

# ***Interactive comment on “African mineral dust sources: a combined analysis based on 3D dust aerosols distributions, winds and surface parameters” by Sophie Vandebussche and Martine De Mazière***

## **Anonymous Referee #1**

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The paper by Vandebussche and De Maziere presents an analysis of desert dust sources over North Africa from satellite data and some ancillary filters. The particularity of this study is that it uses satellite retrievals of vertical profiles of dust derived from IASI thermal infrared spectra. Therefore, satellite data of dust load near the surface is estimated which is an advantage with respect to previous satellite-based studies of North African dust source using column-integrated amounts or lidar transects with very coarse daily coverage. Therefore, the approach is interesting and potentially provides new information. However, several major revisions are needed in order that the paper

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is publishable in the ACP scientific journal. These major revisions are first listed, then important modifications that are also needed and other minor points that should be modified.

I strongly recommend the following major revisions, so that the paper is publishable:

1) The title: The paper only analysis dust over North Africa and not the whole continent. It does not really use 3D dust distributions but surface retrievals (the authors mention this explicitly in page 5, lines 30-31). The use of winds and surface parameters is very limited in the paper to be explicitly mentioned in the title (see below for further comments on this). I strongly recommend to change the title of the paper as: “North African mineral dust sources: a combined analysis based on surface dust detections and ancillary data” or similar.

2) Quality of the figures: readability of most monthly figures is poor, with very small panels, noisy data and very difficult to distinguish the evolution for a relatively small region (e.g. Bodélé depression). Their quality should be revised and improved.

3) MAPIR data coverage: It is clear from Figure 6, that MAPIR retrievals are not available for each cloud free scene. For a given month, MAPIR data is rather limited, covering in most cases half of North Africa and rarely the northernmost part of Africa. This is of course not only linked to cloud cover, since IASI data is mostly available twice a day and clouds over the Sahara do not persist along a whole month. Moreover, MAPIR retrievals detect dust is in most cases (at least two thirds) and in a rather limited number of cases the retrieval detects dust-free scenes. So, to which extend, dust detection is linked to MAPIR data coverage? Therefore, it is important to clearly show and quantify the coverage of MAPIR data as a fraction of all possible measurements. Caution should be clearly point out for regions with limited data coverage (e.g. Northernmost part of Africa?).

4) Atmospheric dynamics over North Africa: The analysis of transport patterns in the current paper is too simplistic. One cannot simply draw out transport patterns around

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a large continent (over distances greater than 5000 km) by using a map of average monthly winds at a pressure level. No reference to previous work is done. The main dynamical actors of the region, largely known in literature, are not mentioned (African Easterly Jet, African Easterly Waves, Sub-tropical westerly Jet, Inter-tropical front, etc). This can only be addressed by dedicated studies using trajectories or transport/dispersion models and considering the 3D aspects of transport. Unless fully revised and properly addressed, I strongly recommend withdrawing Figures 16, 17 and 19 from the paper and the corresponding comments.

5) Surface wind and moisture filters: Monthly estimates of surface wind speeds and soil moisture are not directly linked to dust uplift, but their instantaneous values (in hourly scales). Surface winds can be very strong a few times a month (for example due to Mesoscale Convective Systems) and uplift large quantities of dust. However, this region may not pass the filter of a frequency higher than 10% of 5 m/s. Soil moisture is highly variable in time. It can evaporate very fast (in a few hours) in the first centimetres of soil during daytime over the desert and emit large quantities of dust. Only after a rain event, it clearly inhibits dust uplift but only a few hours later, it dries out and dust can be very easily removed. Only daily or sub-daily estimates of surface winds and soil moisture are useful for determining uplift potential. I really recommend revising the criteria used for these filters and use daily estimates of surface wind and soil moisture in coincidence with the actual satellite data, otherwise these filters do not have much physical sense.

Important revisions:

6) A description of other retrievals of dust using IASI measurements should be provided in the introduction of the paper.

7) Agreement or not between MAPIR and CALIOP detections of dust at the surface: Figures 3 and 5 show very large discrepancies between IASI and CALIOP. Mainly one region of coincidence is observed: The Sahel. These differences cannot only be

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explained by the time of the day of the measurements since once dust uplift occurs in a given region, most dust remains suspended nearby for more than 5 hours. Please, clarify the discrepancies in a more thoroughly analysis.

8) Dust in deposition process (e.g. line 15, page 16): Dry deposition of Aeolian dust always occurs when close to the surface. There is no sense in spotting a particular place as a region for “dust in deposition process”. One can tell that the region is not a source region, but dust is transported across.

9) Accumulation of dust after transport: the concept of accumulation of dust after transport is strange. An atmospheric constituent may accumulate at a given region if there is no wind after emission. However, a dust plumes is uplifted by winds and then it is transported and dispersed in the atmosphere. Air masses do not stop at a certain region after transport, but they are diluted horizontally and vertically by mixing and dust burden can reduce also due to continuous dry deposition and wet deposition when raining. Please clear out this aspect or use another term.

10) The northern part of Sahel is a place pointed out as a dust source by Middle and Goodie (2001) and Israelevich et al. (2002). Differences of current results with respect to their works should be clearly given.

11) Since it is a key and uncertain parameter: A sensitivity test of the MAPIR approach with respect to surface emissivity as a function of the location over North Africa should be given. Imprecise emissivity may cause geographical biases in the region selected as dust sources.

12) Figure 10 show numerous regions with soil moisture above 16% but not suppressed from the “all filters” picture in Figure 11. Please clarify.

13) Figure 13 only gives very approximate positions of mountains and regions. It is not a proper style for a scientific publication. It should be revised.

14) Please verify English language and many typing mistakes all across the

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manuscript.

Minor revisions:

15) Page 5, line 3: “each cloud-free IASI spectrum”. According to Figure 6, this is not the case since many regions are not covered by MAPIR data, which of course, are not covered by cloud all month long.

16) Land cover data (section 2.3.2 and Figure 8): it only points out desert areas. Very little selection is done. Please, clarify this.

17) Page 14, line 5: Citing a reference for NDVI and telling there is “a typing error in their text” seems strange to point out. A different reference should be used.

18) Writing style: Some expressions seem as oral language: “it is reassuring”, “ a huge peak”.. “some kind of loop” “area is tricky”. These terms should be revised.

19) Page 1, line 20: dust is located below 7 km because of their size only? It is because of their sources and mechanisms to mix it in the atmosphere. . . Ash is directly ejected at elevated layers and they are even coarser in size.

20) Page 1, line 24: absorption features? Better to use “absorption bands”.

21) Page 3, lines 1-3: not clear, which mechanism accounts for 1%? What happens with the 99% remaining?

22) Page 3, lines 9-11: Notion of dry and moist convective events should be given, as well as other mechanisms (extra-tropical cyclones/cold fronts, meteorological cold fronts).

23) Section 1.2: Page 4, line 9-10: Only the studies are mentioned and not the results. This should be more precise.

24) Page 6. Lines 20-21: statement “this is true only if those 2 parameters are independent. . .” is not clear. This should be better explained

25) Page 7, lines 23: the threshold for CALIOP AOD is the same for daytime and nighttime? Signal-to-noise ratios are very different in these two cases. For certain, CALIOP cannot measure AOD as low as 0.05 during the day. This threshold seems very low even during the day. What is the accuracy for CALIOP derived AOD using in other studies like Todd and Cavazos-Guerra (2016)?

26) Figures 1, 2 and 4: histograms are not very informative.

27) Page 17, line 4: This is unclear “dust trapped in the ITCZ” what does it mean? What is this mechanism?

28) Page 21, lines 14-20: The Bodélé depression hotspot is not clearly seen in Figure 18, nor the region east of Niger? The quality and size of the images do not allow to easily recognizing this spot.

29) Page 22, lines 14-20 & Page 23: lines 1 - 15: this soil data analysis is not clear. Conclusions are difficult to understand. The analysis should be supported by a figure showing the regions with the different soil types and confronted with dust maps. Why such dataset is not used as filter in section 2?

30) Page 23, line 16: The ITCZ is not expected to be convergence from the north, south and east at a given country. It is a large-scale structure, which changes in position every day, and it is closely linked to the Inter-Tropical front.

31) Page 23: lines 24-29: Transport and deposition of Saharan dust over the Sahel, which is afterward uplifted in a different season: This hypothesis is based on which scientific evidence? Are these speculations? If so, why they are mentioned?

32) Page 24: lines 13-16: “The conclusions of these two different analysis should probably be...” Here the authors of the manuscript justify a discrepancy with the conclusions from two other papers by telling that their dataset should have been interpreted in a different way. This is strange and awkward for a scientific paper. One can tell conclusions from a published dataset, but cannot change the conclusions from other

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scientists.

33) Page 25: line 9: “The Bodélé depression seems to be more active in the morning “ where specifically is this shown in Figures 20 and 21? The Bodele region (with marked limits) should be much better identify in the figures.

34) Maps are very noisy and it is difficult to know where red spots are in the same particular region from one to the other.

35) Page 25 line 14: “the situation might be different during winter” why this statement is not clear? The datasets shown in the paper do not show this?

36) Page 25: lines 14-16: Why conclusions on a source region west of Bodélé are linked to those from Bodélé itself? This is statement is not clear and should be better explained.

37) Page 26: line 4: The sentence is not clear. Re-write it please.

38) Page 26, lines 11-12: Consistency with LLJs during early spring is not clear. This should be explained in a much clear way.

39) Section 3.5: The title of this section is not clear. It should be named “Inter-annual evolution” or similar. This section draws conclusions from a dataset that is not shown. Evidence for these statements is not given. Therefore, either this section should be withdrawn or clear figures showing this inter-annual evolution should be presented.

40) Page 27, line 21: “unique” means that only the MAPIR approach derives dust 3D data? This is not the case. Please correct.

41) Page 27, line 27: “dust in deposition” is always occurring when close to the surface. There is not privileged place for this. Therefore, this cannot be specified as such.

42) Page 27, line 30-32: The analysis of monthly average winds is not sufficient for this statement of transport from central Sahara.

43) Page 28, lines 1-4: Large dust emissions over the Sahel occur often by very strong winds (sporadic and possible missed by the filter) associated with Mesoscale Convective Systems.

44) Page 28, line 16: “probability of local emission is high” how is this probability measured? It is quantified?

45) Page 28, line 18: “global”. This analysis is not global (worldwide). The term is not correct.

46) Page 28, line 35: “good Earth coverage” this is not fully correct since it is not the case for single years as the example of 2015.

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2017-809>, 2017.

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