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Interactive comment on “A climatology of dust emission events from northern Africa using long-term surface observations” by S. M. Cowie et al.

Anonymous Referee #3

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This paper is valuable because it proposes a comprehensive analysis of existing data on dust events over a 29 yr time period by using the systematic observations performed in the meteorological stations. This clearly provides more temporal depth to our knowledge of the occurrence of dust events. For these reasons, this paper should be published. However, significant improvements are necessary before publication. Especially, because the SYNOP data are the key point of this paper a better description of the data set in terms of interannual, seasonal and daytime representativeness is really necessary. On various points of the paper, a more rigorous statistical approach should be used; Lastly, in many parts of the paper, the authors use words like “sug-

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gest” (11 times!), “could”, “we hypothesize” etc. . . which limit the interest of the paper while in some cases, additional examinations of the SYNOP data would have helped to conclude in a clearer way.

Introduction: the bibliography is too much oriented on modeling works especially on the part concerning dust emissions. This does not reflect completely the state on our knowledge on the occurrence of the dust events over North Africa over the last 50-yr period. For example, there is an insufficient use of previous works dealing with the temporal variability of dust events at different scales. Even if some of the publications which have addressed these items are quite old, they contain interesting points that could be discussed in relation with the scope of the present paper (among others: N’Tchayi, G. M., Bertrand, J., Legrand, M., and Baudet, J.: Temporal and spatial variations of the atmospheric dust loading through West Africa over the last thirty years, *Ann. Geophys.*, 12, 265–273, doi:10.1007/s00585-994-0265-3, 1994; N’Tchayi et al., The Diurnal and Seasonal Cycles of Wind-Borne Dust over Africa North of the Equator *J. Applied Meteorol.*, 36, 868-882, 1997; Moulin and Chiapello, Impact of human-induced desertification on the intensification of Sahel, *GRL*, 33, L18808, doi:10.1029/2006GL025923, 2006; Marticorena B. et al.: Temporal variability of mineral dust concentrations over West Africa *Atmos. Chem. Phys.*, 10, 8899–8915, 2010. etc. . . .)

P7427 line 17: The original paper by Bagnold is 1941. Even if the authors have used a new reissue from 2005, it is preferable to use the original reference of this work (Bagnold, R. A. (1941), *The Physics of Blown Sand and Desert Dunes*, 265 pp., Methuen, London).

Page 7429 line 29: The authors mentioned that they use observations which are reported at 3 and 6 h intervals. The fact that all the data have not the same recording time is not discussed while one can imagine that, if dust events are more frequent at a certain period of time during the day (for example in the middle of the afternoon) the absence (or not) of observations at this time (for example 15h) could affect the result. It could be interesting for some stations for which the sampling time is 3h to evaluate if the

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frequency of occurrence is changed when only a value each 6h is considered.

P 7431 lines 14 to 25. The main problem with these data is obviously the representativeness of their sampling and the fact that they can be compared one to another without bias. This implies that the sampling is sufficiently homogeneous in terms of year (because it could exist interannual variability that could induce bias if data are not sufficient for years with a low (high) number of dust events), in terms of season (because there is a seasonal cycle of dust emission in North Africa) or in terms of day (because there is a diurnal cycle of dust events). The information provided by the authors does not allow to have a clear idea on how these potential bias are accounted for. This should be explained with more details because these data are the key point of this paper. For example, it is mentioned that stations with less than 500 observations (of dust emission, I assume, is it correct?) per year for 5 years or less are marked A and those with less than 500 observations for more than 5 years are marked B. But it seems at least as much as interesting to know how much synop observations (not only dust emission) have been reported during the year and when these observations have been recorded? Only in winter? Only during day time? All along the year? A better description of the available data is absolutely necessary to have a clear idea of what it can be concluded or not from these data.

P7432: The choice of NDVI for tracing possible temporal changes in emission threshold can be really discussed. Beyond the comment addressed by N. Mahowald on the accuracy of the NDVI for low LAI as observed in semi-arid regions, and since dust emission is the main objective of the paper, information on rainfall at the station can also be really valuable (even if it is no a direct indicator of the soil moisture). In most of the case, Rainfall immediately stops wind erosion, even if high wind speed occurs and a knowledge of the rainfall at the station could limit the spread when linking wind speed and dust emission. Moreover, recent study by Pierre et al. (Impact of vegetation and soil moisture seasonal dynamics on dust emissions over the Sahel, J. Geophys. Res., 117, D06114, doi:10.1029/2011JD016950.) clearly shows that soil moisture is probably

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the main driver (more than vegetation) of the seasonal change in erosion threshold in the Sahel.

P7434 line 2 : suppressed “all being close together” (considering the range of wind speed for erosion threshold, 7.5 and 9.25 m s⁻¹ are not so close).

P7436 line 11: it is unclear what is called “the semi-arid transition zone between the Sahel and the Sahara”. In fact, it is not completely what you are calling Sahel, Northern Sahel, etc. . . Please precise as much as possible the corresponding latitudes.

P7436 lines 17 and following: the explanation provides for the high dust emission frequency at least for the Tunisian station of Remada and the Algerian station of El Borma is not satisfying and probably wrong. The stations are far from the Chotts region and I do not understand what the authors want to suggest when indicating “this may have to do with their proximity to the Mediterranean Sea”.

P7437 lines 1-2: maybe it is only a problem of wording but the sentence “Overall this analysis demonstrates that emission events contribute significantly to all reported dust events in this region” is just an evidence.

P7437 lines 29-30: Why did not the authors test their assumption that the large difference in T50 and T75 in the Sahel is mainly due to an increase of the threshold in summer? More generally, the text contains a lot of “suggest” (11) or “could” while some additional treatments would have probably allowed to express a more clear conclusion.

P7460 Figure 4: It will be probably more clear for the reader to have two maps (instead of one), one for FDE, one for the ratio emission-transport with only black squares of different radius on each.

Figure 7 is too small and most of the graphs cannot be read (especially the NDVI line).

3.1.3 and 3.2.1: the method used to cluster of stations is not objective (“this was done subjectively by looking for similar characteristics. . .”) while there are many statistical methods allowing to perform objective clustering. In the same way, the significance of

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the seasonality or its link with NDVI is not established on real statistical basis. In fact the authors assume a priori the reasons for which the stations should be grouped as they did: they used mainly considerations on meteorological pattern (circulation, precipitation, etc..) and then they can only concluded on the agreement with the starting hypothesis. Why did not the authors use such powerful statistical approaches allowing classification without a priori?

P7441 line 20: “We hypothesise that this is due to infrequent early season precipitation events. . .”. This hypothesis can be verified with the SYNOP data.

3.2.3 Seasonal cycle in dust emission. There is almost no references in this part while many works have attempted to discuss the seasonal variability of dust emissions in the Sahara and the Sahel. How are these results in agreement with previous studies that have used observations from satellites, photometers or local stations? This should discuss.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 7425, 2014.

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