface PM stations considered in the study. In order to examine the effect that this may have in the drawn conclusions, according to the Reviewer’s suggestion, we have reproduced the scatterplot comparisons between MODIS AOD and surface PM separately for the Spanish and for the other stations, both on year basis (all period) and for summer. The results show that the correlation coefficients are low in summer, though more in Spanish than in the other stations, while on annual basis the coefficients increase in the other stations but not in Spain. Therefore, it seems that the location of Iberian Peninsula in the western Mediterranean basin, where the discussed problem of vertical extension of aerosols mainly occurs in summer, drastically affects the comparison. A relevant note has been made in the text (line 423).

“The computation of the average duration of events is relatively critical since it is based on data that have a one day resolution if I understand well. It would be appropriate to consider frequency distribution and plotting the number of events that have a duration of 1, 2, 3. . .n days, e.g. for the sea pixels or different sub-basins (e.g. western, central, eastern), and possibly land pixel classes. Because the data set is limited at the pixel scale, it could also be more appropriate to integrate such frequency distributions over several basins or land areas for the other derived parameters, namely frequency and intensity.”

Based on the Reviewer’s comment, we have produced and given in figures frequency distribution histograms for the duration of strong and extreme DD episodes for the whole study region as well as separately for the eastern, central and western basins, and also separately over land and sea for each case. These figures clearly show the predominance of one day duration of DD episodes, resulting in a mean duration which is slightly larger than 1 day. We would like to note that such frequency distributions have no sense for the frequency of episodes and that in our opinion this information is satisfactorily provided in Table 1. On the contrary, the frequency distribution histograms for the intensity of DD episodes were produced and are given in relevant figures.

"In general the ms. cites only relatively recent literature and omits historical papers. Here are several additional references to be considered (see other sections below): Rao et al., Int. J. Remote Sens., 1989; Dayan et al., J. Appl. Meteor., 1991; Dulac et al., JGR, 1992; Jankowiak and Tanré, J. Climate, 1992 Moulin et al., Nature, 1997; Hamonou et al., JGR, 1999; Berthier et al., JGR, 2006; Antoine and Nobileau, J. Geophys. Res., 2006; Bréon et al., Remote Sens. Environ., 2011; Nabat et al., Atmos. Meas. Tech., 2013."

Done (references added).

"I find that we are missing a comparative discussion with results from Gkikas et al., 2009: how much dust episodes do contribute to intense aerosol episodes in the Mediterranean region?"

The comparison of our results (DD episodes) with our previous ones (all episodes, Gkikas et al., 2009) demonstrates that there is a similarity, which shows the significant contribution of DD episodes. More specifically, according to our computations, the contribution is equal to 32.7% and 40.1% for strong DD episodes over land and sea, respectively, while for extreme episodes the corresponding values rise to 49% and 71.5%. These are now mentioned in the text, lines 840-844.

"Figs 5-8: the value 0 should be better marked by a specific colour (e.g. white) which is distinct from the colour used for missing data (e.g. grey)."

We believe that there is already a distinction between zero and missing values since the value 0 is depicted by the darkest blue colour, whereas the white colour is used for missing data.

Other comments:

"Introduction, p.16250, line 7: ("Desert dust aerosols are coarse particles with size ranging from 1 μm to 5 μm" is not a correct citation from Tanré et al. (2001) who conclude that the dust size distribution shows a dominant coarse mode at 1–5 μm and

a secondary mode around 0.5 μm effective radius."

Corrected, lines 60-62.

"Introduction, p.16251, l.10-12 and line 15: satellite-derived AOD and desert dust climatologies started in the 1980s, and 1990s, respectively. Only a few recent references are cited here. Some historical references would be welcome such as Rao et al., Int. J. remote Sensing, 1989 for global AOD, and Jankowiak and Tanré, J. Climate, 1992 and Moulin et al., Nature, 1997, for African dust over the Atlantic."

Done, lines 90-92.

"Introduction, p.16252, l.2: Antoine and Nobileau, JGR, 2006 is missing in the citation list."

Done, line 114.

"Introduction, p.16252, l.8: at the end of paragraph, it is worthwhile reminding that it was reported that several layers of dust from different source regions are often superimposed (Hamonou et al., JGR, 1999)."

Done, lines 120-121.

"Introduction, p.16252, l.11: worth citing more historical case studies starting with Dayan et al., J. Appl. Meteor., 1991 and Dulac et al., JGR, 1992."

Done, line 124.

"Section 2.1, top of p.16254: you might refer to Nabat et al., AMT, 2013, who evaluated monthly-mean AOD from various satellites in the Mediterranean."

Done, lines 185-187.

"Section 2.2, p.16256: give precision on the re-gridding method."

Done, lines 247-248.

“Section 2.2, p.16256: any information and reference on uncertainties in the aerosol index.”

The aerosol index uncertainty is +/- 0.1 and it is driven by the sensor spectral calibration (Torres et al., 2007). This is now indicated in the text (line 222).

“Section 2.4, p.16257 end of section: can you explicit the AERONET uncertainty in Reff?”

Please note that the AERONET effective radius (reff) is neither considered in our algorithm nor in its validation.

“End of section 2.5.5, p. 16261: can you specify a bit changes in FF that can be expected? Gomes et al., JGR, 1990, have reported that FF increases with the wind speed at emission.”

In our sensitivity tests, the changes of FF ranged from 25% to 75%.

“The Methodology section omits to give detail on HYSPLIT trajectory computations; if they were used in the final ms., it is necessary to summarize the methodology used (meteo fields, trajectory duration, computation method. . .).”

The relevant section has been removed from the revised manuscript according to the comments made by all Reviewers.

“The Methodology sections omits providing a definition of the season limits used.”

We could not find in our methodology any reference to “season limits”.

“Section 3.1., p.16263, l.16-18: couldn’t we also have events completely missing in satellite data due to the cloud coverage?”

Yes, but we would like to clarify that in case of missing satellite data our algorithm does not operate. In the relevant part of the paper mentioned by the Reviewer we discuss cases for which the algorithm detected DD episodes.

“Section 3.1., p.16263, l.23-24: could you list the given stations?”

This happened in case of Fontechiarri (Rome) station (indicated in line 476).

“Section 3.2, p.16265, l.7: it looks a bit strange that extreme dust episodes are more common over sea than over land; as stated in section 4.1.2 (p.16266), it should be reminded here which area covers land pixels since data are missing over a large part of North Africa. The argument in p.16266, l.21 is very general and not only applies to the present section, it should be discussed on a more general way in the methodological section (see also Major comment 3b).”

We agree with the Reviewer and we made a relevant note in the Methodology (lines 316-319). In addition a reminder has been added in Section 4.1.1 (lines 517-519).

“Section 4.1.3., p.16267: rather use “0.1 day” than “a decimal point”; the 1 day resolution of data seems critical to assess the duration; the weak difference found between land and ocean pixels is most probably unsignificant (provide std dev.); any bias due to missing data?”

We prefer to use the term “decimal point” rather than “0.1 day” because by this we point to the digits of the numbers, referring to the duration of episodes, which are provided in the discussion. The one day resolution of our data is the best possible one from the observational satellite point of view. Therefore, we have had to work with this resolution since our algorithm solely relies on satellite data. Of course, we acknowledge that this is a limitation, especially for the duration of the events, which would be relieved if we were to work with ground based, e.g. AERONET, data.

“Section 4.2.1 on seasonal variability: isn’t there any bias due to very variable cloud coverage with the season?”

We agree and we made a relevant note in lines 634-636.

“Section Other studies, p.16269: what about Antoine and Nobileau, JGR, 2006?”

Done, line 644.

“P.16271-16272: provide the significance level when trends are statistically significant; when they are not statistically significant (section Intensity), it is not appropriate to comment trends.”

In the revised manuscript, the linear regression fit lines and the associated discussion were removed from Figure 9 and the text (Section 4.2.2), respectively. Now we only refer to statistically significant tendencies over land.

“Section 4.3: 3000 m a.g.l. should better be considered in the lower free troposphere than in the boundary layer; Figure 10 does not provide useful information and should be omitted; the whole section 4.3 does not appear very useful since we do not know the vertical distribution of dust during the various episodes, and because we cannot exclude that similar trajectories associated to intense dust events may also be associated to “normal” dust events, or no dust conditions. This whole section can probably be omitted.”

The relevant section has been removed from the revised manuscript.

“Section 5, p.16276, l.12-13: this conclusion is bound to the limits in land areas which should be reminded here.”

We agree and we made a note in Conclusions, line 848.

“Section 5, p.16277, l.2: conclusion on the role of increasing precipitation in the decreasing trend in intense dust episodes does not seem supported by results in the ms.”

The relevant conclusion has been removed from the text in the revised manuscript.

“Fig. 9: legends inside graphs are unreadable; I suggest to specify “of monthly mean” in the legend; it looks that there are some particularly high monthly average AOD values which result from very small numbers of pixels, particularly for the seawater

pixels (probably the case in Nov. 2004 for Strong events and in Jan 2004 for Extreme events) which puts some doubt on their use in the trend computation.”

The caption of Figure 9 has been changed. In order to remove any uncertainty of the results of Figure’s 9 red lines (intensity) we have produced and provide in Fig. S3 (supplement file) the monthly number of pixel-level DD episodes.

Technical comments:

“Introduction, p.16251, l.26-27: replace “evident” by “intense”.”

Done (line 109).

“Introduction, p.16252, l.2: replace “in” by “over”; Antoine and Nobileau, JGR, 2006 is missing in the citation list.”

Done (line 113).

“Introduction, p.16253, l.3: specify the pixel scale in the parenthesis (1° x 1°).”

Done (line 147).

“2.1 MODIS Terra, p.16253, line 17-19: sentence unclear to me.”

In this sentence, we were notifying that, in contrast to the present study which uses MODIS-Terra data, MODIS-Aqua ones will be used in another study, which is in preparation. In that study, the algorithm makes use of data from A-Train satellites only, ensuring close overpass times (e.g. between Aqua and TOMS-OMI). However, to avoid confusion we have removed this sentence from the manuscript.

“2.1 MODIS Terra, p.16253, line 24-25: unnecessary citation of Remer et al., 2008 and Levy et al., 2010, both recited in the following sentence.”

Please note that in the next sentence the cited work is Remer et al. (2002) and not Remer et al. (2008). Moreover, the two sentences refer to different issues, so we preferred to keep all references.

“2.1 MODIS Terra, end of p.16253: the sentence on MODIS AOD accuracy should better be shifted next page, line 9.”

Done (lines 179-185).

“2.1 MODIS Terra, p.16254, l.9: specify “the 7-yr period”.”

We could not find the relevant text in the manuscript.

“2.1 MODIS Terra, p.16254, l.17: not clear to me what “flux” refers to here.”

This part, taken from Remer and Kaufman (2005) should refer to the retrieval algorithm and the calculated radiative fluxes (look up tables).

“2.5 Methodology, p.16257, l.21: “It consists in” rather than “It is consisted of”.”

Done (lines 293-294).

“2.5 Methodology and Fig. 2: harmonize the notation for the standard deviation (STD or STDV).”

STDV is now used in Figure 2.

“2.5 Methodology, p.16258, l.1: use “aerosol” rather than “DD” since we are not yet in a step where Desert Dust is identified.”

Done (line 300).

“2.5 Methodology, p.16258, l.2: This step . . .“aims” at.”

Done (line 301).

“2.5 Methodology, p.16258, l.7: add “among all intense aerosol episodes selected in the previous step” at end of sentence.”

Done (lines 310-311).

“2.5 Methodology, p.16258, l.10: I suggest using “different” instead of “modified”.”

Done (line 314).

“2.5 Methodology, p.16258, l.16: I suggest using “parameter thresholds” instead of “parameters”.”

The relevant text refers just to the parameters and not to their thresholds, therefore we kept it unchanged.

“2.5 Methodology, p.16258, l.17: I suggest adding “for dust episodes identification” at end of sentence.”

Done (line 325).

“section 2.5.2, p.16259, l.11: about the fine mode fraction you might specify “of the particle volume distribution”.”

We clarified this in the text (lines 347-348). It should be noted that MODIS FF refers to the aerosol optical depth and not to volume size distribution.

“section 2.5.2, p.16259, l.12: omit “Thus”.”

Done (line 349).

“section 2.5.2, p.16259, l.13: “threshold” seems more appropriate than “critical”.”

Done (line 349).

“section 2.5.2, p.16259, l.17: Jones et al., 2007 is not in the ref. list. Isn’t it Jones and Christopher, 2007?”

Corrected (lines 353-354).

“section 2.5.3, p.16259, l.26: Middleton et al., 2001 is not in the reference list; isn’t it Middleton and Goudie, 2001 ?”

Corrected (line 363).

“section 3, p.16261, l.15: is it really “episodes” or “days” which is meant here?”

Episodes at a pixel level scale.

“section 3.1, p.16262, l.4-6: clarify the sentence.”

The sentence has been split in two and now reads (lines 423-427): “In Fig. 3-ii are depicted the scatterplot comparisons between ground PM and satellite AOD measurements for the selected 333 DD episodes. The comparison, apart on an annual basis, is also obtained on a seasonal basis (different colored points).”.

“section 3.1, p.16262, l.11: omit “Thus,”.”

Done (line 429).

“section 3.1, p.16262, l.11: ...on “a” seasonal basis.”

Corrected (line 432).

“Section 3.1., p.16262, l.22-23: more historical references have shown this such as Moulin et al., JGR, 1998, and Antoine and Nobileau, JGR, 2006.”

The specific references were added (line 444).

“Section 3.1., p.16262, l.28-29: rephrase sentence ; on one hand, it is not clear to me to what "their nature" refers to; on the other hand since AERONET are surface-based observations, it must probably be specified here "in-situ" when referring to PM measurements.”

We agree with the Reviewer and we modified the relevant text (lines 451-452) which now refers to in-situ PM measurements, while we removed “by their nature”.

“Section 3.1., p.16263, l.1: historical references include Hamonou et al., JGR, 1999, and Berthier et al., JGR, 2006.”

Done (lines 453-454).

“Section 3.1., p.16263, l.6: "lifted" is suggested instead of "removed"; removal includes deposition.”

Done (line 459).

“Section 3.1., p.16263, l.13: “CALIOP data... “show that” rather than “shows that”.”

Done (line 466).

“Section 3.1., p.16263, l.25: “not” instead of “no” representative.”

Done (line 478).

“Section 3.2, p.16264, l.7: what is the probability level of the given “statistically significant” correlation?”

The result is given at 95% confidence level. This is now clarified in the text (line 488).

“Section 4.1.2., p.16266, l.10 and 17: is it by means of average or maximum AOD?”

The text (lines 553-554) now reads: “in terms of AOD550nm ...“.

“p.16267, line 1-2: end sentence unclear (from “and also”), please rephrase.”

The relevant text has been modified (lines 573-574).

“p.16268, l.5: about variability, I suggest using “Seasonal” rather than “Intra-annual”.”

“Intra-annual” was replaced by “Seasonal” (line 605).

“Section 4.2.1 and fig. 6: I suggest using "episodes/season" rather than “episodes/year” as a unit.”

Done.

“p.16269, l.8: add a dot “.” at end of sentence.”

Done (line 634).

“p.16270, l.7: do you mean “average” AOD550nm?”

Yes.

“p.16270, l.9-23: this is relevant to section 4.2.1 on seasonal variability.”

We believe that although it is true that this discussion resembles the one made in the section of seasonal variability, it is better to remain here since it is not exactly the same as referring to the year to year variability.

“Section 5, p.16274, l.4: “aims at describing”... “regime of intense desert dust episodes”.”

The relevant text has been modified (lines 778-779).

“Section 5, p.16274, l.5: “takes” place.”

Done (line 779).

“Section 5, p.16274, l.8: add “at daily and 1° resolution” at end of sentence.”

Done (line 782).

“Section 5, p.16274, l.10: specify “(reff, over ocean only)”.”

Done (line 785).

“Section 5, p.16274, l.23-24: are those seasonal correlations given for the whole Mediterranean basin?”

The correlation coefficients refer to the comparison shown in Fig. 3-ii, which is made for the Meditteranean stations.

“Section 5, p.16275, l.14: use “north” rather than “northern”.”

Done (line 818).

“Section 5, p.16276, l.1-3: provide the ranges of values found.”

Done (lines 832-834).

“Ref. list: missing Borbely-Kiss et al., 2004 (cited in p.16265), Neff et al., 2008 (cited

in p.16250). Adler et al. (2003) and Gerasopoulos et al. (2011) are in the reference list but do not seem cited in the text.”

Please note that these papers have been removed from the present version of the manuscript.

“Table 1: I suggest for the legend “Seasonal distribution of relative occurrences of strong and extreme”; a line with total (absolute) numbers at the bottom of "Land" and "Sea" sub-tables could be helpful.”

The caption of Table 1 was modified. Also, a line was added at the bottom, providing the total annual number of DD episodes, as suggested by the Reviewer.

“Fig.1 is unreadable.”

The figure has been improved.

“Fig.2: in the first test level, it should be specified that the first step detects Strong or Extreme “aerosol” episodes.”

We believe that this is explained in the manuscript (2.5 Methodology).

“Fig.3i is too small. The smallest blue points are hardly distinguishable from lakes and small black dots in the map. Suggestion: it would be nice to have a smoother indication of the number of events based on a full RGB color scale rather than only using two colors.”

We prefer to keep the figure as it is because we believe that the size of circles is adequate and that the two different colours (blue and red) used help the reader to distinguish in which stations the number of the common pair values (AOD-PM) is higher or smaller than 10. In addition, we believe that the problem of readability was induced by the poor quality of production of the figure, which is going to be improved in the revised manuscript.

“Fig. 6: scales are not readable”

The Figure has been reproduced and hopefully the scales can be read more easily.

“Fig. 7: Is this the overall average AOD?”

It is the averaged AOD value over the entire number of strong and extreme DD episodes, i.e. over the whole study period (2000-2007).

“Fig.9 would probably be clearer if plotted in terms of anomalies.”

The corresponding figure with the anomalies is now provided in the Supplement file (Figure S4).

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