

## ***Interactive comment on “African dust outbreaks over the western Mediterranean basin: 11 year characterization of atmospheric circulation patterns and dust source areas” by P. Salvador et al.***

### **Anonymous Referee #1**

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The authors present the results of an analysis of PM<sub>10</sub> data in Spain to understand the origin of dust and its mode of transport from Africa. They define an impact index to evaluate the incidence of dust outbreaks in Spain for each mode of transport. They conclude that dust in Spain originates from the Northwest region of West Africa and is transported essentially by four modes of circulation, with variable contribution depending on the season. The impact index is higher in spring.

These results would be interesting if they were original, but most of them have been

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previously published by the same authors. In the previous manuscript (Alonso-Perez et al., Objective identification of synoptic meteorological patterns favouring African dust intrusions into the marine boundary layer of the subtropical eastern north Atlantic region, *Meteorol. Atmos. Phys.*, 113, 109-124, 2011) they did similar analysis of PM<sub>10</sub> data in Tenerife using 2 statistical methods, and showed similarly four types of circulation patterns. They also discuss the effects of NAO on dust inter-annual variability. As in the present manuscript, they used back-trajectory analysis to identify dust sources. It is surprising that with all these similarities this previous work was not cited. Instead, I would suggest building up on the previous work and focusing on the original parts. The apparent differences are 1) the location of the sites, 2) the length of the data record, and 3) the selection of the K-mean statistical method. It would be valuable to know if the present results differ from Alonso-Perez et al. (2011). Also, the discussion should also highlight similarities and/or differences with Pey et al. (African dust outbreaks . . . , *Atm. Chem. Phys.*, 13, 1395-1410, 2013). Finally, there are many occurrences where clarity can be improved, particularly in the descriptions sections. These sections are excruciating to read and placing the details in annex would help smooth the text. I would not recommend publication of the manuscript in its present form.

Reading the manuscript, it is unclear why the K-mean method was used instead of Empirical Orthogonal Function (EOF), which guarantees that each mode is orthogonal to the others. From Figure 2, the circulation patterns of mode 2 and 3 appear very similar. From Alonso-Perez et al. (2011) work, a justification to select K-mean method should be explained, as it does not seem the right choice.

More detailed comments:

Abstract: The description of the methodology is too long compare to presenting key results: 18 lines for a total of 29 lines. This is particularly true considering that most of the methodology was described previously. Abstract: The source identification is very crude. What you are saying is that dust from most of the northern part of West Africa is affecting Spain. You may want to say that your analysis does not allow differentiating

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between source areas. Abstract: Is there not a physical way to characterize the circulation patterns? If you had used EOF analysis, the first mode is related to NAO, and this has been discussed in numerous papers. The second mode is meridional, etc. But with your K-mean technique, such clear circulation patterns are not obvious. Abstract: Last line is confusing: "...from eastern to western areas of the western Mediterranean basin." Instead, you should finish with a key finding.

Introduction: Line 14-19 can be removed as too general. On the other hand, why should we be interested in dust over Spain? Is it a local problem of air quality that you are addressing? Introduction: It is important to refer to previous work but you should definitely build up a story from your previous work. We should know why you choose K-mean method rather than EOF analysis. Page 5, Line 11 to 14: reformulate more clearly

Circulation classification methodology: Why are you choosing K-mean and not EOF? The terms in Formula 1 and 2 are not properly defined. The double summation in Formula 2 is unclear. Also, what terms depend on time? Page 6 Line 14 : ", if that reduces" What is "that"? Page 7, Line 5 "daily fields" you are not using anomalies? How do you normalize the quantities X of Formula 1? Page 7, Line 8: "cycle 31r2" what is that?

Identification of potential source areas of dust: so excruciating to read that it is difficult to see if this makes sense. Considering at the end that you are unable to identify sources beyond the entire northern Africa, I don't see the point of all this complexity and even doing this back-trajectory analysis. Estimation of the Impact Index: Formula 4 is unclear. Who can understand "ADLi is the average value of ADL registered at this site during episodic days grouped into synoptic situation i, . . ."? Is this "registered" ADL not simply PM10? Page 10 Line 1: "...increased gradient from the N (21% at O Saviono and Niembro) to the S (65% at Viznar)"???

Conclusions: It is difficult to follow with the heavy use of acronyms such as CT. Page

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18, Line 6-10: Some clear readable sentences, that may be copied in the Abstract.

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