Atmos. Chem. Phys. Discuss., 14, C3737–C3741, 2014
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Atmospheric Chemistry and Physics Discussions

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

S. M. Cowie et al.

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Received and published: 17 June 2014

The Manuscript entitled "A climatology of dust emission events from northern Africa using long-term surface observations" provides a very good conceptual and a well written study of dust climatology patterns using SYNOP data. I believe that it deserves to be published at the Atmospheric Chemistry and Physics Journal after some minor revisions:

1) Introduction Page 3 Line 4: The authors should expand a little more on the radiative effects of desert dust particles and add a couple more updated refer-

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ences.

Thank you very much for taking the time to read and review this paper. Your comments have been extremely helpful.

We have updated this sentence and added in information from the 2013 IPCC report.

"Once airborne, dust particles from northern Africa can be transported for thousands of kilometres and alter climate by scattering, reflecting and absorbing incoming shortwave and outgoing long-wave radiation at both the surface, and the top of the troposphere (Sokolik et al., 2001). Cooling takes place when absorbing and scattering reduces the amount of energy which reaches the surface (Kaufman et al., 2002; Spyrou et al., 2013) while atmospheric warming takes place when aerosols absorb and re-emit outgoing long-wave radiation (Dufresne et al., 2002). Current radiative forcing estimates of mineral dust are highly uncertain, but an overall negative (atmospheric cooling) effect of 0.1($\pm$0.2) W m2 is predicted (Stocker et al., 2013)"

2) Introduction Page 4 Line 16: ". . ..produces gusty winds and dust emission. . ." should be changed to something like ". . ..produces gusty winds which in turn initiate dust emission. . ." just for clarity.

Section now reads: "The morning breakdown of a NLLJ by surface heating produces gusty winds which in turn initiate dust emission. The heating of the surface in the morning expands the boundary layer to the point where it erodes the jet and transports momentum to the surface"

3) Section 2.1.1. Page 7 Lines 10-11: What is meant exactly by time resolution? Please rephrase.

We have reworded this in the following way: "... reported at a given time of day or during a certain month, divided by the corresponding total number of observations.

4) Section 2.1.1. Page 7 Line 22: What is the importance of the ratio of day to night observations? Do you assume that during the night the observation of

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dust is hindered, therefore biased? Please explain in the text.

We have added the sentence: “More daytime observations will give extra weight to daytime dust emitting mechanisms. This is a problem for Sahel stations where haboobs commonly occur and are known to uplift dust in the evening and night-time (Marsham et al., 2013)”

5) Section 2.2.1. Page 9 Lines 5-13: The main mechanism for the production of small particles that can travel great distances is the saltation bombardment. When the authors define the threshold of wind speed is it for this process? Please expand.

Our measurements of dust emission are based on a subjective observation made by a station observer. The descriptions of dust emission given by the WMO do not specify the exact mechanism, as this could not be accurately measured considering the expertise and equipment available at WMO SYNOP stations.

We have added the following information to section 2.2.1, P7, Line 1. “. . .the ww codes describing dust emission are 07–09, 30–35 and 98 (WMO, 1995). These descriptions of dust emission weather do not provide information on the physical processes at a particle scale, but simply describe how the dust appears in the atmosphere to the observer.”

6) Section 2.2.1. Page 11 Line 3: End of paragraph. Is there a reference indicating the “. . .fact that emission occurs over a range of wind-speed values.”? Just to strengthen the point made.

We have added the Helgren and Prospero 1987 paper as a reference. They found a range of 6.5 – 13 ms-1 for western Sahara stations using a similar method to ours.

Helgren, D. M., and J. M. Prospero. "Wind velocities associated with dust deflation events in the western Sahara." Journal of climate and applied meteorology 26.9 (1987): 1147-1151.

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7) Section 3.1.1. Page 12 Line 12: Can you please add a reference also indicating that the ". . ..dust emission is generally more frequent in the semi-arid transition zone between the Sahel and Sahara. . ."?

The following sentence is now included "This is in agreement with the identification of a "Sahel dust zone" by Klose et al. (2010), though their study included transported events."

Klose, Martina, et al. "Sahel dust zone and synoptic background." Geophysical Research Letters 37.9 (2010).

8) Section 3.1.1. Page 12 Line 19: ". . .proximity to the Mediterranean Sea. . ." Please expand a bit how the Mediterranean has the described effect.

Having looked at the seasonal direction of winds, these stations only show a predominantly north-east direction in summer and this is when dust emission frequency is lowest. We therefore decided to omit this sentence.

9) Section 3.1.2. Page 13 Line 27: Please expand on the physical reasons why Dongola and Abu Hamed in Sudan are prominent outliers.

It is not clear why Dongola and Abu Hamed have much higher annual FDE, but it could be due to local environmental factors such as an easily erodible local source or local orographic circulations, which allow winds to exceed the threshold regularly. A comment on this has been added to the text.

10) Section 3.2.3. Page 18 Line 23: ". . .emission thresholds found in the literature. . ." Can you provide some references?

We have included Helgren and Prospero 1978 and Chomette et al., 1999.

11) Section 4. Page 24 Line17: ". . ..Seasonal variations in thresholds are largest in the Sahel and smallest in Sudan." Can you explain why this happens (a small sentence)?

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This sentence has been included: “In arid Sudan (this work focuses on the northern stations of Sudan) there is significantly less rainfall than in the Sahel. Hence, there will be less seasonal variation in soil characteristics leading to less variable thresholds in Sudan.”

General Remark: I would like to see if this method can be applied to other desert regions of the world (perhaps the Gobbi desert). Can the authors add a paragraph addressing this? I was impressed by the method used and it would be interesting to see the possibilities of applying it to other regions as well.

This method has already been used in the Gobi Desert by Kurosaki and Mikami, 2007 and is currently referenced in 2.2.1.

Kurosaki, Y., and M. Mikami. "Threshold wind speed for dust emission in east Asia and its seasonal variations." Journal of Geophysical Research: Atmospheres (1984–2012) 112.D17 (2007).

Figures:

Figure 5: Can the authors create the plot with thicker lines (red, green, blue) so to be more clear?

Done

Figure 7: If the orography is not essential it would be better to omit it from the plot for clarity.

Done

Figure 12: Please write a complete caption like the one on Figure 11.

Done

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This is a nice paper with a good approach that should be published. I had just two comments on reading through it: "The normalized difference vegetation index (NDVI; Tucker et al., 2005) is a popular proxy for vegetation, especially in semiarid areas such as the Sahel (Huber and Fensholt, 2011; Olsson et al., 2005) to which it is particularly well suited." None of these papers argues that the NDVI is particularly well suited to low LAIs: in fact if you look closely at Zhu et al., 2013, it is clear that there is a lot of spread in the LAIs estimated from NDVI and from the data, and that there is a lot of bias in low LAIS (e.g. about 0.1 LAI over-

prediction, I think). I think it would be more fair to say that we use it because there is nothing else, but it does not do a good job in arid places. NDVI can only get greenness, so brown vegetation, which holds down soils isn't well captured.

Thank you very much for taking the time to read the paper, and replying with useful comments.

We took the reference from this particular sentence of section 2. of Olsson et al. 2005 "These data are particularly well suited to the study of semi-arid regions because: (1) NDVI becomes saturated at LAI levels higher than 3–5 (Prince, 1991a, b; Field et al., 1995), and (2) atmospheric contamination (particularly clouds) of the data is less of a problem in semi-arid regions than in more humid ones (Chappell et al., 2001)"

We didn't consider that LAI's were low in the Sahel as we did not calculate or use them and were not aware of the Zhu paper at the time of writing but this is good information to consider.

We have included the following sentence in Section 2.1.2 "A limitation of this dataset is that NDVI only detects green vegetation and may overestimate areas with low Leaf Area Indexes (Zhu et al., 2013), such as semi-arid regions. After the main growing season brown vegetation, which still affects dust emission (Zender and Kwon, 2005), is likely to be left over, but not detected by NDVI."

Typo: Koch, J. and Renno, N. O.: should be Kok, J.

We shall keep it as Koch as this is the paper we wish to reference the paper: Koch, Jacquelin, and Nilton O. Renno. "The role of convective plumes and vortices on the global aerosol budget." Geophysical Research Letters 32.18 (2005).

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

Atmos. Chem. Phys. Discuss., 14, C3744–C3754, 2014
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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 "suggest" (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.

1) 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 content 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. . ..)

Firstly, thank you for taking the time to provide these really useful comments.

These papers are indeed relevant and useful to this work, though perhaps the Moulin and Chiapello 2006 one less so as it is more concerned with trends of dust presence over the Atlantic from satellite. We have been unable to gain access the first paper on the list, but from reading the available abstract it would appear to use a similar methodology as the 1997 paper which focuses on the diurnal and seasonal aspects of

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dust presence. A couple more sentences have been added into the introduction:

"The seasonal and diurnal cycles of dust are explored in relation to visibility and rainfall in Mbourou et al., 1997 while Marticorena et al., 2010 discusses dust concentration and strong wind frequency. The time and spatial scales of these two studies are quite different with 53 stations and three four-year periods investigated in Mbourou et al., 1997, compared to three stations for a three year period in Marticorena et al., 2010. This paper intends to build on the information gathered by these studies by including a larger spatial area, a longer data record and new data analysis techniques which aim to clearly separate emission from transport events."

2) 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).

This will be amended.

3) 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 interesting for some stations for which the sampling time is 3h to evaluate if the frequency of occurrence is changed when only a value each 6h is considered.

We have added the following information to section 2.1.1

"In the Sahel region 6-hourly sampling is more common than 3-hourly sampling. Different sampling regimes tend to be confined to within countries. For example in Mauritania nearly all stations have no night-time observations, while in Mali and Niger sampling

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tends to be limited to 6-hourly. This is documented in detail in the supplementary material P1–18."

"Analysis of six hourly data was applied to stations which have stable three hourly sampling. The magnitude of annual FDE varied at a few stations, but overall the seasonal cycle remained similar at all but one station (supp material pp19 - 28). We suspect that this is due to natural inter-station variability in the diurnal distribution of dust emission mechanisms between SYNOP and inter-SYNOP hours"

"The approach we take in the paper is to highlight the main biases which might affect conclusions that apply to large areas and groups of stations, rather than focusing on the effect of individual station biases on their individual results. "

FDE is quite different if you cut down the stations with regular 3-hourly sampling to 6-hourly sampling. We have done this for the following stations which have a healthy record: Biskra (P19, supp. material), Touggourt (P19), ElOued (P20), Ghardaia (P20), Bechar (P21), HassiMessaoud (P21), TripoliAirport (P23), Sirte (P24), Cairo (P26), ElArish (P25), PortSaid (P25), ElGolea (P22), In Amenas (P22), In Salah (P23), Mersa Matruh (P24), Luxor (P26), Asswan (P27) and Kharga (P27).

While there is some variation in the magnitude of FDE (green and red combined), overall the seasonal cycle is similar with a peak in spring still occurring at most stations. At the stations where the annual FDE clearly increases (e.g. PortSaid, ElArish) or decreases (Asswan) when 6-hourly sampling is used it is probable that these stations have a dominant dust emitting mechanism, perhaps caused by a local wind regime, which mainly occurs at a particular SYNOP hour and is sensitive to the sampling time. At Kharga the main FDE season switches from spring to summer, though it is not clear why. It is perhaps not possible to explain a general effect of reducing 3-hourly sampling to 6-hourly sampling because – as shown in supp material, P19–28 – the impact of this will be variable on a station-to-station basis.

4) P 7431 lines 14 to 25. The main problem with these data is obviously the

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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 wintr? 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.

The 500-observation criterion applies to all observations, not just those with dust emission. So the criterion does, to some extent, consider the point of how many SYNOP observations altogether there are. The number of dust emission reports is considerably less (see supp material P 29–99 for each of the 70 stations). These plots do not provide any evidence that variability in the number of observations in each year, or each season, have a considerable affect on the interannual variability of dust emissions.

A detailed description of the bias at each station is perhaps not possible within the scope of the paper, as there is a great deal of local variation. We have investigated all of these aspects of the record however, and highlighted in the paper the biases we think could impact the results – a daytime bias and a much shorter record. A more detailed description of the data in respect to seasonal reporting and time series (P 29–

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99), SYNOP hour (P 1–18) are now included in the supplementary material and the reader will be referred to these in the text.

The biases can only be very generally accounted for, as the variability in data sampling is huge between stations. We cannot really comment on the seasonality of dust sampling as it is impossible to know if dust is absent simply because surface winds and meteorological conditions were not right. We have looked at the seasonal time series of all observations and these are presented in P29–99 of supplementary materials. For example, where there is a significant lack of night-time data, such as in the Sahel, it is discussed in the paper how this could affect dust emission statistics as night-time dust emitting phenomena such as haboobs may be under-represented at biased stations. The variations in data sampling are so large (see supplementary material P1–18, 29–99) between the different stations that it is impossible to discuss and document the effect each one would have on the analysis.

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

Thank you for the reading suggestion. After carefully comparing Figs. 6 and 7 in the Pierre et al. paper, we can see that in the parts of the Sahel where dust emission is

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not largely inhibited by vegetation, soil moisture is the dominating factor.

We had a closer look at the precipitation information contained in the MIDAS SYNOP dataset. Unfortunately, the majority of values are blank and there is no documentation to advise whether this is because there was no rainfall or there was no reading. If we ignore the blank values and only use those which have reported a number, between 1 – 5 % of rainfall values are greater than 0.00 (up to 17% at Elbayadh situated in the lee of the Atlas mountains). We found that out of 18387 observations of high winds with no emission from the 70 stations, 434 events (2.3%) experienced a documented rainfall event in the previous 24 hours. This provides evidence that some high wind events do not produce dust due to high soil moisture. However, the incompleteness of rainfall records is a major problem, so this effect cannot be quantified this way.

A respective comment has been added to the text:

"Out of 18387 observations of high winds with no emission from the 70 stations, 434 events (2.3%) experienced a documented rainfall event in the previous 24 hours. This provides evidence that some high wind events do not produce dust due to high soil moisture. However, as the majority of rainfall values are blank in the SYNOP records, and we cannot ascertain if this is because there was no rainfall or no reading, we are cautious of using SYNOP precipitation to investigate this any further."

6) 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).

This sentence has been removed.

7) 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.

This sentence was referring to the earlier statement in the paragraph "All eleven sta-

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tions with FDE > 10% are located between 15 and 23 N (large circles, Fig. 4). ". For clarity we have included the latitudes again and added a reference to Klose et al., 2010:

"This pattern indicates that, although dust sources are found across the entire region, dust emission is generally more frequent in the semi-arid transition zone between the Sahel and Sahara (15–23°N). This is in agreement with the identification of a "Sahel dust zone" (Klose et al., 2010), though their study included transported events."

8) 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".

We decided to take this sentence out. See reply to reviewer 1.

9) 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.

This sentence has been reworded to the following to highlight the comparison between emission events and transported events.

"Overall, this section demonstrates that emission events contribute significantly to the frequency of all reported dust events, which includes transported dust, in this region"

10) 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.

The seasonal variability in thresholds is discussed for grouped regions in 3.2.2. In

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this section we discuss the role of precipitation events in the early growing season. Thresholds are highest when vegetation growth is at a maximum in autumn (P7438, Line 18. See point 14 where we insert a reference to Fensholt et al., 2012 here too).

11) 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).

We hope that the addition of the individual plots shown in Fig. 7 of the main paper, in the supplementary materials P100 -117, which the reader will be referred to, will be satisfactory.

12) 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 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?

Due to the multidimensional nature of the problem we are investigating, our concern was that the time invested in working out how to use the statistical methods (we did consider a PCA analysis) would not be well spent, as the design of the metric would require many subjective decisions too. In some regions we wished to consider the quality flags we applied earlier, and information from the literature. Not all of the information which went into the decision making process is included in section 3.2.1., such as descriptive knowledge gained from literature, but what is there is deemed enough

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to justify the choice of grouped stations.

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

As discussed in point 5, the precipitation fields are not complete enough for a reliable analysis.

14) 3.2.3 Seasonal cycle in dust emission. There is almost no references in this part while many works have attempted to discuss he 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.

We don't presently compare to many other studies as we are specifically looking at the frequency of emission, which is quite different to visibility and concentration, which has been studied in the past. We are also cautious about the difference in spatial and time resolution of other studies too. We do refer to Klose et al., 2010 when discussing the Sahel "dust zone".

Some changes have been made to the manuscript regarding this comment:

P7438, Line 13 "Faya, the only station in Chad, shows high values of both FDE and DSF during the winter half year (Washington and Todd, 2005).." – Washington, R., and M. C. Todd (2005), Atmospheric controls on mineral dust emission from the Bode′le′ Depression, Chad: The role of the low level jet, Geophys. Res. Lett., 32, L17701, doi:10.1029/2005GL023597

P7438, Line 18

"in Autumn, following the summer rainfall maximum (Fensholt et al., 2012)" - Fensholt, Rasmus, et al. "Greenness in semi-arid areas across the globe 1981–2007âĂŤAn Earth Observing Satellite based analysis of trends and drivers." Remote Sensing of Environment 121 (2012): 144-158.

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P7443, Line 20 remove sentence “FDE is high during winter and spring and dips during the summer and autumn, when the monsoon influence reduces emission by changing the soil type and wind regime (Fig. 8d and e)”

and insert

“A similar effect on dust concentration was observed by Marticorena et al., 2010 for two stations located within the C and W Sahel regions.” Marticorena, B., et al. "Temporal variability of mineral dust concentrations over West Africa: analyses of a pluriannual monitoring from the AMMA Sahelian Dust Transect." Atmospheric Chemistry and Physics 10.18 (2010): 8899-8915.

P7444, Line 10 insert sentence

“Extremely intense, local emissions were found to be important to dust concentrations in Marticorena et al., 2010 in the Sahel summer months, associated with the passage of MCSs. However, with their short durations at the observation stations of less than 1 hour, these might not be properly represented by the 3 hrly SYNOP data used here”

P7444, Line 14 insert sentence after “..DSF. ”

“..DSF. This summer maximum is also identified in TOMS AI measurements (Englestaedter and Washington, 2007) ” - Engelstaedter, S., and R. Washington (2007), Atmospheric controls on the annual cycle of North African dust, J. Geophys. Res., 112, D03103, doi:10.1029/2006JD007195

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