Review for ACPD manuscript acp-2014-657 „Parametrization of convective transport in the boundary layer and its impact on the representation of diurnal cycle of wind and dust emissions“ by F. Hourdin et al.

General:

This paper presents results for winds and dust emission over West Africa from simulations with the model LMDZ for December 2005 to March 2006 nudged to ERAI reanalysis. A new parameterization NP for the mixing in the daytime boundary layer is applied to analyse the effect on dust emission. The results for near-surface wind are compared against simulations with the former standard parameterization SP and observations, of which the latter is limited to two stations away from dust sources. In addition the aerosol optical thickness (AOT) and surface concentrations are compared at locations away from dust sources.

I welcome this study and see the value for dust modelling, but I recommend to revise the strong conclusions and weak physical explanations prior to publication. My first main concern is the weak evidence for the conclusion that NP improves winds for dust emission. The near-surface wind validation at Banizoumbou and Chinzana away from dust sources indicate a worse and better performance relative to SP, respectively. The LLJ at Banizoumbou is stronger with NP, but 10m-winds in ERAI compare better with the observations at the morning. This inconsistency is not discussed. Conclusions on emission are than drawn by relating these wind changes to a similar signal in one time series of winds of 11 days at one grid cell at the southern margin of the West African dust maximum. More evidence is needed to support the general conclusion that NP is better for dust modelling. The effect of the new Weibull distribution relative to the NP of plumes is not discussed. My second point addresses the physical explanation of boundary layer dynamics. The downward momentum mixing due to mechanical turbulence generation by the LLJ itself should be considered in the explanation of the results.

Specific:

- *Lines 7 -9/3: „The most uncertain dust-related process is emission which depends non linearly upon the friction velocity. Experiments indicate“* References to existing literature are missing here. Observations based on which the models have been developed also show these relationships. Please consider adding information.

- *Lines 19 -20/3: „ which corresponds to a quasi systematic maximum of winds in the observations“* It is not clear what is meant with “quasi systematic”. Consider removing this clause.

- *Lines 16-18/4: „ i. e. upward the gradient of potential temperature since the atmosphere is generally neutral or even somewhat stable above the first few hundred meters which corresponds to the (unstable) surface layer.“* You probably want to focus on the boundary layer not the atmosphere as a whole. The unstable surface layer varies in height and is only present during the day. Please revise this sentence.

- *Lines 8-9/5: „in which the turbulent diffusion alone is at work“* Vertical wind shear can be important for turbulence generation in the nocturnal boundary layer, e.g. when a LLJ occurs.

- *Lines 9-10/5: „ daily conditions in which the role of turbulent diffusion is confined to the surface layer“* You here use the term surface layer for the lowest few cms above the ground whereas in line Lines 16-17/4 you use it more generally for the entire lower unstable part of the boundary layer. Please resolve this inconsistency.

- *Lines 2-7/6: „primitive equations of meteorology and conservation equations for trace species.“* LMDZ probably also has conservation of other quantities than just tracers.

- *Line 16/7: „large scale“* Ambient air is probably more appropriate here.

- *Line 14/7: „ large scale model variables“* It could also be mesoscale.

- *Line 14/7: „a classical approximation in parameterizations“* Please provide reference(s). Why is the assumption valid?

- *Line 17/7: „which is equivalent to neglect the plume fraction αth in this part of the computation“* The equivalence is not clear from the information that has been given. Does this imply that the fractional coverage of the plume has no vertical dependency anymore? If so, why do you introduce αth than?

- *Lines 8-18/9:* Please specify the particle size range from your model. Also check singular/plural forms in this paragraph.

- *Lines 25-26/9: „the zoom was chosen so as to get a quasi uniform 1 × 1 resolution over a (70 W–30 E; 10 S–40 N)“* Does the regional nesting not always have a resolution of one degree or what do you mean with “quasi uniform”?

- *Lines 12-15/11: „75 Mt for the NP version. The latter value is already in the lower range of current estimates of the climatolgical total dust emission by North Africa for March (see e.g. Figure 6 of Laurent et al., 2008).“* Dust emissions have a large uncertainty. How do your simulated values for March compare to other studies?

- *Line 20/11: „in the main emission area in Mauritania“* Why have you chosen to present a grid cell at the southern margin of the emission maximum and not in the centre of the West African dust maximum? A selection of more than one grid cell over more time periods or a statistical approach capturing extreme values would be better to support the strong conclusions you draw later.

- *Lines 7-15/12:* How does this result change when you analyse other grid points, e.g. in the centre of the West African dust maximum? A few days at one grid point is a too small sample for your conclusion, that NP is producing overall more variability and larger peak winds. I would expect that from the NP but more evidence from the region of dust emission is needed. In this context, how do you know that the winds change due to NP of convective plumes and not due to the introduction of a Weibull distribution for winds?

- *Line 19-27/13:* It is interesting that NP shows an improvement at one station but not at the other one compared to observation. Why does NP overestimates the maximum winds at Chinzana? This needs to be discussed since you conclude that NP improves the model performance.

- *Line 26-27/13: „than the absolute mean value and mean field“* The meaning of this is not clear.

- *Line 8-9/14: „Note that there is also a significant and systematic increase of dust when weakening the nudging, going from τ = 3 h to 48 h“* This indicates that the relaxation to ERAI winds suppresses the development of strong winds at M'Bour causing the underestimated emission and concentration. However, the observed morning winds at Banizoumbou compare better with ERAI than SP, NP3 and NP48, despite a stronger LLJ with NP.

- *Line 17-19/14: „The fact that the improvement is slightly smaller for large values is consistent with the larger role played by large scale dynamics for those events. But even then, the representation of the diurnal cycle of winds plays a significant role.“* Please explicitly show that the large values are connected to large scale events and/or provide other evidence from the literature for supporting this statement.

- *Lines 5-15/15:* I note that you name possible reasons for the over-/underestimation at the two stations here. Please add a reference to this discussion on page 13 (see comment above) or consider to change the arrangement of the text.

- *Lines 12-15/15: „In particular, tuning of emission algorithms with overestimated winds from reanalyzes may lead to artificially underestimate the emissions when better winds are given to the emission module, as is the case here.“* This is based on a station away from emission sources. Relating the finding to a similar signal in one grid cell for 11 days does not allow to support this strong statement. Please provide more evidence, since other studies (that you cite in the introduction) have shown the contrary, namely a model underestimation of wind speeds in the Bodele as important dust source in winter.

- *Lines 10-18/16:* The Richardson number is named already earlier in the manuscript and would be helpful to explain mechanical production of turbulence below the LLJ. Consider to describe it in the introduction.

- *Lines 25-26/16: „The jet maximum intensity varies from about 8 to 25 m s and the height of the jet core from 200 to 500 m depending on the night considered.“* You could compare these values against observations to support your argument that NP leads to a better model performance.

- *Lines 13-14/17: „The thermals still accelerates the surface layer as long as the boundary deepens in the morning. As shown by the green curve in the second panel of Fig. 9, this decrease is the consequence of turbulent exchange with the surface. The acceleration by thermals is then smaller because of the reduced vertical gradients in the mixed layer.“* The wind speeds decrease in the afternoon despite the occurrence of thermals. The mixed layer has by definition small vertical gradients in potential temperature which does not explain the wind development. Thermals contribute to the gustiness of the winds and the growth of the daytime boundary layer. The latter helps to mix momentum from higher layers where stronger winds prevail, e.g. a LLJ. The major source for the near-surface momentum is the breakdown of the LLJ during the morning in the cases here (see e.g. Knippertz and Todd, 2012). You could explain the development by incorporating the Richardson number. Once this LLJ momentum has been transported downwards, the near-surface winds decrease.

- *Lines 14-18/18, conclusion point 4:* The results do not support this general conclusion and ignores the worse comparison to observation with NP compared to SP at one of the two stations shown. For instance morning peaks do not agree better with observation at Banizoumbou and the mean near-surface winds at nighttime are still overestimated with NP. Please revise this conclusion.

- *Lines 19-20/18, conclusion point 5:* At the three stations away from dust sources, small differences are found with nudging of 3 and 48 hours. The implications stated are too general as the effect of nudging may change for other models, seasons and geographical locations.

- *Lines 3-8/19:* Even though the winds are better with NP at one station during the morning, these lie away from the emission sources. In order to support that NP is better compared to SP I suggest to extend the discussion of morning winds directly in sources. The current presentation of one grid point for 12 days is not sufficient to support the large implications you assign to the NP for dust emission modelling. The credibility of the conclusions would benefit from a comparison in other seasons and years, which you say you have done but you do not show.

- *Figure 9:* Pick another abbreviation for the turbulent diffusion as TKE typically describes turbulent kinetic energy which is misleading here.

Technical:

- Check singular/plural forms throughout the manuscript.

- *Lines 5-7/3: „Dust is a rather simple tracer of atmospheric motions that sediments into the atmosphere more or less rapidly depending on the size of the grains and can be washed out by rainfall. “* Omit *„rather simple“* and better one of: that can be deposited to the surface/from the atmosphere rapidly

- *Line 9/3: „ dust emissions flux“* replace with: the vertical dust emission to make the sentence clearer

- *Line 26/3: „on the depth“* replace with: over the depth

- *Lines 8-9/4: „of the boundary layer transport, contrast between ...“* replace with: of the boundary layer. The contrast between …

- *Line 26/4:* replace *„raise“* by rise

- *Line 17/6: „ introduced above“* it is actually introduced below

- *Lines 8-9/10: „ by nudging (relaxing) the model meteorology toward observations“* You nudge to re-analysis not observation.

- *Line 24/10: „evalable“* available

- *Line 4/11: „interactif“* interactive

- *Lines 5/16:* omit *„ of the module“*

- *Figure 9:* The labels are too small at the two lower sub-figures and the y-axis of the bottom figure is not sufficient for showing all values of the 925hPa winds.