Atmos. Chem. Phys. Discuss., 8, S4037–S4050, 2008
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Atmospheric Chemistry and Physics Discussions

ACPD

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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.

Anonymous Referee #1

Received and published: 24 June 2008

Journal: Atmospheric Chemistry and Physics

Manuscript ♯: acpd-2007-0539

Title: Observed boundary-layer/mesoscale impacts on Saharan dust

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General comments

The authors consider two flights in the Sahara during the GERBILS campaign to study the effect of land surface temperature variation on boundary-layer (BL) mesoscale spatial variability in wind and temperature and on dust uplift, at scales $>$ 2 km and $<$ 20 km. They are stating that they observe convective boundary-layer rolls that increase the dust uplift and they use a mesoscale model and a Large Eddy Simulation to lend further support to their analysis and to estimate the contribution of the rolls.

The subject is of practical interest and challenging. Convection definitely lifts and transports dust within the BL through the turbulence associated with thermal activity and shear at surface. Structures at larger scale participate to their mixing and transport. In the global models, the uplift rate is parameterized and deduced from the wind at the first level. No-wind situation, as pointed by the authors, will typically raise issues, since they will be cases with potential large role of convectively driven boundary layers, with large wind gusts due to thermal activity. So that the GCM will have to represent correctly the subgrid turbulence kinetic energy and friction velocity to properly estimate the dust uplift.

However, the authors often seem precipitate to draw conclusions from their observations and numerical simulation. Several arguments are uncorrect or not sufficiently convincing, although crucial for the conclusions drawn.

1. When considering weak winds with convection as the main contributor of dust uplift, it is likely more due to trigerring thermals and especially dust-devils than due to rolls and mesoscale organization. The later are found above surface layer and even higher, and they have a major contribution to vertical transport, and also to the spatial distribution of dust, but probably not directly to dust uplift. Unless the shear associated with them plays a crucial role that would then need to be proven and estimated. Note that Koch and Renno (2005) whom the authors quote found

a contribution of 34% due to convection, including 26% due to dust devils. That is within the convection contribution, more than 75 % is due to dust devils rather than non vortex thermals.

2. In any case (weak wind or moderate wind), the occurrence of the convective rolls need to be better proved in the present study, as well as their direct contribution (through the shear associated with them for example) or the authors should not focus on well organized rolls specifically (if neither the observations nor the LEM convincingly show their existence). See several specific comments below for this aspect.

3. The authors do not consider scales smaller than 2 km in their study. It is very interesting to consider scales $>$ 2 km and $<$ 20 km for studying the impact of LST heterogeneity on wind circulation in the boundary layer and possible impact on aerosol transport, but since the authors are considering the role of convection, and albedo anomalies that are responsible for varying potential temperature and depth of the BL, it seems important to consider the smaller scales of the associated processes (with for example an analysis of the variability of turbulent kinetic energy, heat fluxes, and other variables that can estimated during the low level leg). An analysis of the turbulent kinetic energy and turbulent fluxes would enrich and lend further support to their analysis of the BL larger scale variability in wind, temperature and dust lift and transport.

4. Before leaning on the COSMO mesoscale modelisation to deduce some processes of dust transport, the authors need to validate the simulation. The validation of the LEM is also short.

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

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mesoscale. Maybe: ‘Observed impacts of boundary-layer mesoscale variability on Saharan dust’.

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

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.

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 ($\sim$ 1 km) and than the inertial subrange ($<$ 500), 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.

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.

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

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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.

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 $z_* = z/z_i$ 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.

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.

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.

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 recent 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).

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.

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.

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 be-

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ing 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.

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.

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.

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 ?

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 ? 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.

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.

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.

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.

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

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.

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

25. *Page 8825, line 21, 'the high θ_v regions are dry'*: Wouldn't this lead to a phase between θ_v and WVMR of 180° ? Fig. 5 shows a phase around 135-90° close to that of θ_v with the along-track wind.

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26. *Page 8826, lines 11-13, 'but the monsoon affected regions east of 6.6°W and west of 9.9°W'*: Fig. 3c shows westerlies all along the low level leg. How do the authors define the regions affected by the monsoon ?

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 aircrat 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.

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 θ_v.

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 ? What about the larger and wider increase of BL θ_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.

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

31. *Page 8827, lines 8-15*: This paragraph has got several statements that need

further investigation before some conclusions can be drawn.

- *Page 8827, lines 9-11, 'WVMR is also related to θ_v... and in phase with θ_v'*: This is the opposite result to what was observed in B301 (page 8825, line 21, 'the high θ_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 surface must be close to zero, while the entrainment flux at the top may be large, due to entrainment processes.
- *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.
- *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 θ_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

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possible occurrence of rolls and on updrafts that transport dust, they should consider analyzing 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 argumentation.

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.

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 estime the spacing using the correlation function (see e. g. Lohou et al (2000), Lothon et al (2007)).

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'.

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.

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.

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

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.

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.

40. *Page 8828:* Equation (1) is uncorrectly spelled. According to Marticorena et al (1997), it should be $(1+R)(1-R^2)$. Also the friction velocity is commonly spelled u_*, not u^*. 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.

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).

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 straighforward. And since the evidence of rolls is not convincing in their manuscript, they may need to use

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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.

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.

Technical corrections

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

2. *Page 8831, line 23:* 'for' apperas twice.

3. *Figure 5, caption, 'Horizontal black dashed lines show 80, 90 and 95 % significance thresholds respectively'*: Add 'from bottom to top' ?

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Weckwerth, T.: 1999, ‘An observational study of the evolution of horizontal convective rolls’. *Mon. Wea. Rev.* **127**, 2160–2179.

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