

Interactive  
Comment

## ***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.

Received and published: 8 September 2008

**23. Page 8825, lines 14-20, “which is consistent with the along-track winds... [...] similar coherent relationship between along-track winds and LSTs...” : But the flow is mainly transverse to the track (further in the text, line 25: “across-track winds were greater than the along-track winds”). The authors can make this argument only for an LST anomaly patch that is sufficiently elongated to the north (which seems to work for the strongest sharp albedo decrease at the border of the plateau at 8° W, but what about other patches ?). Otherwise, any impact of an LST increase below the track would be advected south of the track and consequently not probed.**

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The aircraft is low (approximately 350 m AGL), so the features do not have to be very elongated across the flight track for them to be detectable (perhaps 4 km, with a  $1 \text{ ms}^{-1}$  updraught speed, as discussed in reply to point 15 above). This issue is now discussed in the text at this point,

“which is consistent with the along-track winds ( $\simeq 4 \text{ ms}^{-1}$ ) resulting in the high buoyancy air being located downstream of the high LSTs and these LST anomalies having sufficient upstream extents to have observable effects on the boundary layer along the aircraft track. Effects on the CBL from LST anomalies with too small an upstream extent would have been impossible to observe, since they would have been advected downstream, away from the flight-track.”

**24. Page 8825, line 16, “with convergence towards regions of high  $\theta_v$ ” : As pointed before, this is relevant if the flight level is not higher than  $0.5 z_i$**

Please see the reply to point 8 above regarding the height within the CBL.

**25. Page 8825, line 21, “the high  $\theta_v$  regions are dry” : Wouldn’t this lead to a phase between  $\theta_v$  and WVMR of  $180^\circ$  ? Fig. 5 shows a phase around  $135\text{--}90^\circ$  close to that of  $\theta_v$  with the along-track wind.**

This has been revised, “Figure 6 shows that on scales greater than 20 km the high  $\theta_v$  regions tend to be drier (WVMR is out of phase with  $\theta_v$  by between  $135^\circ$  and  $180^\circ$  on these scales).”

**26. Page 8826, lines 11-13, “but the monsoon affected regions east of  $6.6^\circ\text{W}$  and west of  $9.9^\circ\text{W}$ ” : Fig. 3c shows westerlies all along the low level leg. How do the authors define the regions affected by the monsoon ?**

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There were clear fronts in WVMR at 6.2 and 10.2°W. These gave dew-points above 287 K so can be defined as the ITD (Hastenrath, 1985). To the west and east of these fronts elevated WVMRs were observed, so only data from 6.6 to 9.9° W were used to exclude regions affected by the monsoon. This is discussed in Marsham et al (2008) and this is now stated, “but monsoon air was present east of 6.6°W (and probably also convectively generated cold pools, as discussed by Marsham et al, 2008).”

**27. Page 8826, lines 19-20, “Since windspeeds upstream were lower than those at the point of observation, the observed dust must be from local uplift” : this is not a convincing argument. The aircraft flies at a speed of about 10 times the wind- speed. The fact that the wind increase toward the east as well actually makes it possible to argue that the increasing dust loading is related to that increase in the wind: as the aircraft flies to the west, the wind decreases and it is less and less likely that it lifts dust up. However, local peaks of dust concentration (like at 7.7 or 8.4 ° W) would more convincingly be related to local “convective” uplift.**

This has been rewritten,

“Figure 8 shows windspeeds (which are dominated by the along-track winds) increasing from west to east, which is consistent with the COSMO simulation (dashed lines in Figure 8 and also see Figure 3(c)). COSMO gives an accurate forecast for the low-level leg, with temperatures within 1 K of those observed, WVMRs approximately 2 gkg<sup>-1</sup> too moist and east of 8.3°W, windspeeds within 1 ms<sup>-1</sup> of those observed. The errors in windspeed increase towards the west, reaching approximately 4 ms<sup>-1</sup> at 9.5°W (Figure 8).

Dust loadings west of 8.8°W were low, but are observed to increase towards the east with the increasing windspeeds (Figure 8(b)). Winds were oriented from approximately 260° *i.e.* almost along the west to east flight track. Windspeeds upstream were lower

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than those at the point of observation and COSMO simulations showed 10 m windspeeds increasing from midnight to the time of observation. East of 8.5°W there was a strong positive correlation between observed windspeeds and aerosol scattering (Pearson correlation coefficient = 0.8, not shown). Therefore, it is likely that the dust observed was uplifted locally along the flight-track, in the area of higher windspeeds shown by COSMO to be oriented along the flight track (Fig. 3(c), 6.6 to 8.5°W).”

Please see Figure 2 (in [http://homepages.see.leeds.ac.uk/~lecjm/ACP-2007-0539/acp-2007-0539-add\\_figs.html](http://homepages.see.leeds.ac.uk/~lecjm/ACP-2007-0539/acp-2007-0539-add_figs.html)). for the correlation between dust and windspeed.

**28. Page 8826, lines 21-23 : Note that the sharp increases in albedo at 8.7 or 9.2 °W, associated with increases in BT, do not seem to be associated with decrease of  $\theta_v$  .**

We apologise, this sentence should have been,

“and effects on the BT from *high* albedo features can be seen at 8.7 and 7.0°W.”

We have clarified this using,

“Decreased BTs from high albedo features can be seen at 8.7 and 7.0°W (Figure 8) which perhaps lead to decreased  $\theta_v$ . The narrower high albedo at 9.2°W is associated with a decreased BT, but any effect on  $\theta_v$  is unclear.”

**29. Page 8826, lines 24, “region of high buoyancy...” : How can one explain that the signature of this patch on the BL temperature is smaller than the patch at the surface, while in case of B302, it was larger with smaller wind ?**

If both  $\theta_v$  peaks (7.78 to 7.93°W) to the east of the high BT anomaly are from that anomaly then the area of increased  $\theta_v$  is not smaller than the BT anomaly.

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**What about the larger and wider increase of BL  $\theta_v$  further west (7.2-7.6 ° W) ? (Could it come from the same source of BT patch but earlier in time than the detail shown, and so further downstream; but also from another source not seen on the BT measurements that is not below the flight track ?). Because its amplitude is larger than the one discussed, the authors should not ignore it.**

Given the along-track windspeed of approximately  $10 \text{ ms}^{-1}$ , at 600 m AGL we do not expect to see the effects of a 30 km wide BT anomaly 60 km downstream. It is unclear where the  $\theta_v$  anomaly at 7.2 to 7.6 originates from,

“ The elevated  $\theta_v$  between 7.2 and 7.6°W does not appear to be related to any LST anomaly.”

**30. Page 8827, lines 4-6 : How can the authors explain the large difference from B302 to B301 between the phase between  $\theta_v$  and along-track wind shown on the black curve of the sub-panels in Fig. 5 and 9 ?**

This plot has been removed (please see reply to general comments above).

**31. Page 8827, lines 8-15 : This paragraph has got several statements that need further investigation before some conclusions can be drawn.**

**(i) Page 8827, lines 9-11, “WVMR is also related to  $\theta_v$  ... and in phase with  $\theta_v$  ” : This is the opposite result to what was observed in B301 (page 8825, line 21, “the high  $\theta_v$  regions are dry”). So it would mean that different processes occur that need clarification. For B302, the authors suggest the effect of entrainment, while the flight level is lower in the BL, that is further from the top, where the dry intrusions have their sources (that are often at smaller scale than 5 km (see Couvreux et al (2007) or Lothon et al (2007)). “moist updraft” are typical of thermals, but the ground in this case is probably very dry and the latent heat flux close to**

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surface must be close to zero, while the entrainment flux at the top may be large, due to entrainment processes.

This has been removed (please see reply to general comments above).

**(ii) Page 8827, line 13, “updrafts on scales larger than 4 km, which includes the scales of boundary-layer eddies” : This is not right. Boundary-layer eddies are precisely less than 4 km. Boundary-layer rolls are example of structures that can be at larger scales, but turbulent heat fluxes and mixing are usually at smaller scale than this, and at least contribute for a very significant part. The spectra in Fig. 2 show that small scale contribution in variances. Cospectra (or coherence) should show it for turbulent fluxes.**

This has been removed (please see reply to general comments above).

**(iii) Page 8827, line 14-15, “Given the strong along-track winds on this day this is suggestive of boundary-layer rolls.” : The strong along-track wind does not make the rolls more probable, even harder to detect if the rolls are aligned with the wind, as noted previously. A more direct way to check whether rolls were observed or not is to use the autocorrelation function of  $w$  first, but also  $\theta_v$ , WVMR (and dust if the sampling rate allows it). Rolls would appear as a strong periodicity of the autocorrelation function, with significant correlation of secondary repetitive maxima. The vertical velocity estimates are not absolute, but allow the analysis of coherence, cospectra and auto- or cross-correlations. Since the authors are basing their argumentation on the possible occurrence of rolls and on updrafts that transport dust, they should consider analysing more thoroughly the fluctuations of vertical velocity, and their link with dust. The spectra shown in Fig. 2 are not sufficient to justify the presence of rolls, nor is the rest of the**

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## argumentation.

This has been removed (please see reply to general comments above).

**32. Page 8827, lines 16-17, “... confirm that we expect linear boundary-layer structures...” : Band-like structures like convective rolls would appear much more organized and elongated than what is shown in Fig. 10.**

This has been removed (please see reply to general comments above).

**33. Page 8827, line 18, “The roll-spacing of approximately 2.5 km...” : As noted before for the observation, there is a quantitative way to estimate the spacing using the correlation function (see e. g. Lohou et al (2000), Lothon et al (2007)).**

This has been removed (please see reply to general comments above).

**34. Page 8827, line 25, “Due to the latent heat fluxes moistening the boundary layer...” : See comment above about likely negligible latent heat flux close to the ground that need caution when using the term “moistening”.**

This has been removed (please see reply to general comments above).

**35. Page 8827, line 27, “updrafts are dusty”: See comment above about considering the vertical velocity in a larger extent to lend further support to your argumentation.**

This has been removed (please see reply to general comments above).

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**36. Page 8827, line 27, “consistent with dust uplift at the surface”:** Authors should not mix the uplift and the transport. Rolls are often not detected close to surface, because the surface layer processes are less organized and of smaller turbulent scales. Rolls build above a certain height within the mixed layer, and should not participate directly to the dust uplift but more on the dust distribution in space and its transport. Dust uplift, when considering the turbulence convection, would be more due to individual thermals that are strong enough when they start that they are associated with large wind gusts at the surface. That is why an analysis of the turbulent moments (variances, momentum and heat fluxes, turbulent kinetic energy,...) along the track would probably lend further to the analysis.

This has been removed (please see reply to general comments above).

**37. Page 8828, line 1: When and where were the aircraft profiles made and do they lend support to the COSMO simulation ?**

Only one profile was not affected by the monsoon. This (the ascent from the eastern end of the low-level leg of B302) is now discussed in the text (please see the reply to general comment 4 above).

Profiles from B301 were affected by the monsoon. However, the good agreement between COSMO and the aircraft data on the low-level transect of B301 this supports the use of COSMO in identifying the region of local dust uplift in the area of elevated windspeeds along the aircraft track.

**38. Page 8828, lines 4-5, “these two factores are expected to make the detection of any roll effects in data from B302 more difficult”:** No, as noted before, a transvers flight track is much more favorable for detecting rolls.

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This has been removed (please see reply to general comments above).

**39. Page 8828, line 6, “Variations in windspeed in the LEM were very similar to those observed”: It may be more accurate to say “spatial distribution of the windspeed in the LEM” rather than “variations”. Did you use the LEM distribution along the flight track or over the whole 2D domain ? Those can differ, and the 1D distributions can vary with the orientation of the line chose especially in a field of band-like structures.**

This has been corrected to,

“The probability density function of the windspeeds from the LEM is very similar to that observed (Fig. 10b),”

It is the pdf of winds at the lowest level that controls the dust uplift, so it is important to evaluate the pdf of windspeeds, which is only possible at the flight level.

**40. Page 8828: Equation (1) is incorrectly spelled. According to Marticorena et al (1997), it should be  $(1 + R)(1 - R^2)$ .**

This has been corrected. The plots are unchanged as the correct formula was used.

**Also the friction velocity is commonly spelled  $u_*$ , not  $u^*$ .**

This has been corrected.

**Also in the quoted reference, Marticorena et al (1997) integrate over a surface and over the particle size distribution, so the authors should specify their way to simplify the equation for their purpose, with the assumptions made.**

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This is now stated,

“ $u_{*T}$  depends on the particle size and Marticorena et al (1997) integrate over the size distribution, with contributions from different sizes weighted by their areal abundance.”  
and

“ We do not consider the effects of particle size on the uplift threshold, but look at uplift rates as a function of this threshold.”

It is noted that,

“It is not clear to what extent parametrisations and thresholds used by Cakmur et al (2004) and Marticorena et al (1997) are applicable on the small spatial and temporal scales resolved by the LEM. Therefore the quantitative details of the enhancement discussed above remain somewhat uncertain. In addition, scales smaller than those by the LEM may be significant for dust uplift. However, despite these uncertainties, the estimated enhancement of uplift, support the other existing evidence that boundary-layer convection plays a significant role in driving dust uplift in the Sahara (Cakmur, et al, 2004).”

and in the conclusions that,

“The applicabilities of dust uplift parametrisations and thresholds in the literature to small spatial and temporal scales are not clear, which increases the uncertainty in the details of these enhancement effects. We recommend that the range of the applicability of such parametrisations should be investigated.”

**41. Page 8828, lines 20-21, “neglecting effects from any spatial variations in the stability” : Meanwhile, you are discussing the role of the convection in dust uplift which is strongly linked with instability. Also the shift from convective rolls to convective cells is mostly governed by instability (Weckwerth, 1999).**

This is true, but we are not neglecting stability effects here, only the spatial variations in stability resolved by the LEM over the model domain. The aim here is to evaluate

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the contribution of boundary-layer convection to dust uplift, by obtaining “an estimate of the effects of the modelled boundary-layer convection on dust uplift by comparing the rates calculated using the LEM winds from the lowest model level, and rates calculated from the windspeed derived from the mean wind velocities in the lowest model level”.

Dust uplift is normally modelled as a function of  $u_*$ . If effects from any spatial variations in the stability are neglected then  $u_*$  is proportional to  $u_1$  (as used by Cakmur et al, 2004).  $u_*$  is affected by stability, however (equation 3).  $\psi$  is a function of  $z/L$  where  $L$  is the Monin-Obukhov length, which is itself a function of  $u_*$ . We can calculate the contribution of the spatial variations in  $\psi$  to variations in  $u_*$  by calculating  $u_*$  using  $\psi = 0$ , using this value of  $u_*$  to calculate  $L$ , and then calculating  $\psi$  using this  $L$ . This was then repeated to iteratively calculate  $u_*$ . Pdfs of  $u_*$ , calculated using  $\psi = 0$  the converged iteration of  $\psi$  are shown in Figure 3 (in [http://homepages.see.leeds.ac.uk/~lecjm/ACP-2007-0539/acp-2007-0539-add\\_figs.html](http://homepages.see.leeds.ac.uk/~lecjm/ACP-2007-0539/acp-2007-0539-add_figs.html)). They are almost indistinguishable. This justifies neglecting the spatial variations in  $\psi$ , since they are dominated by spatial variations in the wind. This is now stated in the paper, “Therefore, for a constant surface roughness, if effects from any spatial variations in the stability are neglected (as used by Cakmur et al, 2004)  $u_*$  is proportional to  $u_1$ . A calculation of the variability in  $u_*$ , with and without spatial variations in  $\psi$  showed that the variability in  $u_*$  was completely dominated by variations in  $u_1$  (not shown), justifying the neglect of the spatial variability in  $\psi$ .”

**42. Page 8829, lines 1-9: The authors should explain more clearly their way to calculate the uplift rates and their way to separate the contribution of rolls which is not clear. This part of the manuscript is important because it gives an attempt to estimate the impact of the km scale thermal and dynamical structures on dust uplift, but the presentation and explanations are not straightforward. And since the evidence of rolls is not convincing in their manuscript, they may need to use a different terminology of the processes that they are discussing. What they**

**actually seem to estimate is the role of the resolved and unresolved eddies on dust uplift rates with a parametrisation of uplift rates based on friction velocity.**

We were intending to present, “an estimate of the role of the resolved and unresolved eddies on dust uplift rates with a parametrisation of uplift rates based on friction velocity.” as you say. We have now clarified this,

“an LEM simulation is used to investigate the expected contribution to dust uplift made by the boundary-layer circulations resolved by the LEM, that would not be resolved by a regional or global model.”

“We can therefore obtain an estimate of the effects of the modelled boundary-layer convection on dust uplift by comparing the rates calculated using the LEM winds from the lowest model level, and rates calculated from the windspeed derived from the mean wind velocities in the lowest model level”

and in the conclusions,

“ assuming an uplift threshold of  $8 \text{ ms}^{-1}$ , the LEM results showed that the boundary-layer convection resolved by the LEM is expected to have increased dust uplift by approximately 30%, compared with uplift calculated using the mean wind.”

**43. Page 8831, line 16-18, “We therefore suggest that the impacts of these processes on dust uplift and transport are investigated using numerical modelling, or using observational data not available to us” : Are the authors thinking of a specific dataset that would be relevant for the topic but is not available ? AMMA experiment should give useful data to study this issue.**

In order to investigate the effects of albedo anomalies on the growth of the Saharan CBL into the Saharan Residual Layer (SRL) using in-situ data you would need long legs within the SRL, ideally with another aircraft in the CBL. We are not aware of any such datasets (there is very limited AMMA data from the Saharan CBL, away from mountains). It may be possible to investigate this using remote sensing, but this is

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beyond the scope of this paper. We have clarified this remark by now stating, “We therefore suggest that the impacts of these processes on dust uplift and transport are investigated using numerical modelling, or using future observational datasets.”

## 0.1 Technical corrections

### 1. Page 8819, line 25: Washington et al, 2005 (not 2006).

This reference has been removed in the light of the analysis of Knippertz (2008).

### 2. Page 8831, line 23: “for” apperas twice.

Corrected.

### 3. Figure 5, caption, “Horizontal black dashed lines show 80, 90 and 95 significance thresholds respectively” : Add “from bottom to top”

This has been added.

## 1 Additional references

Bou Karam, D. C. Flamant, P. Knippertz, O. Reitebuch, J. Pelon, M. Chong, M. and A. Dabas: 2008, “Dust emissions over the Sahel associated with the West African Monsoon inter-tropical discontinuity region: a representative case study”, Quart. J. Roy. Meteorol. Soc., 134, 621-634.

Hastenrath, S.: 1985, “Climate and Circulation in the Tropics”, D. Reidel Publishing Company, page 163.

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 8817, 2008.

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