Review of manuscript acp-2013-174 by A. Gkikas et al. entitled "The regime of desert dust episodes in the Mediterranean based on contemporary satellite observations and ground measurements"

Detailed comments in complement of review summary by francois.dulac@cea.fr

Major comments:

1. 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).

2. 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.

3. 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.

4a. The areas covered should be made clearer on a map (land vs sea pixels and limits between the 3 considered basins).

4.b. 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 extreme 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). 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.

5. 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.

6a. 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.

6b. 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.

7. 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.

8. 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.

9. 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.

10. 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.

11. 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.

12. 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.

13. 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?

14. 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).

Other comments:

-1. 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.

-2. 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.

-3. Introduction, p.16252, l.2: Antoine and Nobileau, JGR, 2006 is missing in the citation list.

-4. 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).

-5. 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.

-6. 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.

-7. Section 2.2, p.16256: give precision on the re-gridding method.

-8. Section 2.2, p.16256: any information and reference on uncertainties in the aerosol index.

-9. Section 2.4, p.16257 end of section: can you explicit the AERONET uncertainty in R_{eff}?

-10. 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.

-11. 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…).

-12. The Methodology sections omits providing a definition of the season limits used.

-13. Section 3.1., p.16263, l.16-18: couldn't we also have events completely missing in satellite data due to the cloud coverage ?

-14. Section 3.1., p.16263, l.23-24: could you list the given stations?

-15. 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).

-16. 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?

17. Section 4.2.1 on seasonal variability: isn't there any bias due to very variable cloud coverage with the season?

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

19. 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.

20. 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.

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

22. 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.

23. 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.

Technical corrections:

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

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

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

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

-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.

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

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

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

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

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

-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.

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

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

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

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

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

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

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

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

-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?

-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 ?

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

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

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

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

-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.

-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 measurments.

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

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

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

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

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

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

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

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

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

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

-p.16270, l.7: do you mean “average” AOD_{550nm}?

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

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

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

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

-Section 5, p.16274, l.10: specify “(r_{eff}, over ocean only)”.

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

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

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

-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.

-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.

-Fig.1 is unreadable.

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

-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.

-Fig. 6: scales are not readable

-Fig. 7: Is this the overall average AOD?

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