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Atmospheric
Chemistry
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Discussions

Interactive comment on “Observations of mesoscale and boundary-layer circulations affecting dust uplift and transport in the Saharan boundary layer” *by* J. H. Marsham et al.

J. H. Marsham et al.

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1.2 Specific comments

1. I am suggesting to slightly modify the title, because “boundary-layer/mesoscale” can be confusing, it seems to mean that boundary-layer is equivalent to mesoscale. Maybe: “Observed impacts of boundary-layer mesoscale variability on Saharan dust”

The title needs to capture both key results from the paper, *i.e.* both the observed mesoscale effects of LST anomalies (scales $>$ 10 km) on the Saharan boundary layer and the modelled effects of boundary-layer convection on dust uplift.

We think a revised title of,
“Observations of mesoscale and boundary-layer scale circulations affecting dust transport and uplift over the Sahara”
captures this, whereas “Observed impacts of boundary-layer mesoscale variability on Saharan dust” does not really describe the effects of boundary-layer convection on dust uplift.

2. The different layers shown in Fig. 1 seem well mixed for potential temperature, but not so much for specific humidity.

This is commonly observed in boundary layers since there is a source of potential temperature at both the bottom and top of the CBL and a source of water vapour at the base of the CBL (weak in the Sahara) and a sink at the top (entrainment of dry air). Therefore, it is not surprising that we see this in a residual layer as well. They are close to “well-mixed’ in WVMR as well, however, so “each essentially internally well mixed” is a reasonable description.

3. Page 8819, lines 16-21: This paragraph deserves some more links between the statements made. The weak stratification can effectively affect the PBL growth. And especially land surface variations will make some areas more favourable to the occurrence of locally deeper PBL. The mesoscale circulation is another consequence of land surface variation. And both impact on dust vertical and horizontal transport, respectively.

This has been revised,
“This weak stratification of the SRL, and the weak lid between the convective boundary layer and the SRL, means that even small anomalies in the surface heating may significantly affect the growth of the CBL into the SRL. We expect land surface variations in

desert regions to affect surface fluxes and so induce mesoscale circulations (Segal and Arritt, 1992). In particular, low albedo regions are expected to increase surface fluxes. As a result low albedo regions are expected to increase the rate of growth of the CBL into the SRL, and so affect the vertical transport of dust between the CBL and SRL. In addition, mesoscale circulations generated by such land surface variations may also affect low-level winds and so the uplift of desert dust."

4. Page 8820, lines 1-5: Here and all along the manuscript, the authors should be cautious about their use of "small scale", "mesoscale" and "boundary-layer" scales. Fundamental mixing processes in the boundary-layer are turbulent, that is "small scale" and even smaller if "small scales" means 2 km in the present study. But rolls do have scales of a few km, larger than the non-organized convection (approx. 1 km) and than the inertial subrange ($<$ 500 m), but smaller than usual mesoscale. Scales between 1 and 10 km are somehow in between small scale and mesoscale, and could be either called "sub-mesoscale" or the authors should specify clearly what scales they are considering and which terms they use to denote them.

The terms used are now clearly defined,
"For B302 the main peak in the power spectrum of vertical winds occurs at a scale of approximately 4 km, which, as expected (Jonker et al, 1999), is of the order of the CBL depth ($\simeq$1.5 km, Section 3.1, Figure 4). As a result, throughout this paper, "boundary-layer scale" is used to describe variations on scales between 500 m and 5 km, *i.e.* on the order of the CBL depth. In this paper, "mesoscale" is used for larger structures (which are smaller than the synoptic scale)."

5. Page 8820, lines 10-14: Wouldn't it be possible to show a sounding as in Fig. 1, but with aerosol concentration? It seems from page 8827 line 29 that the authors

have aircraft profiles for this purpose, and it should illustrate well several of their points made about dust loading and possible exchanges between the different well mixed layers.

A dropsonde was used as it shows a good example of a well developed Saharan CBL growing into the Saharan SRL. Figure 5 now shows an aircraft profile and this is referred to in the text,
“Stratifications in dust loadings in the SRL and SAL (similar to the stratification in WVMR shown in Figure 1) were often observed during the campaign (e.g. Figure 5 and Marsham et al, 2008), as has previously been observed (Parker et al, 2005).”

6. Page 8820, lines 22-23: What is the rate of the FAAM BAe146 measurements for the different variables ?

This is now stated,
“A large array of instruments was present on the FAAM BAe146 aircraft during GERBILS and all data used were recorded at 32 Hz.”

7. Page 8821, lines 6-9: Since the authors are working on dust loading and its spatial variability and showing measurements of dust loading within the PBL, it would be better to — if not correct for — at least give an estimate of the effect of the dust loading of the few first hundred meters below the aircraft loaded with aerosols. There are some conditions when the assumption made here may not be so legitimate.

The error in surface temperature caused by the presence of mineral dust aerosol can be assessed by using the Airborne Research Interferometer Evaluation System (ARIES) which has previously been used to investigate the effect of mineral dust on thermal radiation in the atmospheric window region at 8-12 microns (Highwood et al, 2003). Measurements over ocean made while the BAe146 was in transit to the GERBILS operating region reveal that dust of optical depth of approximately 0.5 reduced the brightness temperature by an average of around 0.8 K (Jim Haywood, personal communication). While the precise error in using the Heimann broadband instrument will depend on the intrinsic properties of the dust (e.g. the refractive indices, the size distribution etc), the ambient atmospheric temperature, and the surface temperature, the aerosol optical depth below the aircraft is expected to be between 0 and 0.125 (given the nephelometer data between 0 and 250e-6 m^{-1} and the aircraft at 500 m AGL) and thus effects on the Heimann broadband BTs of up to 0.2 K.

Furthermore, this small offset to the Heimann BT is likely to be quite smoothly varying, since the column integrated aerosol from the aircraft to the ground is likely to vary more smoothly than the nephelometer signal from the aerosol at the flight-level.

This is now noted in the text,
“The dust below the aircraft is expected to affect brightness temperatures (BTs) from the Heimann radiometer by less than 0.2 K, and again be quite smoothly varying[1]”
“[1]Estimated using data from GERBILS data from over the ocean using the Airborne Research Interferometer Evaluation System, which has previously been used to investigate the effect of mineral dust on thermal radiation in the atmospheric window region, Highwood et al, 2003.”

8. Page 8821, lines 17-25: Considering B302 flight level (600 to 700 MSL) and PBL top height (900 to 1400 m MSL), this gives $z_* = z/zi$ between 0.4 and 0.8, which is not what one can call the “lower half of the boundary layer”. This is of primary importance, since the following sentence says “Therefore we expect to oberve convergence in the boundary-layer winds over warm surface anomalies” (with z_* ranging from 0.4 to 0.8, this is not what one can expect) and the authors discuss later in the text some peaks of convergence during that flight. So I suggest the

authors to check their numbers or statements and arguments. Flights B301 is made in the upper part of the PBL but B302 is between the mid and upper PBL.

As you noted there were errors in this analysis and this has been corrected using a more rigorous approach. This is now discussed in Sections 3.2,
“ The accuracy of the CBL depth shown by COSMO in Figure 5(a) lends some support to its accuracy elsewhere along the low-level transect of B302. Figure 4(d) shows this modelled CBL depth (shown by the dashed black line and determined as the lowest model level where the potential temperature was not more than 0.5 K than the modelled mixed-layer depth). This shows that B302 was within the lower part of the CBL (0.18 to 0.35 time the CBL depth), which is consistent with the observed convergence over warm land surface anomalies.”
and Section 3.2,
“CBL heights from COSMO suggest that the low-level leg from B301 was at an altitude approximately in the middle of the CBL, which is expected to make identification of convergence or divergence over land-surface anomalies difficult.”
“A region of high buoyancy (θ_v, red line) is observed downstream of this LST anomaly. This is further downstream than shown for B302 due to the larger along-track winds (Figures 4 and 9). This corresponds to a region of divergence in along-track winds (Figure 9(c)), which suggests at this point the aircraft was towards the upper half of the CBL and that the boundary-layer depth from COSMO was too large.”

9. Page 8822, lines 1-9: The COSMO simulation does not seem to be validated although the authors are using the wind fields and PBL height given by the model, and they never show any comparison between observations and model. I am curious to see what the model sees along the flight track. Wind, temperature, surface temperature, water vapour mixing ratio all considered in Fig. 4 and 7 could show what the model finds, even if the authors will have to take account of the change in time somehow in their representation. At least the wind direction

along the track should be compared, because the authors are using the COSMO wind fields (Fig. 3) to know about dust uplift source and advection, and consider the aerosol concentration and windspeed observed by the aircraft (wind direction is not shown in Fig. 4 and 7) to make their argumentation. Even if the direct comparison between aircraft measurements and the model might be difficult to make, it remains important to evaluate the discrepancies between the simulation and the observation before using the model to argue about the possible sources of the dust observed with the aircraft.

As described above in reply to “General Comment 4” COSMO is now evaluated using low-level aircraft data from the Saharan CBL and the only profile available that was unaffected by the monsoon.

10. Figure 2 - representation: kE (k) are plotted as a function of the wavelength in Fig. 2, rather than as a function of k . Also I think plotting the spectra with the usual logarithmic scale would be more appropriate, not only because that is more commonly used (the authors may have features that are better seen with a linear scale), but because it would show in a usual way the contribution of the turbulence scales, and also avoid the large scale variation hiding the contribution of smaller scales like for WVMR of flight B302 (top right panel). Otherwise, the authors should justify their choice of representation.

These have been replotted using log(E(k)) against k and the discussion revised accordingly (please see points 11 and 12 below).

11. Figure 2 - spectral gap: It is rather commonly accepted now that the usual “spectral gap” (Van der Hoven, 1957) is rarely observed in the real word, and at least not as usual as firstly thought. See e. g. Lenschow and Sun (2007) for re-

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cent works on spectra of scalars and wind components and for more references. So I suggest caution when talking about spectral gap as introduced by Van der Hiven (1957).

The reference to the “spectral gap” has been removed, since as you say it does not seem to be generally observed and is not important for the conclusions of this paper.

12. Figure 2 - WVMR: Authors should discuss more the WVMR spectra. I believe that the very small turbulent energy found in B302 is due to the flight level lower than for B301. There is probably no water vapour source at surface, and consequently no significant heat flux. The fluctuations in water vapour are mainly due to entrainment from the SAL into the PBL that result in large fluctuations close to the top, as seen on B301 WVMR larger energy spectrum.

This discussion has been revised,
“Variability in θ_v is lower for B301 than B302 and the boundary-layer scale contributions to variance in WVMR are much more significant for B301 (the peak at 10 km is also clear in the WVMR spectrum for B301). Surface latent heat fluxes in the Sahara are small compared with the surface sensible heat flux, and therefore BL convection is expected to create variability in WVMRs largely by the entrainment of dry air from the SRL into the CBL. The BL was much moister during B301 compared with B302 (approximately 9.5 gkg^{-1} compared with 5 gkg^{-1}, Figures 4 and 7) and COSMO simulations showed WVMRs in the SRL were similar on both days (approximately 4 gkg^{-1} and always drier than the CBL). Therefore, the greater boundary-layer scale contributions to variance in WVMR for B301 compared with B302 is probably because of the much stronger contrast between WVMRs in the CBL and SRL on this day. Furthermore, B301 was closer to the top of the BL where the effects of entrainment are likely to be more significant.”

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13. Page 8823, lines 21-25 and Figure 2: The authors do not discuss the large peak at large scales of about 100 km during B302.

This is now described,
"For B302, variations in virtual potential temperature (θ_v) and WVMR are dominated by larger scale contributions at scales around 100 km, particularly for WVMRs, but contributions on the "boundary-layer scale" are still significant."

14. Page 8823, lines 25-27: The statement "this greater contribution for B301 at scales between 1000 m and 20 km is thought to be due to the flight-path being orientated approximately along the axes of boundary-layer rolls" — on which a following discussion and conclusions are made later — is wrong. One observes greater fluctuations when flying across convective rolls than along them. The closer to the transverse axis, the larger the variance associated with the rolls. The closer to the longitudinal axis, the smaller the variance.

This statement has been removed. However, given a regular field of BL rolls the scale of the contribution made by the rolls to the variance in BL variables will increase as the aircraft flight track changes from across the rolls (with the contribution on the scale of the rolls spacing) to along the rolls (infinite scale). However, the total variance introduced by the rolls is expected to be the same, as long as many rolls are sampled (impossible given a flight track exactly along the rolls, and perfectly regular rolls, but this does not occur in reality).

In the LEM simulation based on B301 (Figure 10) the roll spacing was approximately 2.5 km. Therefore, if the BL convection was organised into rolls then the observed contribution to the spectrum of vertical winds contribution at 10 km would have to be from a flight track oriented to some extent along the rolls. This is consistent with the orientation of the modelled BL convection shown in Figure 10. This is now briefly discussed in Section 3,

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“The vertical velocity spectrum for B301 is similar to that from B302, except that the peak occurs at a larger scale (approximately 10 km). This shows a larger scale of organisation in the boundary-layer convection than for B302 and, as discussed in Section 3.3, this may be a result of the flight-path being oriented approximately along the axes of linearly organised boundary-layer structures.”
and in Section 3.3,
“ It is possible that such linear organisation of the boundary-layer convection, almost along the east to west flight-track of B301, explains the peak in the power spectrum of vertical winds for B301 at approximately 10 km (Figure 2).”

15. Page 8824, lines 11-13: Since the wind is northerly along this track, only the NO-SE elongation of the smaller albedo feature can explain that its effect on the BL potential temperature can be observed. Otherwise, it would be advected downstream, that is south of the aircraft track. What has to be explained then is that the increase in virtual potential temperature is observed right over the patch rather than slighty before, as if the wind was exactly aligned with the small albedo feature.

The aircraft is approximately 350 m AGL. Given an updraught speed of 1 ms^{-1}, typical of the BL and observed and an across-track wind of 12 ms^{-1} an albedo feature has to have an extent of approximately 4 km to the north of the track for it to be observed. Coherence between LSTs and θ_vs was only significant on scales of more than 10 km along the flight-track. Presumably such anomalies typically have a similar extent upstream and can be observed.

The feature at 8°W was aligned approximately NW to SE. However, the winds were also from the west of north, so it is not surprising that the region of increased θ_v is approximately over the albedo feature - and may also precede it (it is unclear in Figure 4).

This is now noted,

"This anomaly in potential temperature is approximately in phase with the albedo anomaly as the albedo anomaly has an extent upstream, extending approximately in the wind direction (Figure 3)."

16. Page 8824, lines 14-15: It is not only the albedo feature but also the change in terrain in this area that has an effect on the boundary layer, making it locally deeper.

This is now noted,
"In this case, where there is a change in orography co-located with the albedo feature, this effect may be enhanced by the effect of the orography on the BL."

17. Page 8824, lines 16-20: The decrease of albedo at 6.7, 7.7 and 9.2°W are much smaller to that discussed before, as noticed by the authors, but the possibly corresponding increase in virtual temperature is not much smaller than the increased found at 8°W. Is there an explanation for this ?

When the overall increase in BL temperature with time is accounted for, the BL temperature perturbation at 8°W is larger than those at 6.7, 7.7 and 9.2°W.

18. Page 8824, lines 21-23: "West of 9.5°W, the air is moist and dusty. [...] the COSMO model showed this was from the monsoon flow (Fig. 3b)". Why would monsoon flow be dusty ?

The monsoon air at low-levels was consistently dusty during GERBILS. This is likely due be due to the monsoon flow acting as an intrusive density current overnight (BouKaram et al, 2008), and in some cases embedded cold pool outflows (Flamant et al 2007, Marsham et al, 2008). This is now referred to in the text,

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“the monsoon air close to the ITD was usually observed to be dusty during GERBILS, as discussed by Marsham et al, 2008”

Also Figure 3b shows a northwesterly flow along the track at low altitude, which does not seem to be monsoon. Only west of 11°W, one can see a westerly flow, coming from the Senegal coast (and so maybe not appropriately called monsoon). What about the wind measured by the aircraft ? It seems essential to consider it as well when interpreting the aerosol concentration measurements and using the COSMO wind field for the analysis.

The clearest monsoon front, in terms of WVMR, is at 10°W (Figure 3b). WVMRs were elevated just to the east of this so data west of 9.5°W were rejected. There are often differences in the location of the inter-tropical discontinuity (ITD) as defined by dew-point and winds (Hastenrath, 1985). As is typical during the day, (Parker et al , 2005) when there is strong dry convection, the windspeed does not show any clear change at 10°W (the location of the front in WVMR). However, Figure 3(a) shows a region of low windspeeds at 17 to 20°N, which is the convergence zone of the ITD. This intersects flight B302 at approximately 10°W.

During the 27th (B301) there was a northwards surge in the monsoon which at midnight (according to ECMWF analyses) gave southwesterly winds as far north as 19°N at 10°W. Similar winds can be seen 12 hours later in Figure 3(c). As such the moist air observed west of 10°W is the remains of this monsoon flow. Figure 3(c) is now referred to in the text,
“ Simulations performed using the COSMO model showed this moist air remained from the previous days monsoon flow (Figures 3(b) and (c))”

19. Page 8824-line 24 to page 8825-line 2: The authors need to be clearer here about which wind maximum they are talking about. They seem to consider the local maximum along the track. But since the wind is NNW, they need to consider

the wind field NNW to the considered trajectory segment.

This is been rewritten,
"This is consistent with advection of dust into the flight-track from a location upstream, where the windspeed maximum was further to the east than the windspeed maximum on the flight-track. Such an eastwards displacement of the upstream windspeed maximum is shown by the COSMO model (Figure 3(b)), although as noted there are significant errors in the COSMO wind field. Overall, this does however suggest that much of the dust observed was probably uplifted by the high windspeeds upstream, rather than locally ."

20. Page 8825, line 4: "for example at 8.5, 7.9, 7.7 and 6.7°W" : There is no local increase of dust concentration at 7.9°W.

This has been removed.

21. Page 8825, line 10: "for all scales discussed (*i.e.* greater than 2.5 km, not shown)" : The authors should explain why they do not consider smaller scales, that can be important in the context of their study.

Although smaller scale processes are clearly important for processes such as dust uplift, smaller scales are not discussed since the cospectral analyses presented did not show coherence between the land-surface and the atmosphere on these smaller scales. This is now stated,
"For the remaining data cospectral analysis showed that there was a significant relationship between albedo and LST for all scales where a relationship between LST anomalies and boundary properties were observed (*i.e.* $>$ 10 km, but in fact for all scales $>$ 2.5 km, not shown)."

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22. Figures 5, 6 and 9: What is the goal of showing the coherence squared rather than coherence, which is more usual ?

Coherence squared is usually used (Matthews and Madden, 2000).

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